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SCIENCE PROGRESS

THE X-RAY STUDY OF PROTEINS AND RELATED STRUCTURES

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The study of proteins is not exactly a popular subject, though it ought to be : perhaps it is too important to be popular. It would be too much to claim that our lives depend upon it, but it is no exaggeration to say that the proper control of our lives does ; for there is no life without proteins, and they are life's most important physical constituents.

They are also the most complicated. In some ways they are almost frightening in their complexity—but not so frightening as they were a short time ago, when we could hardly see the wood for the trees. A jungle rather, a tangled mass of facts with only a glimmer of light here and there. The light is not bright even now, but it grows warmer, and there is an exciting feeling of something happening, a feeling that we are approaching a great secret. It is a worthy excitement, and a really great secret, none other than that of our own existence.

As in so many other fields during the last twenty-five years or so, X-rays have played a notable part in bringing us where we now stand in protein studies, and probably they are destined to take us farther still. In a sense, proteins first became " real " in the light of the X-rays : they took on a form, acquired a stereochemistry of their own ; they became something that we could make mental pictures of, build models of, and handle, not with the familiarity of the hydrocarbon chain for instance, but at least with the conviction that they must be " something like that." All that was known of their inner structure with any certainty—and it is a sobering thought that this certainty is still uncertain for some proteins, as we shall see—was that the proteins are somehow polypeptide chain systems : Hofmeister and Fischer established that at the beginning of the

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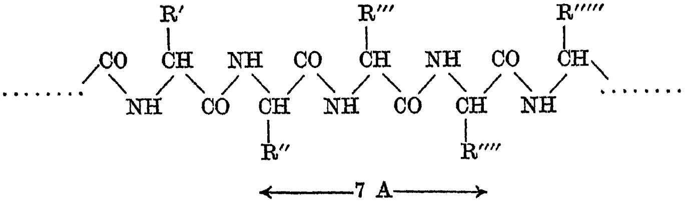
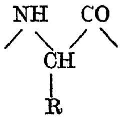
century, and the evidence is still overwhelmingly in favour of it. Now these polypeptide chains have become real things, as real as hydrocarbon chains, or the benzene ring.

Symbolically, a polypeptide chain may be written:



that is to say, it is a sequence of "α-amino-acid residues" of the form:



where R stands for a univalent group such as —H, —CH_3, —CH_2—COOH, —$(CH_2)_4$—NH_2, —CH_2—C_6H_4OH, etc.; twenty-two different kinds have so far been recognised in proteins, some of which are acidic, some basic, some of unexplained function, and some (apparently) non-committal. When proteins are digested, we get back the amino-acids themselves, and these are continually being recondensed in the body to build up other proteins.

Now a polypeptide as just written out is a typical *chain-molecule*: it consists of a long main-chain to which are attached at regular intervals numerous relatively short side-chains; and when the chain is stereochemically fully extended it has the zig-zag form shown and each residue, as first pointed out by Meyer and Mark, should take up a length of about 3½ Angstrom units.[1] Here then is something to look for, something that reminds us of the hydrocarbon chain, and of the cellulose chain, those two splendid examples of the way X-ray analysis can give precision to the ideas of the chemist. The problem of the proteins is infinitely more difficult and varied, but if polypeptide chains exist as such it should be possible to detect them and characterise them to some extent. Straight away, though, we come up against a curious anomaly: the molecules of many proteins are so obviously round, or roundish, that to call them chain-molecules seems a contradiction in terms. Such proteins are egg albumin, hæmoglobin, insulin, pepsin, trypsin, edestin, etc. We may call them "globular" or "corpuscular" proteins. They play active and outstanding parts in metabolism, but we shall leave them aside for the moment and consider some rather more humble proteins —partly because they are more stable and easier to manipulate, but chiefly because they are *fibrous.*

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[1] 1 Angstrom unit (1 A) = 10^{-8} cm.

That is the clue; proteins such as silk, hair, muscle, tendons, etc., are fibrous, and therefore by analogy with other natural fibres should consist of chain-molecules: their structure should be that of a molecular yarn made from polypeptide chains in the way a domestic yarn is made from fibres.

FIBROUS PROTEINS

We can say at once from the X-ray photographs and properties of the macroscopic protein fibres that they consist of long thin units lying in the general direction of the fibre axis: always these photographs and properties have certain features in common, features in fact like those of the vegetable fibres, in which it is accepted that the long thin units are simply regular bundles of chain-molecules. These chain-bundles are the sub-microscopic crystallites that Nägeli called "micelles."

Fig. 1 is an X-ray photograph of a typical fibrous protein structure, the foot retractor muscle of the common mussel, *Mytilus edulis.* The halo arises from the fluid contents, but of the other reflections it should be noticed that the one on the meridian is sharp while that on the equator is diffuse. We nearly always see that sort of thing in protein fibre photographs. It will be recognised as an example in resolving power: there are many repetitions of molecular pattern along the fibre axis but only a few at right angles to it; that is to say, the crystallites are long and thin.

It is a reasonable conclusion, then, that polypeptide chains do exist as such in the obvious protein fibres, but of course we cannot leave it at that; we have to see at least whether the diffraction patterns conform quantitatively with the idea. Actually, they seem not to do so in most cases—the muscle photograph shown in Fig. 1, for instance. That of silk[1] does, however, as Meyer and Mark showed; the crystallites of silk fibroin (as the protein is called) are undoubtedly bundles of *fully extended* polypeptide chains just as written out above. The diffraction pattern repeats along the fibre axis at a distance of $2 \times 3\frac{1}{2}$ A, corresponding to side-chains alternately on one side and the other.

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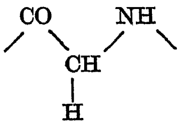
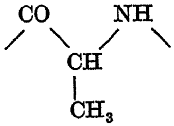
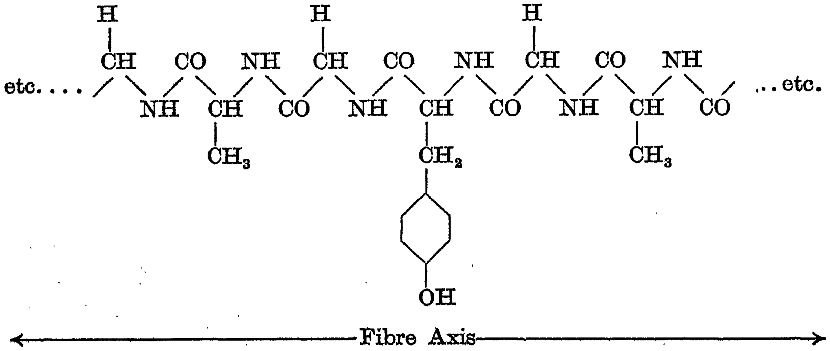
[1] See "The X-ray Interpretation of Fibre Structure" in this Journal, Oct. 1933, p. 210. It may be permitted to refer the reader to the X-ray photographs shown there, so that further photographs may be presented in the present article.

It was originally thought that fibroin consists almost entirely of glycine and alanine residues





in equal proportions, but more recent estimates by Bergmann and Niemann indicate that the correct proportions for most of the residues are: glycine, 1/2; alanine, 1/4; tyrosine, 1/16; arginine, 1/216; lysine, 1/648; histidine, 1/2592. We shall have occasion to refer to these cryptic numbers again later on: they suggest that the fibroin chain is probably something of the form:



We know now a number of proteins that give fibre photographs, or that can be made to give fibre photographs, analogous to that of silk, but at first silk seemed unique, the only example of straightforward polypeptide chains to be found. It was not to be believed that the other fibrous proteins were radically different, but there was something missing in the argument, because their diffraction patterns did not repeat at the right distance. Hair, for instance, which is composed of the protein keratin, was a decided misfit, and so was muscle. The trouble was got over through a study of the elastic properties of hair, which are also quite different from those of silk—and here is a beautiful example of the way two blacks can sometimes make a white! Unlike silk, hair when wet or moist has really amazing elastic properties, second only to those of rubber: it is quite easy to stretch it by 50 per cent. or more in water at ordinary temperatures, and it will go back perfectly when released. And as it is stretched, its X-ray photograph changes into one of another type, into one of the silk type in fact ; and on letting go, the first photograph returns again.

It is clear what is happening ; the polypeptide chains in hair are shaped like those in silk only when they are pulled out straight : in the ordinary way they are in some regularly folded configuration, repeating at a distance that is unfamiliar just because they are folded. The folded configuration is their normal state, and it requires force to pull them out of it and get them into the fully extended configuration we wrote down above—and even then they revert to the folded state when we let go. This is the explanation of the long-range elasticity of hair : it introduces a new principle in vital mechanics. Its implications are immense, whether in stereochemistry or biology, and we could very easily talk about the elastic properties of hair and suchlike things for the rest of the space allotted to this article. As it is we must forget for the moment the " permanent wave ", put away the odd thought that it has been left to X-rays to explain a centuries-old trick for which a woman was once burnt at the stake because it was contrary to the will of God, or the pleasant thought that the romance of beauty can be a romance of science, and remember only for our present purposes that the missing argument is that polypeptide chains need not necessarily be straight ! That seems more or less obvious, but it is probably the main principle in protein structure.

We get substantially the same X-ray photograph from every sort of mammalian hair—human hair, sheep's wool, pig's bristles, or what you will ; and also from the nails and hooves of mammals, from horn, whalebone, hedgehog spines, porcupine quills, etc. All are alike to X-rays in that they are composed of the same kind of protein, keratin : they differ in degree but not in kind. The keratin is always present normally in the folded form (α-keratin), and it always changes into the straight form (β-keratin) on stretching. It is the actual cell substance, for Woods has shown that all the chief X-ray and elastic properties of hairs are inherent in the cells themselves.

These " keratinous growths," then, form a group, not of identical structures to be sure, but of structures that have something very much in common, something that X-rays abstract from the chemical unknown. This something is a molecule, or molecular complex, not of constant constitution, but of constant shape. We must speak not of α- and β-keratin, but rather of the α- and β-keratin *types*. It will suffice here to spare the details and say that according to the X-ray findings the β-keratin type is that of a flat polypeptide " grid ", fully extended main-chains cross-linked by interactions and combinations between their side-chains, while the α-keratin type is that of a buckled or folded grid, the folds lying in planes transverse to the direction of the side-chains (see Fig. 2). The elastic transformation from the α- to the β- configuration is thus simple enough in principle; and whether flat or buckled the grid behaves as a mechanical unit.

It may not be so very remarkable that all the obviously hairy and horny structures of mammals give the same kind of X-ray photograph, but it begins to be intriguing when we find that the epidermis does too, and it is definitely surprising when we get the same thing from muscle! But that is the fact, and it is a very significant fact indeed, for in spite of imperfect analyses we cannot delude ourselves that all these tissues have the same chemical constitution. The X-ray photograph of muscle arises mostly from the fibrous protein myosin, which runs in fibrils along the length

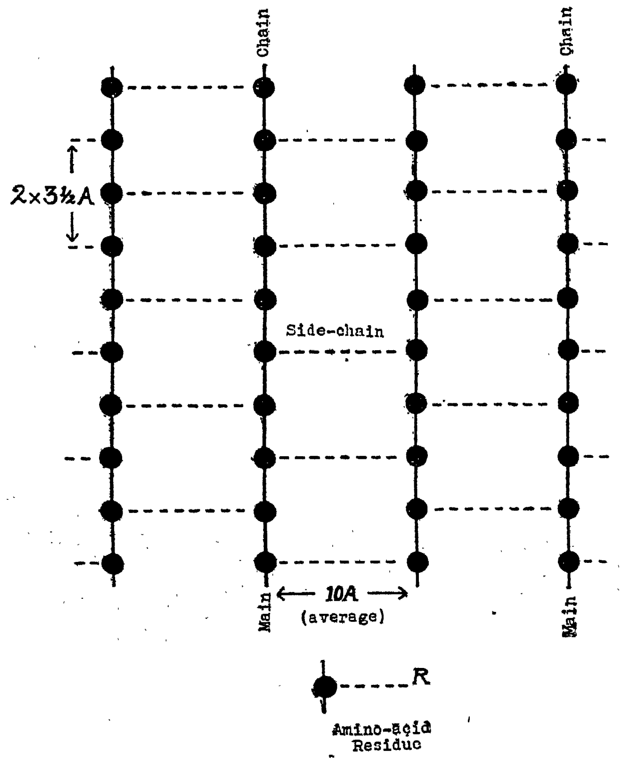


Fig. 2.—A polypeptide "grid" in the β-form (purely diagrammatic). The α-form may be obtained by throwing the paper into a series of folds parallel to the side-chains.

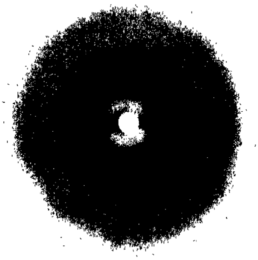


FIG. 1.—X-ray " fibre photograph " of a living muscle (foot retractor of *Mytilus edulis*). Fibre axis vertical.

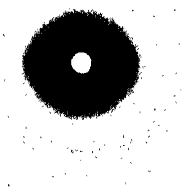


FIG. 3.—X-ray fibre photograph of α-myosin.

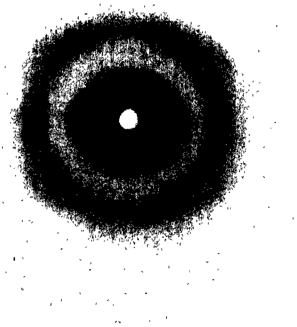


FIG. 4.—X-ray fibre photograph of β-myosin.

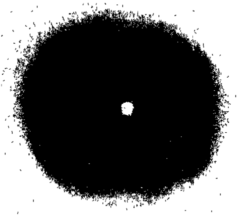


FIG. 5.—X-ray fibre photograph of sea-gull quill.

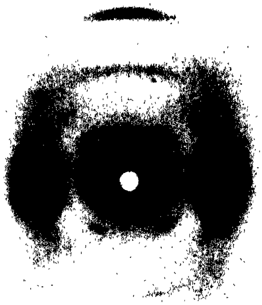


FIG. 6.—X-ray fibre photograph of the tendons of a frog's toes (collagen).

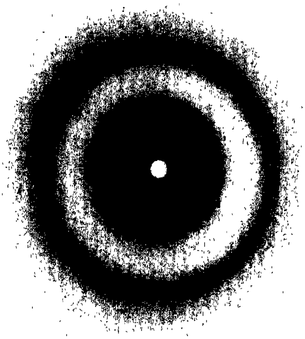


FIG. 7.—X-ray powder photograph of dried boiled egg-white.

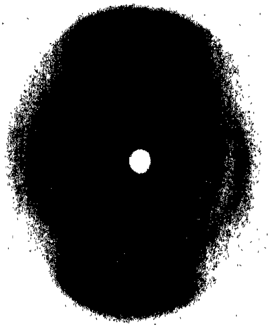


FIG. 8.—X-ray fibre photograph of sodium thymonucleate.

of the muscle and is the seat of its elastic properties. We can extract pure myosin from muscle and make oriented preparations that give, when unstretched, the photograph shown in Fig. 3, or when stretched, that shown in Fig. 4. These two photographs are so like those of α- and β-keratin as to be almost indistinguishable —and there is a whole series of resemblances between the various X-ray photographs and elastic properties of keratin and myosin, especially when the former has been treated in such a way as to break down certain of the cross-linkages of the grid. Keratin in this state can be made to contract [1] just like myosin or muscle, and muscle can be stretched into the β-form just like hair. When we take into consideration all the known experimental evidence from myosin and keratin, set in its proper framework of evidence from the proteins in general, the most satisfying interpretation of the contraction of muscle—our very heart beats depend on it !—is that it is a manifestation of the folding of polypeptide chains. The myosin chains in relaxed muscle are already in the folded α-configuration, but they can be stretched into the straight β-configuration, and when they contract, they do so by virtue of further folding. They are the moving parts of the muscle engine, presumably activated by a cycle of chemical exchanges with smaller accessory molecules : we know no details yet, but it will be appreciated that the state of folding of a polypeptide chain must be a function of the nature, distribution and state of combination of its side-chains.

And so the keratin group of fibrous proteins is much wider than we thought ; it includes also the fibrils of the epidermis and even the myo-fibrils of muscle : we may call it now the keratin-myosin group. In its β-subgroup it includes also the covering of birds and reptiles—feathers, beaks, tortoiseshell, snake skins, and the like. All these give similar X-ray photographs, not of a protein in the α-configuration, but of one in a slightly constricted β-configuration. Fig. 5 is perhaps the best example of its kind : it is an X-ray photograph of the quill of a sea-gull feather. It can hardly be confused with the α-photograph shown in Fig. 3, but it is clear that it bears a strong family resemblance to the β-photograph shown in Fig. 4, in spite of its greater profusion of " spots."

The fact that the feather protein is normally in a slightly contracted β-configuration gives it great strength combined with a certain degree of reversible elasticity—actually up to an extension of about 7 per cent. When we stretch a piece of quill over this range and take X-ray photographs, we see the " spacings " along the fibre axis changing continuously and reversibly in a way that is unique in structure analysis ; and the limit of extension is reached just where we should expect it, namely, when the average length of an amino-acid residue approaches about 3½ A. From the point of view of the molecular biologist a feather is most fascinating. X-rays show too that the great strength of the quill comes in part from its two-ply structure : most of the chain-molecules run along the length of the shaft, but there is an outer layer in which they run round it. And another point worth mentioning again here is this, that the X-ray classification of the keratins into two types characterised by diffraction patterns like Figs. 3 and 5, respectively, agrees with the well-known verdict of palæontology, that the birds and reptiles are more closely related to each other than they are to the mammals. For those who look forward to the day when the boundaries between physics and biology will have vanished entirely, there is something peculiarly happy in this finding. There is a special thrill in demonstrating an evolutionary principle by the methods of molecular physics.

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[1] " Supercontract " is a better word in this connection, when the fibre becomes shorter than its normal unstretched length.

One more group suffices apparently for all the remaining natural protein fibres. We may call this second group the collagen group, and it includes connective tissue, tendons, cartilage, fish skin and fins, jelly fish, the filaments of various shell fish, etc. A typical X-ray photograph is shown in Fig. 6 ; it is that of the tendons of a frog's toes. It will be seen that it resembles neither the α- nor the β-type of the keratin-myosin group ; it corresponds to polypeptide chains in some other configuration. What this configuration is exactly we do not know, though it seems fairly certain that it is stereochemically fully extended, for no one has yet succeeded in transforming the collagen molecule by stretching. The collagen fibres are not ordinarily elastic in themselves, though they make elastic tissues, such as the dermis, by interweaving : indeed, as is well known, tendons for example are extremely strong and have very little " give " at all. Yet under the right conditions they are every bit as elastic as the fibres of the keratin-myosin group. These conditions are realised when we raise the temperature : the collagen fibres then contract spontaneously to sometimes only a quarter of their normal length, and in this state they give an X-ray photograph suggesting that the polypeptide chains have coiled up. Their newfound elasticity is therefore not really surprising when we remember the lesson of the keratin-myosin fibres : they stretch—and X-rays confirm this—by uncoiling the chains again, and so long as the temperature is high enough, they go back to the coiled state when we let go. The contractile mechanism is thermal agitation.

There is one member of the collagen group whose real character is masked: it is elastin, the highly elastic fibre that is perhaps most familiar in the "ligamentum nuchæ."[1] Unlike tendon it stretches to great lengths, and moreover it is only when stretched that it gives the usual collagen photograph. The inference is that its chain-molecules are coiled up even at ordinary temperatures; in other words, its thermal transformation temperature is abnormally low. But it is a true member of the collagen group—and so is gelatin. Gelatin is an artificial degradation product of the natural collagen fibres, but it still gives an X-ray photograph like Fig. 6.

That is a strange thing—it is remarkable that *all* the collagen fibres give the same type of X-ray photograph, for they undoubtedly vary considerably in composition. It is the case of the keratin-myosin group over again—one molecular configuration suffices for all. How can that be? What does it mean that so few configurations account for the whole of the natural protein fibres? It must mean that there are divisions of protein chemistry something like the aromatic field in ordinary organic chemistry. We know now that the aromatic compounds are based on the carbon hexagon, but if we did not know that and had only empirical formulæ at our disposal, we should be no better off than we are now with the proteins: the simplest derivatives of benzene, of identical configuration, might seem to bear absolutely no relation to one another. In one respect we are much worse off with the proteins than with an aromatic chemistry of purely empirical formulæ, because the protein molecules are so enormous, their chemical analysis is so difficult, and the available data are so distressingly incomplete—yet even so, this new principle brought to light by X-rays, this idea of only a few configurations sufficing for many compounds of different chemical constitution, gives hope that the problem is not insuperable. If only we can find out precisely how the fibrous proteins are folded, we shall then have a guide like benzene. Already there is a strong suggestion that protein chains, too, fall into hexagonal rings, for we can explain the X-ray photographs and elastic properties of keratin and myosin very plausibly indeed on such a basis.[2]

CORPUSCULAR PROTEINS

We may now turn to the "globular" or "corpuscular" proteins. Our minds turn to them almost inevitably when we think of limited numbers of special configurations. There is nothing more surprising in what we have just said about the fibrous proteins than what has been known for several years with regard to the corpuscular proteins. The epoch-making experiments of Svedberg and his collaborators with the ultra-centrifuge have revealed that the corpuscular proteins in solution have molecular weights that fall into groups, groups that are multiples of about 17,600. For example, egg albumin, insulin, pepsin, zein, Bence Jones protein, etc., all have molecular weights of roughly twice this number, while for hæmoglobin and serum albumin it is four times, for edestin, excelsin, and other seed globulins sixteen times, and so on, up to weights of millions. Furthermore, molecules of higher weight can be split into units of submultiple weight. There is obviously some common factor or factors running through the whole series, some way of building them up, some trick of molecular architecture that suffices for the lot. Granted this, we can probably explain the fact that the molecular weights in any one group are not strictly constant by the circumstance that the chemical constitution also is not constant—it varies, of course, from protein to protein: the same number of linked amino-acid residues might conceivably always take up the same configuration, but the total weight would vary, though never a great deal. Anyhow, be that as it may, it is a sound enough conclusion that the corpuscular proteins are not based on all sorts of molecular configurations, but on relatively few. Thus all proteins are alike in this respect: there is nothing unexpected in the X-ray classification of the fibrous proteins.

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[1] A ligament in the human neck.

[2] See "The X-ray Interpretation of Fibre Structure," *op. cit.*, p. 223.

The story of the X-ray attack on the corpuscular proteins is only at its beginning. Though many of them form beautiful crystals, they are exceptionally difficult subjects for X-ray examination, partly because the crystals are so small, partly because they are generally very unstable, and partly because they diffract X-rays but feebly. These three troubles together for a long time proved more than a match for structure analysts, but fortunately it is possible to report progress at last—not so much as we should like, it is true, but still a very definite advance. Bernal, Crowfoot, and their collaborators, for instance, have succeeded in obtaining excellent photographs of pepsin, insulin, lactoglobulin, chymotrypsin, hæmoglobin, etc., and have deduced therefrom the respective units of pattern and probable molecular weights. The latter agree well with the values given by the ultra-centrifuge—but there the story more or less ends for the moment. Attempts have been made, and are still being made, to elucidate the *intra*-molecular structure from the X-ray photographs, but with no great success so far. Certain rather tentative conclusions have been drawn, but on the whole it is fair to say that the problem remains unsolved by this method.

There is another X-ray method of approach, however, and that is through the phenomenon of "denaturation." Denaturation is the sad end, in one easy way or another, of almost all corpuscular proteins—the word is self-explanatory; they lose their specific properties, become insoluble and useless for the special purposes for which they were intended. Many proteins denature simply on drying (hence one of the difficulties in X-ray analysis), and heat is almost infallibly disastrous. A boiled egg is denatured protein—still good, even better, to eat since we only need the amino-acids, but quite beyond all hopes of raising a chicken from it. Speaking generally, it is depressingly easy to denature corpuscular proteins: their structures are so delicately balanced that they "go wrong" on the slightest provocation. And yet when this happens, there is never any great change in chemical constitution, only in internal arrangement. The correct amino-acid residues are still there, and in their correct proportions, but everything is spoiled somehow.

Fig. 7 is actually an X-ray photograph of a piece of dried boiled egg-white, but without careful examination it might equally well pass as a photograph of boiled muscle, or boiled serum albumin, or numerous other heated proteins—or even as a bundle of stretched hair! Denatured proteins are all alike in this, that they are like disoriented stretched hair: they have all gone the same way and ended up like β-keratin. There can be only one answer to such a result, that denaturation is the breakdown of a specific configuration with the formation or liberation of polypeptide chains. It is impossible to say which; we still do not know for certain that polypeptide chains pre-exist in the corpuscular molecule and they might very well be formed at the moment of denaturation, but taking everything into consideration it seems much more reasonable to conclude that they are there all the time and that *they simply unfold*—perhaps in some cases to make longer chains by secondary condensation or ring-chain polymerisation. The corpuscular proteins from this point of view are the three-dimensional counterparts of the folded fibrous proteins; and for any given molecule unless the right polypeptide chains are folded in just the right way, its properties are not specific.

In general, the breakdown of a specific configuration must lead to all sorts of irregular arrangements of polypeptide chains, and among them there is bound to be a proportion of fully extended chain-bundles analogous to the crystallites of β-keratin: it is these chain-bundles that give rise to the disoriented β-photograph shown in Fig. 7. Theoretically it should be possible to pull out the chains of a denatured corpuscular protein into the form of a *fibrous* protein. That is a direct deduction from the X-ray data that has now been verified experimentally. Edestin is a seed globulin, a crystalline corpuscular protein of molecular weight about 309,000, and it dissolves in dilute salt solutions to form an ordinary thin sol; but if excess of urea is added the solution becomes very viscous, and it can then be "spun" into fibres in the way rayon is spun from viscose or cuprammonium solution: all that is required is to squirt the urea solution through a capillary tube into water. Urea is a well-known denaturing agent, and when it is added to solutions of native[1] proteins the rise in viscosity is a most striking indication that the originally round molecules have become very much elongated. The X-ray findings explain this at once by the concept of unfolding.

On diluting the urea solution by squirting into water, the liberated polypeptide chains are precipitated as elastic fibres, and these fibres when stretched give not merely a disoriented β-photograph like Fig. 7, but a true "fibre photograph" of the type of Fig. 4. The experiment is as convincing a demonstration of the soundness of the X-ray interpretation of protein structure as it is possible to devise. It reduces the stereochemistry of proteins to its simplest terms and, incidentally, provides the theory of a new textile industry.[2]

Still another way of catching a glimpse of the interior of corpuscular proteins is by studying the monolayers they form on aqueous substrates—and here again X-rays have made a decisive contribution. The round molecules spread not merely themselves, *but their contents*, over the surface of the substrate, for the resulting layer is definitely thinner than their original diameter; and what is more, under the correct conditions it is always about the same weight per unit area, namely, about 1 mgm. per sq. metre, whatever the original molecular weight. Indirect estimates suggest a thickness of the order of 10 A, and the natural inference is that we are dealing now with more or less free polypeptide chains—polypeptide chains lying with their side-chains dipping into the water. From the X-ray data given by protein fibres we can calculate very easily what the thickness and weight per unit area should be, and they agree remarkably well with experiment.

There was no out-and-out proof, though, till lately that protein monolayers really consist of polypeptide chains. Now Blodgett and Langmuir have shown how to build up protein *multilayers* by repeatedly dipping a chromium-plated metal slide into a substrate covered with a monolayer: every time the slide passes vertically up or down through the surface it picks up a portion of the monolayer, and by continuing the process we can build up within reason multilayers of any thickness.

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[1] Undenatured proteins are also often called "native" proteins, though some native proteins are already in the fibrous configuration associated with denaturation.

[2] "Lanital," for instance, the Italian "artificial wool," is denatured casein spun into fibres.

At any rate, by means of a special dust-proof automatic apparatus working on the Blodgett-Langmuir principle, multilayers have recently been prepared consisting of up to 1764 monolayers of egg albumin, and these have been stripped from their metal base and examined mechanically, optically, and by X-rays. The mechanical and optical properties indicate that the films are *fibrous* in a direction corresponding to that in which the slide was moved through the surface of the substrate, while X-ray examination reveals directly that they are composed of polypeptide chains lying roughly in that direction, with their side-chains roughly perpendicular to the surface. If now these polypeptide chains had been liberated or generated after the protein had been deposited and dried on the metal slide, we know that the result would have been a disoriented X-ray photograph; but as it is, we may conclude quite definitely that they must have pre-existed in the original monolayer, and in such a free state that the movement of the slide was sufficient to orient them—to spin off a polypeptide ribbon, so to speak, in very much the way fibres may be spun from denatured edestin as described above, or in fact as rayon is spun every day in the textile trade.

The side-chain "spacing" (see Fig. 2) given by X-ray photographs of egg albumin multilayers is about 9½ A, just as with macroscopic protein fibres, whether natural or spun from denatured material, and this number must therefore represent also the approximate thickness per monolayer. To prove the point we have to measure the multilayer thickness by other means. This was accomplished in two ways. The first was by inserting the film under one of the feet of a small three-legged interferometer specially constructed for the purpose, thus altering the angle of an air wedge included between two pieces of optically flat glass. The instrument was so designed as to minimise all pressure on the film, and the measurement consisted in counting with a travelling microscope the number of interference bands first without and then with the film under the foot. It should be noticed that no optical properties of the film itself are invoked—the measurement is direct and the calculation involves only the wave-length of the light used and the geometrical constants of the interferometer. The method proved successful even for multilayers composed of only 600 monolayers, *i.e.* only about 0·6 μ thick, and in a series of films of increasing thickness the thickness per monolayer was found to be always about 9½ A, in excellent agreement with the side-chain spacing given by the X-ray photographs.

The second method of direct measurement was by means of a screw micrometer—and this is the first time that the thickness of a protein chain, or perhaps of any molecule, has been measured with a common workshop tool. A single film was still too thin for the purpose, but ribbons of film were folded a known number of times so as to build up comparatively thick pads, and these gave significant readings in the micrometer. The accuracy of measurement was admittedly not high, but it was sufficiently good to point once more to a thickness per monolayer of about 10 A.

And so in effect the formation of a protein monolayer is just another example of denaturation: the monolayer is a fibrous polypeptide system no different in fundamentals from other fibrous proteins, natural or artificial; and as for the actual mechanism of formation, we can hardly doubt that it is one of simple unfolding under the influence of surface forces. The thread of the protein story is thus always the same, always one of polypeptide chains, their states of combination, *and their states of folding.*

But what are these states of folding? We still cannot say; this further glimpse is still too fleeting. Immunology, the ultra-centrifuge, crystallography, X-rays—witness the beautiful feather photograph—all proclaim the precision and regularity of protein structure—how impressive is such perfection in these giants among molecules!—but the key to the puzzle is yet to be found. We have no *detailed* theory that rigidly stands the test of experiment, in spite of the fact that so many things, the X-ray data in particular, point to the conclusion that the transition from the corpuscular to the denatured form must be remarkably easy, that no kind of cataclysmal change need be considered. But surely all proteins must be fundamentally fibrous, and the structure of α-keratin and α-myosin—polypeptide chains cross-linked through their side-chains and folded in planes transverse to their side-chains—must be a prototype for all. And surely, too, the giant molecules are not built up "all in one go"; they are multiples of similar or related sub-units of perhaps only a few dozen residues each at the most: their perfection of detail is never achieved in one single heroic stroke, but by piecing together already perfected parts of a more familiar nature. It is this "more familiar nature" that it is urgently necessary to recognise first.

There was a time, not very long ago, when chemistry could hardly subscribe to the idea that proteins are normal molecules constructed to stoichiometric proportions : the analytical data were chaotic, even one and the same protein never giving the same answer twice. The technique of protein analysis is supremely difficult, it is true, yet all the same there seemed to be neither rhyme nor reason in the proportions of the products of hydrolysis. The most that could be said was that from time to time there turned up ratios involving approximate twos and threes. But now, thanks largely to the pioneer work of Vickery and Block and the intensive, systematic investigations of Bergmann and Niemann and their collaborators, things are beginning to look different, and twos and threes—still twos and threes—are beginning to crowd upon us. Indeed, the conclusion that Bergmann and Niemann have arrived at is that not only the total number of residues in any protein, but also the numbers of each of the different kinds of residues, are expressible in the form 2^n3^m ; that is to say, the number of residues of any one kind is always 1/2, 1/3, 1/4, 1/6, 1/8, 1/9, 1/12, etc., of the total number of residues in the molecule. For example, in silk fibroin quoted above the different kinds of residues are present in the proportions : glycine, $1/2^1$; alanine, $1/2^2$; tyrosine, $1/2^4$; arginine, $1/2^3.3^3$; lysine, $1/2^3.3^4$; histidine, $1/2^5.3^4$. From such fractions the minimum number of residues required to make up a molecule, and therefore (provided we know the average residue weight) the minimum molecular weight, follow at once—and it is a most encouraging sign that the molecular weights so far deduced in this way agree quite well with values obtained by other methods, such as the ultracentrifuge. If the Bergmann-Niemann scheme is sound, it means that each of the proteins in Svedberg's egg albumin group (see above) is probably constructed from 288 residues, those in the hæmoglobin group from 576 residues, and so on.

The X-ray method, by independent reasoning, leads also to a similar conclusion. It would take too long here to develop the argument adequately, but it can be shown that in keratin, for instance, the principal residues are present in the following proportions : glutamic acid, $1/2^3$; arginine, $1/2^4$; aspartic acid, $1/2^4$; tyrosine, $1/2^5$; lysine, $1/2^4.3^1$; tryptophane, $1/2^5.3^1$; histidine, $1/2^6.3^1$. From this new analytical point of view, therefore, the distinction between fibrous and corpuscular proteins becomes more unreal than ever. Already it was clear from the X-ray interpretation that the two types correspond only to different configurations of straight or folded polypeptide chains, and now it transpires also that all proteins are related stoichiometrically : *they are synthesised according to a common plan.*

The problem of the structure of proteins is the problem of the common factor in their mode of synthesis. This factor must be responsible both for their multiple molecular weights and for the common configurations of proteins of different constitution. We have no idea what it is, beyond the feeling that it is less likely to be something inherent in the proteins themselves, because of this seeming independence of constitution—and that lays the burden once more on enzymes, which leaves us, if anything, worse off than before, since enzymes themselves are also generally proteins ! The truth of the matter is that we are suffering from lack of data : we are in that thoroughly unsatisfactory state where we may suppose anything or nothing, and only facts—many more facts than we are aware of now—will lift us out of it. Probably our besetting weakness is trying to explain too much in terms of the proteins alone. Granted that in the physical scheme of life they are pre-eminent—and who can doubt it ?—that still is not the same thing as saying that they are molecularly omnipotent. They must evolve and work in co-operation with other molecules, simpler molecules maybe, but indispensable partners whose structures and properties it is every bit as important to understand. Such molecules, it is becoming increasingly evident, are the polysaccharides for instance, and above all, the nucleic acids.

NUCLEO-PROTEINS

Two kinds of structure that lie at the very source of life are the chromosomes and the viruses, and they have two main characteristics in common, their power of reproducing themselves and an organisation based on the association of proteins with nucleic acids. The chromosomes, as everyone knows now, are the components of the cell nucleus : they are the controlling elements in cell division and growth and the bearers of the factors of heredity. It has been known for many years that the cell nucleus contains relatively large amounts of nucleic acid—the name speaks for itself—but it is only during the last few years that it has been recognised that the viruses, too, are nucleo-proteins. The most thrilling biological discovery of this century was undoubtedly the first isolation of a pure virus—in point of fact, the tobacco mosaic virus—by Stanley : it turned out to be no other than a crystalline compound of protein with nucleic acid. A dozen or more other viruses have been isolated since and they have all turned out to be nucleo-proteins too : they are apparently enormous molecules that have the property, in the correct environment, of producing other molecules exactly like themselves. In this sense they are " living " : otherwise they are just molecules !

Before considering these things further we must delay for a moment to look closer at the nucleic acids. In complexity they are inferior only to the proteins themselves. They are made up of " nucleotides," and a nucleotide is a fairly large double-ring structure, one half of which is a purine or pyrimidine base and the other half a sugar with phosphoric acid tacked on. We need not go into the finer points of the various nucleotides ; it will be sufficient for our purpose to describe them in this general sort of way and to add that when they are linked together to form a nucleic acid, the linkage is believed to be through the phosphoric acid molecules. The kind of nucleic acid present in the chromosomes is thymo-nucleic acid, and Caspersson has shown that it is located in the dark bands and that it goes through a cyclic rise and fall corresponding to the cycle of cell division.

Sodium thymonucleate is a fibrous material that gives in suitable preparations X-ray photographs like Fig. 8, in which the principal repetition of pattern along the fibre axis occurs at 3·34 A. Its aqueous solutions are very viscous, and they show optically negative " streaming double refraction " (the fibres are also optically negative with respect to their length) and other properties pointing to long rod-like particles of molecular weight of the order of 500,000 to 1,000,000. The X-ray view of these particles is that they are columns of flat nucleotides spaced at 3·34 A—something like piles of plates, about two thousand high, each plate representing a nucleotide. Their most intriguing feature, however, is this, that the spacing of 3·34 A between successive nucleotides is almost exactly the spacing between successive side-chains in a fully extended polypeptide. It is tempting to infer therefore, in view of Caspersson's work just cited, that the agreement is no mere numerical coincidence signifying nothing in particular, but is a matching of intra-molecular patterns that Nature has adapted specially to the processes of chromosome division. During mitosis and meiosis the chromosomes pass through a cycle of length changes, and we can imagine that some critical moment when the proteins are fully extended is decided under the influence of just this period of 3·34 A between the nucleotides of thymonucleic acid.

We are a long way from either verifying or disproving such an idea, of course, but at least we can show by X-rays that it is stereochemically sound. One of the chief proteins found in sperm is clupein, a simple polypeptide of about 28 residues, 21 of which are arginine residues. The arginine side-chains are basic, and so the clupein chain should combine very easily with the phosphoric acid groups down the side of the thymonucleic acid column. And so it does, to form fibres of clupein thymonucleate that give X-ray photographs hardly different from Fig. 8: the principal period along the fibre axis remains unchanged at 3·34 A. These fibres are optically negative with respect to their length, just as the giant salivary chromosomes are in those regions where the nucleic acid is found; which means that if the protein-nucleic acid compounds in the chromosomes resemble the synthetic material, *the polypeptide chains must run along the length of the chromosomes.* That is something that had often been assumed before, but never demonstrated experimentally: it is a conclusion that means much to genetics. The "genes"—the loci of the hereditary factors—form a linear sequence along the length of the chromosomes, a chain that may be broken, re-united, and have its parts transposed and interchanged—and the chromosomes as a whole have the essential physico-chemical character of fibres. What more promising molecular basis, then, could we find for such properties than a system of polypeptide chains running lengthways? Again it is much too early to be sure about anything in chromosome structure, but here at least is a possibility to think about and work upon, a definite experimental start. The proteins are the most important molecules in life: it may be that the patterns of life are only the patterns along polypeptide chains.

The tobacco mosaic and related viruses have been examined by X-rays by Wyckoff and Bernal and their collaborators, particularly the latter group, who have shown that the particles are rod-like bodies about 150 A thick. They are readily oriented on drying or by streaming in solution, but *whether dry or in solution* they always give the same X-ray pattern. The particles are molecules almost certainly, but that is a beggarly description: they are molecules of such grand size and regularity of structure that they are crystals too. Though their molecular weight is many millions, the X-ray photographs show that they are constructed of sub-units weighing no more than a few thousands. Along the length of the rods the true intramolecular pattern repeats at about 68 A, but there are indications that the actual sub-units are of approximate dimensions 20 × 20 × 22 A, or possibly as small as 10 × 10 × 11 A.

What is the secret of the virus structure? Still the answer is that we do not know yet—but it is an exciting question with which to conclude an article of this kind. The whole subject is exciting at the moment, and rightly so, considering the issues at stake. Already the protein and nucleic acid data are beginning to run together, and it is possible to define at least the chemical sub-unit of the tobacco mosaic virus. It is the X-ray sub-unit, just quoted, of volume 20 × 20 × 22 A. It contains the substance of about

54 amino-acid residues *and just one nucleotide.* This then is the building-stone of the simplest " living " thing.

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MODERN WORK ON EARWIGS

By MALCOLM BURR, D.Sc., F.R.Ent.S.

I. Their Ancestors

The Dermaptera, or Earwigs, in spite of several highly specialised features, are an archaic and generalised type of insect.

The earliest known fossil with any claim to be regarded as an earwig is from the Lower Permian of Kansas, where have been preserved wings with the characteristic folding. On the strength of these Tillyard formed a distinct order of insects, the *Elytroptera*, which he considered ancestral to the Dermaptera. Not all, however, accept this judgment.

For the next step we must go to Asia. In shales of Jurassic age in Kazakstan two kinds of fossil have been found which have reasonable claim to be regarded as the remains of earwigs. No living form seems to represent the line of one, *Semenoviola*, which is no doubt extinct. The other is well named *Paradiplatys fortis*, as it is in many ways reminiscent of the primitive genus *Diplatys*. The fact that the cerci are segmented is no serious objection, as segmented cerci survive in larvæ of *Diplatyinæ* and *Karschiellinæ*. A more serious objection is in the five-segmented tarsi. This, however, is not insuperable, as the ancestral forms of the related Orthoptera were pentamerous. Moreover, the number of tarsal segments is not immutable, as different numbers occur in related groups and instances of hypotypy are by no means rare. There are several other ancestral features, but only one, the apparent venation of the elytra, has completely disappeared in all recent forms.

Remains from Mesozoic beds claimed as those of earwigs must be accepted with caution, but we meet with unquestioned Dermaptera in the early Tertiary.

Many years ago Scudder described about a dozen species, for which he created the genus *Labiduromma*, from lacustrine shales of early Tertiary age from Florissant, in Colorado; and from Baltic amber, reputed to be of Lower Oligocene age, several species are known. These are of modern appearance, and Bey-Bienko has pointed out the resemblance of two to several existing species.

II. Geographical Distribution

There are something more than 900 species of earwigs known to science, the great majority of which are confined to the tropics. No less than 400 are recorded from the Oriental Region alone. Many more must remain to be discovered in Africa and especially in South America. The numbers drop rapidly towards the temperate zones. In the Palæarctic Region there are known about 90 species and in the Nearctic there are no indigenous earwigs at all. It is interesting to compare the 9 species of Russia with 45 in the Mediterranean and 75 in the Belgian Congo.

Earwigs are hygrophilous insects, loving a warm, and above all, moist atmosphere ; aridity is fatal to them. So it seems likely that the cause of the extinction of *Labiduromma* was the desiccation of the Western United States, and no doubt the glaciation exterminated any other Dermaptera that may have lived in other parts of North America. That in pre-glacial days it shared the forficuline fauna of the now temperate Old World is indicated by the curious survival in the mountains of Mexico of *Anechura vara* Scudd., an isolated species of a genus characteristic of high altitudes in Central Asia and Europe. Presumably the genus was once holarctic in distribution and this single species survived in the New World in the south, having migrated along the higher crests. The only other earwigs known to-day in North America are either immigrants from the Neotropical Region or naturalised aliens. It is curious that the almost cosmopolitan *Labidura riparia* Pall. appears to be unknown on the west coast.

In the Old World we find in the temperate belt two dominant genera, both high in the scale of development and closely related to each other, *Forficula* and *Anechura*. The former, associated with woodlands, contains about 50 species, three-quarters of which are found only in the Palæarctic Region. The latter contains 18 species, almost confined to the region, with the Mexican exception alluded to.

In the West we have respectively 14 and 6 species of these two genera, while in the East there are 12 of *Anechura* and 20 of *Forficula* so far known. In the centre there are but two of each.

This striking discontinuous distribution, of which analogous cases are known to most zoologists, is explained by the extermination of the ancient fauna of the centre owing to the drying-up of Asia consequent upon the elevation of the mountains to the south, which curtained off oceanic moisture.

The survivors in the West were then drastically affected by the

Pleistocene glaciation, but in the Far East the ancient fauna has to a great extent survived under favourable conditions. The varied relief of the mountains and ravines of Szechuan, lower Han Su and Eastern Tibet, as Semenov-Tian-Shansky has pointed out, afford us a living picture of the ancient fauna and flora of Miocene days, when the moist, warm forest extended from the Pacific to the Atlantic. The occurrence of the panda, *Ailurus*, in Eastern Tibet and the Red Crag of Norfolk is analogous to the close resemblance between the members of the genus *Forficula* in China and Western Europe. Both phenomena are due to the same cause.

Semenov-Tian-Shansky and Bey-Bienko have pointed out the close relationship between the Western and Eastern species. In many instances they occur in corresponding pairs. Thus, our common Western *Forficula auricularia* L., which extends no farther east than Kopet Dagh in Transcaucasia, is represented in the Far East by three closely related species, *F. vicaria* Sem., *F. burriana* Sem., and *F. subauricularia* Sem. Similarly, the Levantine *F. smyrnensis* Serv. is paired with the Eastern *F. davidi* Burr; the Russian *F. tomis* Kol. with the Japanese *F. robusta* Sem. and the Chinese *F. macrobasis* B.-B.

The genus *Anechura* underwent a somewhat similar fate. The elevation of the mountains led to the evolution of species by isolation. The Italian *A. orsinii* Géné pairs with the Chinese *A. svenhedini* B.-B., and the Pontic subgenus *Borelliola* with the Tibetan *Burriola*. In this genus we can detect character-gradients or clines.

It is significant that two of the most primitive of recent members of the genus *Forficula*, namely *F. davidi* Burr and *F. schlagintweiti* Burr, occur in China and the Himalayas respectively. As far back as 1910 I pointed out certain Anechurine features in the latter plastic species, which Bey-Bienko now regards with *F. davidi* as representing the ancestral line of the two genera. He has also called attention to the resemblance of *F. schlagintweiti* to the Oligocene *F. præcursor* Burr, from Baltic amber.

To come nearer home, it is interesting to ask whence we received that abundant insect, the Common Earwig, *Forficula auricularia* L. I am strongly of opinion that it is a survivor of that ancient, rich fauna of Miocene days, which ranged from ocean to ocean. It does not dread the cold. It conducts its domestic affairs in Central Europe during the winter. It ranges as far north as latitude 60°. Its close relative, *F. tomis* Kol., can stand even the Russian winter as far north as Vitebsk and Omsk, where it hibernates in the larval stage. It is rather the humidity than the cold that was fatal to so much life in the days of the glaciation, and against that earwigs are protected by their secluded life and hygrophilous disposition. Eloquent evidence in support of my view is the identification by Lesne of the pronotum of a common earwig from the Pliocene of Castle Eden, in co. Durham. Dr. Zeuner will not accept this view. He considers that the species may certainly have lived with us in those days, but that it could not have survived the glaciation and that the ancestors of our present earwigs came over from the Continent during that relatively short period between the retreat of the glaciers and our separation from the mainland. But if it is correct that the hippopotamus survived in the valley of the Rhine well into the Pleistocene and in England into the Middle, and even Upper Pleistocene, lingering in our oceanic climate for some 200,000 years after its disappearance from Germany (*v.* article in *Nature*, June 20, 1936, p. 1041), why should not our damp-loving, cold-resisting earwig have survived down to our own day ?

Another somewhat obscure point in the geographical distribution of earwigs is worth bringing into prominence. There is a small but very sharply defined subfamily called *Esphalmeninæ*. The shape of the sternal plates and the peculiar form of the male genital armature separate it very clearly from all other groups. There are about half a dozen species, all apterous, well flattened, with strongly dilated abdomen, and all dark in colour, features characteristic of high altitudes. It is not surprising, therefore, that the single genus occurs in the Andes, from Patagonia to Ecuador, but it is surprising that one species is known only from the Caledon River in Cape Colony, where it was discovered by the late L. Péringuey many years ago. This remarkable instance of discontinuous distribution is, I think, the chief contribution of the Dermaptera in support of the hypothesis of continental drift.

III. THE SENSES OF EARWIGS

As earwigs are lovers of the dark, it is not surprising to find that they draw their perception of the world rather from touch than from sight.

Their eyes are of the type termed acone. That is, instead of the optical elements called cones occurring in most faceted insect eyes, there is merely a group of elongate transparent cells. This somewhat primitive type of eye is found also in the *Tipulidæ* or Daddy-Long-Legs and in some Bugs and Beetles. The long "crystal cells" cannot form an image, and we can think only that they have a concentrating and directing effect upon the pencil of rays received from the corneal lens. The picture must be formed by the massing of such specks of light, in a manner analogous to that of the half-tone plate. It is therefore hardly surprising that Weyrauch found poor reaction to optical stimuli. The range of clear vision in such an eye, in his opinion, is not more than 1 centimetre. Earwigs are negatively phototropic and react more quickly to all stimuli in the dark than in a bright light.

The dominant stimulus is tactile, for it takes but the slightest unevenness of surface on which the insect is standing to enable it to orientate itself. The antennæ are particularly sensitive to such stimuli. These are in a state of almost perpetual motion, testing the environment during movement and even when at rest in a crevice the insect maintains contact with the walls around it by means of its antennæ. If one be amputated, the function is carried out by the other. The behaviour of the antennæ can be easily seen in a specimen in a glass tube of small diameter, when the earwig will maintain contact with the walls of the tube by means of its antennæ. If placed in a vessel with curved walls, the earwig will crouch as close as possible to the wall, its body bending to fit the curve. If then disturbed, instead of crawling or flying away, it will cling all the more closely to the wall. This reaction is very characteristic of the earwigs, which always seek to maintain contact, preferably dorsal, with some solid, fixed object. This accounts for their preference to narrow crevices. Earwigs are in fact thigmotactic and such a reaction can be stronger even than the negative phototropism, marked though that be in these insects. There is certainly connection between this thigmotropism and their gregarious habits. In everyday language, we should say that they derive a feeling of comfort from the contact of their kind.

Earwigs respond also to various chemical stimuli, as can be seen in their discrimination between a sound egg and a rotten one (*v. inf.*, p. 29). The seat of perception of such stimuli is no doubt in the antennæ and palpi, which are studded with minute sense organs.

Weyrauch found earwigs a suitable subject for experiments in hypnosis. They have insensitive callipers by which they can be handled with a minimum of disturbance; they are abundant, and in nature not much exposed to such stimuli as be applied.

He found that by the excitement of a fixed point in the central nervous system there was formed, as a counter-reaction against further excitement of the ganglion cells concerned, a localised stage of fatigue, an inhibition. This inhibition radiates from the point of origin and involves other regions of the central nervous system in sympathy. Such a far-reaching inhibition, involving akinesis, he terms hypnosis. Weyrauch points out that in this conclusion he agrees with Mangold in Jena, with Robimarga in Italy who worked with birds and mammals, with Ten Cate in Holland who worked with *Raja clavata*, the thornback skate, and with Pavlov in Russia who worked with dogs.

Starting from the fact that the earwig is most sensitive to tactile stimuli, he held one by the callipers and gently pulled it backwards for a distance of about 20 cm., across a sheet of paper and then lifted it suddenly into the air. The insect put up a resistance by clawing that was appreciable. The effect of lifting it was instant immobility. Another method is to hold down the head and antennæ gently with the finger. This activates the defence reflex, and the insect raises its tail backwards, but as the pinch reflex cannot be satisfied, the abdomen drops and remains immobile. He found six other methods by which he could induce akinesis. The difficulty is to avoid shock to the nervous system and such counter-irritations as exposure to draught, shaking, or contact where not actually desired, all of which spoil the effect.

He observed a transitional stage before immobility was reached. The duration depends on the method employed. Naturally, the deeper and longer the hypnosis, the slower the recovery. He modified the first method by waxing the tarsi, so that the claws slipped over the paper. Like this, mere contact without resistence was enough to induce the condition, though not so effectively.

Weyrauch found no difference in the reaction of the sexes and that none was refractory. The condition of akinesis lasts for a period varying from a few seconds to half an hour. His record was 1 hour 27 minutes 23 seconds.

IV. Variation in the Length of the Forceps

The marked variation in the length of the callipers of the male earwig has long attracted the attention of biologists. In 1892 Bateson and Brindley found evidence of a partially discontinuous variation in this secondary sexual character. They found that the male earwigs fall into two well-marked groups, those with short forceps and those with long. The shortest measured 2·5 mm., the longest 9 mm. Plotted to the millimetre, these were found to be grouped around 3·5 mm. and 7 mm., thus forming two peaks, a bimodal polygon. This conclusion was accepted without hesitation, and the terms "high" and "low" males passed into current phraseology in the literature of the order.

But these results have been challenged by Willi Kuhl, who criticised the methods of the English observers. They measured 583 males, to approximately 0·5 mm. He handled 11,630 specimens, and measured to tenths of millimetres: they measured the length only; he measured ten dimensions and ratios: they handled only males; he handled both sexes and also a large number of malformed specimens: they plotted one graph only; he plotted dozens, and also made models in the solid. His main criticism was against their method of collection, which resulted only in a thousand specimens, from which they selected the males. He maintains that the ideal material to measure is the entire population of the species in a given spot, in two or more seasons. Only then are the proportions true.

He approached this ideal in his collecting ground in Heligoland and somewhat less closely at Frankfurt am Main. At a locality in Holstein conditions were less favourable, and the relatively small collection from there was the only one which gave a sign of a second peak. He also collected in two successive seasons.

From this thorough method he drew definite conclusions. So far from there being very few specimens of intermediate length, as was hitherto firmly believed, he found the passage from the shortest to the longest imperceptible. The variation is so great that he scarcely found two specimens alike. There are not two peaks, but only one. The terms "high" and "low" must go. Kuhl's method was so thorough and the scale on which he worked so big, that there can be no question that his conclusions must override those both of Bateson and Brindley and of Diakonov.

He reveals one new and interesting form of variation. He found a certain proportion of the forceps notably broader than the rest and in these the tips do not meet, as they do in the slender forms. He showed that the largest males arise from the best-nourished and most-vigorous larvæ, but if at the critical moment of the last ecdysis from some external cause the pressure of the hæmolymph is inadequate completely to inflate the still soft forceps, they do not attain their full length. The scene is set for what we used to term the "high" or macrolabious form, but development is arrested. In other words, the short broad type with gaping tips is pathological in origin. Correlative to this result is his statement that the variation in the forceps depends entirely upon environmental conditions and never upon racial, hereditary causes. Such influences may be internal, as the amount of nourishment absorbed during larval life, that is to say physiological, as Julian Huxley has suggested; or they may be external and physical, when due to wounds.

There is much of interest and importance in Kuhl's work, which is worth careful attention.

V. MATERNAL CARE

The construction of a nest by the mother earwig, her care of her eggs, and even of the young larvæ have been a commonplace of books on insects ever since first described by de Geer in 1756.

It is, in truth, a remarkable phenomenon, almost if not quite unique among insects, to be fairly compared only with the domestic activities of the truly social groups. It has won the mother earwig a certain regard and does, in fact, really look like an instance of true solicitude, so that an observer may be pardoned for crediting the insect with affection and even sentiment of duty.

This anthropomorphic view, however, fades in the cold light of scientific enquiry, such as has been carried out by Weyrauch, who shows in a convincing manner that the entire series of phenomena can be accounted for by reflexes and tropisms.

In temperate Europe the common earwig is single-brooded. the larvae hatching in the spring. They are gregarious and this characteristic survives the last ecdysis, which occurs early in August. Consequently it is by no means rare to find assemblages of several hundreds hiding in suitable crannies, such as crevices in old posts. Sexual maturity is reached about September, which results in promiscuous intercourse, perhaps with temporary alliances, which lasts till about the end of October, when males and females pair off. After November such colonies no longer occur. The insects are then to be found in holes in the ground, but only in pairs. From December to February Weyrauch found seventeen nests with solitary females in them and sixty-two with a male and female but no eggs. In sixteen of these he marked the males by clipping their wings. On February 1 he opened the nests, and found eleven of the marked males still in them. Clearly monogamy obtains from November until oviposition.

At the first sign of impending oviposition the mother begins to dig a hole or to improve an existing one, in which to spend the winter and rear her brood. She brings out the pellets of earth in her mandibles and drops them outside the mouth of the hole. This is from 2 to 3 inches deep, usually in some sheltered but moist situation. Occasionally she excavates a branch tunnel in the upper portion, sloping upwards, which ensures drainage, at the end of which she lays her eggs. Out of many hundreds of nests examined, three were of this design.

The two essential conditions for her nest are (*a*) a minimum number of tactile contacts to satisfy her thigmotropism, and (*b*) a definite range of humidity. Excess of moisture, however, is more dangerous to the eggs than aridity.

From the moment she starts excavating her nest, the defence instinct is evoked, and she will attack any moving object that appears near her work. She tests it with her antennæ, as though to locate it, and then swings round and nips with her forceps. This is very like the ordinary defence of a non-maternal earwig, but definitely more energetic.

The fighting technique in the common earwig is stated to be poor. Goe writes that in the United States it is preyed upon by a Cicindelid beetle and by *Staphylinidæ.* Yet Fuller saw one drive away a big and active Carabus, and that curious isolated species *Anatœlia canariensis* I. Bol. is a formidable carnivore, for Escalera saw it successfully attack such dangerous enemies as young Mantids.

The most striking manifestation of this instinct is the expulsion of the male from the nest as soon as eggs are laid. To the mother he is not different from any other moving object, but the effect is to save the eggs from being devoured by the father. It is to be noted that no instance has been recorded of a male successfully defending himself against this divorce.

The stimulus which activates the oviposition and this marked instinct of defence is no doubt the rising temperature of the ground.

In the wild state oviposition may begin in December, but as a rule it does not occur till January, culminating in February and ceasing in March. The rhythm of oviposition varies considerably, but is constant for each individual. One may lay an egg every two hours, while another may lay six in an hour. The maximum clutch recorded is seventy, fecundity remarkable in an insect less than an inch in length. The average clutch seems to lie between forty and sixty.

If the nest be disturbed or become too wet or two dry, the earwig will abandon it and make another in a better site. She generally carries the eggs across in her mandibles, one by one, to the new nest. This involves a frequent journeying to and fro. Weyrauch placed a female on a perfectly flat surface of beaten mud which gave no distinctive point of contact. Then he pricked out a path in such a way that, once the insect had accustomed herself to the road he could at will make her follow any direction he wished.

This transportation of the eggs from the old to the new nest is closely connected with the instinct for re-assembling the eggs if they are scattered. Weyrauch noticed that when a heap of eggs was spread over a space of 8 or 10 inches, she collected them first into little provisional heaps, and then assembled these into one. She appears to work by trial and error.

The appearance of the first egg marks the crisis in her life. It brings into action two remarkable instincts. One, to lick eggs, her own or any strange eggs put before her: the other, if the eggs are scattered, to reassemble them into a heap.

The instinct to lick eggs is stronger than the instinct to eat them. Before her first laying, she would devour any egg as regardlessly as would the male. Very occasionally a hungry female will eat the first few of her own eggs, but the licking instinct quickly overcomes.

The effect of this licking and turning is the disinfection of the egg by the removal of the spores of fungi or other deleterious matter. This is the most important item in the whole equipment of the maternal instinct, for observation on several species has shown that eggs rot and become mildewed unless licked daily. But for this licking, earwigs would become extinct in a single generation. In such hygrophilous insects, the eggs are always exposed to a damp atmosphere without ventilation. In Ceylon, Green observed a *Diplatys* rolling and licking her eggs. As *Diplatys* is at one end of the scale of classification and *Forficula* at the other end, this observation suggests that this vital instinct is common throughout the order, regardless of latitude.

The eggs are highly susceptible to infection by fungus, for many are spoilt in spite of the mother's attention.

They are not discriminate. When Weyrauch placed eggs of spiders before a mother, and even globules of wax, she treated them in the same way as her own eggs. The requisite conditions are size, rotundity and surface. If he roughed the surface of the wax eggs with a pin, they were rejected. Clearly, the stimulus in this instinct is tactile.

There is, however, also a chemical stimulus, for the foreign eggs are eventually rejected during the continued process of licking and cleaning. This stimulus dominates the tactile, as bad eggs are at once destroyed, although the superficial conditions remain unaltered.

If several broody females are put together, their mutual attitude is hostile. There ensues keen competition for the possession of the eggs, in which the strongest female always ends in collecting all the eggs into a single heap, which she defends successfully against the rest.

The larva hatch about three weeks or a month after oviposition. Out of a total of four moults, the first two take place in the nest. The gregarious instinct is so strong that if they are scattered during the night, in the morning they will be found all asleep together in a pile. The mother is still useful to them for defence against intruders, but when she has completed her term and died, her body is still of use to the larvæ, for they eat her, just as they eat their own cast skins. Eggs taken away from the mother as soon as laid change colour quickly and become mouldy. This change of colour releases the eating instinct, for in that case the mother devours them at once.

If she be separated from her eggs for a certain time, the maternal instinct with all its separate actions and expressions is lost for good. It is a remarkable fact that all these manifestations of the maternal instinct, the digging of the nest, the defence of the home and brood, the re-assembly of scattered eggs, all are lost if the master-instinct of licking be checked. Once that has gone, it cannot be restored. When that has happened any eggs placed before her are at once devoured.

There is a transition period, varying from one to four days, during which there is a conflict between the two instincts. When Weyrauch placed an egg before a mother during this period, she picked it up in her mouth, turned it over two or three times, and licked it. Then suddenly the movements of her mouth-parts began to slow down and finally ceased. She waited then for a period from ten minutes to half an hour in a state of indecision, retaining the egg in her mouth. Then suddenly the mouth parts began to move again, either to clean or . . . to eat.

The maternal care may be prolonged as much as three months by substituting fresh eggs for newly hatched larvæ. This shows that the care of the brood is not dependent upon a hereditary fixed period, but upon the continuous presence of the necessary stimuli.

VI. Does the Common Earwig Fly?

Although the common earwig rejoices in the possession of the most highly specialised wing in the insect world, its power of flight has been repeatedly questioned. As long ago as 1839 Westwood found it necessary to bring forward "proof of their flying," which he did by describing some 50 specimens caught on palings freshly coated with pitch, to which some of them had their expanded wings still adhering.

Yet in 1917 Dr. Karl Verhoeff seriously questioned their power on purely theoretical grounds and dismissed records of their flight as unsatisfactory or unscientific. Two years later Mr. H. R. A. Mallock, F.R.S., queried their power, also on theoretical grounds, and quite recently L. Chopard, in his fine work *La Biologie des Orthoptères*, still declares himself unconvinced.

But all this theorising is superfluous, as there is abundant proof not only that the common earwig can fly, but that it does so frequently. Sir Edward Poulton has collected a number of instances, mainly indirect it is true, of mass flight, and a string of instances by entomologists of high standing of individuals flying. This has been confirmed by several writers in German reviews, of which the most interesting is the record of Herr Richter, who on a warm September afternoon in fifteen minutes saw fourteen earwigs flying in the sun.

THE DETERMINATION OF TEMPERATURE BELOW 1° K.

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1. INTRODUCTION.—Until recently the milestones on the path towards absolute zero have been the successive liquefactions of gases with low boiling points, the most important being air, hydrogen and helium. In this way the temperature regions down to about 60° K., 10° K. and then 1° K. have been made accessible. Soon after each liquefaction it was possible to determine temperatures in the new regions. This, however, was not the case in the newest region of all, made available a few years ago, by the magnetic method. The object of this paper is to describe the fundamental distinctions and to show how the problems peculiar to the new region can be solved.

After the liquefaction of air, hydrogen and helium the absolute temperatures could be determined with the gas thermometer, which I need not describe here, by applying the equation of state of the "ideal gas" $pV = \text{const.}T$. This relation can be deduced from kinetic theory on the assumption that the atoms or molecules can be treated as mass-points. This means that: (*a*) they do not possess any dimensions and (*b*) they do not exert any forces on each other. The energy of the gas then consists only of the kinetic energy due to the disordered motion of its constituents, and if one puts the absolute temperature proportional to this energy one arrives at the equation of state of the ideal gas.

Obviously an ideal gas cannot actually exist, for the fact that a gas condenses shows that there must be forces between the atoms, and the very small compressibility of the condensed phases contradicts the first assumption. However, it is easy to see the method of overcoming this difficulty. With increasing dilution of the gas the influence of the forces between the atoms and also the effect of their finite size become smaller and smaller, and thus a very dilute gas behaves very nearly like an "ideal gas." If, therefore, readings are taken with a gas thermometer filled at increasingly lower pressures, an extrapolation to zero pressure will give the temperatures which would have been obtained by using an ideal gas.[1]

In the new temperature region it is no longer possible to use a gas thermometer. The fact that the lowest temperature (0·7°), which has been reached by Keesom by pumping liquid helium, was attained only by reducing the pressure to a few thousandths of a millimetre, shows that there is no possibility of using a gas thermometer at temperatures of this order or still lower; for obviously the pressure in the thermometer must be lower than the vapour pressure at the temperature in question. A few values, of the vapour pressure of helium below 1° K., calculated from the second law, show that gases practically no longer exist below 0·5°:

T°	P mm.	T°	P mm.	T°	P mm.
0·7 . .	$1{\cdot}5 \times 10^{-3}$	0·3 . .	7×10^{-10}	0·05 . .	4×10^{-62}
0·5 . .	$2{\cdot}5 \times 10^{-5}$	0·1 . .	3×10^{-31}	0·03 . .	6×10^{-103}

2. THE THERMODYNAMIC SCALE OF TEMPERATURE.—So far we have taken the laws of an ideal gas as the basis of thermometry. It is true that this had always been the foundation of actual temperature measurement, but it is not the definition of temperature adopted by science. This definition, which was originally proposed by Lord Kelvin more than ninety years ago, is based on the second law of thermodynamics.

The simplest method of explaining this scale is to consider a Carnot cycle working between two temperature levels T_1 and T_2. In this cycle an amount of heat Q_1 is absorbed at the high temperature level and an amount $-Q_2$ delivered at the low temperature, the difference between them being given out as mechanical work (W). If we use a gas as the working substance, we expand it at the high temperature, move to the low temperature by an adiabatic expansion, compress it at the low temperature and return by an adiabatic compression to the starting point. If we use an ideal gas and work *reversibly* we can calculate the quantities involved and we find:

$$W = Q_1\frac{T_1 - T_2}{T_1} = -Q_2\frac{T_1 - T_2}{T_2} \quad . \quad . \quad (1a)$$

$$\frac{Q_1}{T_1} = -\frac{Q_2}{T_2} \quad . \quad . \quad . \quad . \quad . \quad (1b)$$

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[1] Of course the best gas to use in practice is that with the smallest interaction forces, viz. helium.

If in our cycle we use any other substance, or if we use any other *reversible* procedure, for example an electrical or magnetic one, then the same relation must hold. If it did not, this procedure could be combined with the first cycle in such a way that the result would be an apparatus doing nothing else than using up heat energy and turning it into mechanical energy. The non-existence of this famous apparatus (the perpetual motion machine of the second kind) is the starting point of the second law.

One of the two equations, (1), can now be used to define the temperature. We have still to choose the unit in which it is measured and for scientific purposes the Centigrade scale is generally adopted. We divide the difference between the boiling and the freezing points of water into 100 degrees and by carrying out a Carnot cycle between the two temperatures we can find the absolute temperature of the ice-point (T_x): It follows from (1*b*) that $Q_1/(T_x + 100) = -Q_2/T_x$ and therefore $T_x = \frac{100}{[Q_1/(-Q_2)]-1}$. The measurement of Q_1 and Q_2, therefore, gives the absolute temperature of the ice-point, and this result must be the same as would be obtained by calculating it by means of the laws of an "ideal" gas. It may be noted here that at present the best value for the ice-point in the absolute scale is 273·15° K.

It is true the thermodynamic scale and the scale of the "ideal gas" must lead to identical results, as equations (1) have been derived by means of the laws of an "ideal gas." But we have now arrived at a definition of the temperature scale which is independent of the use of any special substance and is only based on the very general property of heat which is expressed by the second law.[1]

3. AN EXAMPLE.—For the practical realisation of the thermodynamic scale it is not necessary to carry out an actual Carnot cycle whose experimental errors would generally be prohibitively large. Theoretically every law deduced from the second law of thermodynamics can be used to establish the temperature scale, some being more convenient experimentally than others. I shall illustrate the thermodynamic establishment of this scale by one simple example, the steam engine.

The work done by this engine can be derived from two different points of view. From the purely mechanical one the work done is determined by the saturation pressures at both temperature levels and the volume changes. On the other hand the Carnot cycle gives the work done as the amount of heat flowing from the higher to the lower temperature level (in this case the heat of evaporation) multiplied by the temperature difference and divided by the absolute temperature. Therefore there must be a connection between the change of equilibrium pressure with temperature, the absolute temperature, the heat of evaporation (λ), and the volume changes (ΔV), and this is the well-known formula of Clausius-Clapeyron:

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[1] It should be mentioned that the so-called International temperature scale which is adopted by law in most civilised countries approximates to the thermodynamic scale as closely as experimental accuracy allows. In this scale certain temperatures are taken as fixed, the values being given by the most accurate determinations, and the instruments for interpolating the temperatures in the intermediate regions are described and standardised.

$$\frac{dp}{d\mathrm{T}} = \frac{\lambda}{\mathrm{T}.\Delta\mathrm{V}} \quad . \quad . \quad . \quad . \quad (2)$$

If the heat of evaporation and the volume difference between the liquid and the gaseous phase are given, there is only one value of $dp/d\mathrm{T}$ which is in agreement with the second law. Any other value would permit the construction of perpetuum mobile of the second kind.

We can now use this formula for the determination of the thermodynamic scale by means of the equilibrium between a liquid and a gas. We start at a temperature T_a; we then lower the temperature to T_b and measure continuously the heat of evaporation and the difference of volume between the phases as functions of the equilibrium pressure. It follows from equation (2) that

$$\log\frac{\mathrm{T}_a}{\mathrm{T}_b} = \int_{p_b}^{p_a}\frac{\Delta\mathrm{V}}{\lambda}dp \quad . \quad . \quad . \quad . \quad (3)$$

If for instance T_a represents the boiling point of water and T_b the freezing point, then by putting $\mathrm{T}_a - \mathrm{T}_b = 100°$ again, we would arrive at a thermodynamic determination of the ice-point. If, however, T_a is already known in the absolute scale (and is for instance the boiling point of helium), then by proceeding in the manner described we can determine the temperatures below this as long as it is possible to determine ΔV and λ as functions of p.

4. The thermodynamic scale can be expressed from another point of view—namely, that of the entropy. The second law leads to a definition of a quantity, the entropy, which is a potential, that is, it depends only on the state of the system and not on the manner in which that state is attained. It is shown that the change of entropy is given by summing the amounts of heat with which the system is supplied in a reversible manner, divided by the absolute temperature. If an amount of heat Q is supplied to the system at a constant temperature the entropy is increased by the amount

$\Delta S = Q/T$. If this temperature is changing, the change of entropy is given by

$$\Delta S = \int_1^2 \frac{dQ}{T} \quad . \quad . \quad . \quad . \quad (4)$$

If we carry out a reversible adiabatic process dQ equals 0, and the entropy remains constant. The second law can be stated in terms of entropy in the following manner : The entropy of a closed system can only increase or, in the limiting case of reversible changes, remain constant.

Boltzmann has given the statistical interpretation of the second law. He has shown that the entropy is a measure of the probability of the state of a system or, in other words, of the disorder which is present in it. The fact that the entropy always assumes the largest possible value in a closed system means that Nature prefers the most probable state, that is the state of greatest disorder.

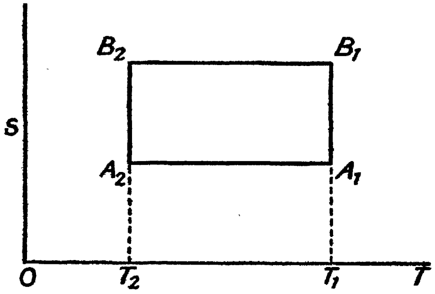

Fig. 1.

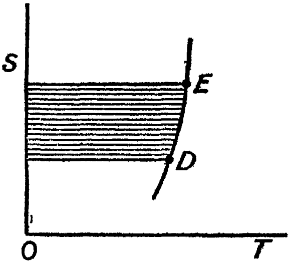

Fig. 2.

We shall now consider a Carnot cycle in the so-called entropy diagram (Fig. 1) in which the entropy is plotted as ordinate and the temperature as abscissa. We start with an isothermal expansion from A_1 which brings us to B_1. We continue with an adiabatic expansion which results in a lowering of temperature from T_1 to T_2. We then move in an isothermal way from B_2 to A_2 and finish with an adiabatic compression to A_1. Obviously the entropy changes at both temperatures are the same and thus we see relation (1*b*) $Q_1/T_1 = -Q_2/T_2$ illustrated in a different way. (The amount of heat connected with a reversible change from one state of a system (D) to another (E) can easily be shown on the entropy diagram, as is seen on Fig. 2. The shaded part is equal to this amount of heat as $Q = \int T\, dS$.)

We can now express the essence of the thermodynamic determination of temperature in the following way : If we have two states of a system and we can determine both the entropy difference between them and also the amount of heat that has to be supplied to the system reversibly to get from one to the other state, then T is given by $Q/\Delta S$. In other words, if we know that a certain amount of heat gives rise to a certain change of entropy (that means creates a certain amount of disorder), then we know at what temperature the heat has been supplied to the system. If the same change of entropy (the same increase in disorder) is effected in one case by a small supply of heat and in another by a large amount, in the first case the temperature is low and in the second high.

Although the principles of the thermodynamic scale have been known a very long time, in practice it has hardly ever been applied. The reason is that until a few years ago, it was always possible to realise this scale with the gas thermometer which was simpler and more accurate than the thermodynamic method. In consequence the determination of temperature and the gas laws are intimately connected in the minds of nearly every physicist, and the thermodynamic foundation of the temperature scale is unfamiliar to most of them. This fact has become evident recently when in the new region below 1° K, as we shall see, there was no alternative to the thermodynamic method of determining temperatures. It therefore seemed advisable to explain the principle in detail.

5. THE MAGNETIC METHOD: I.—We now have to discuss the situation in the new temperature region attainable by the magnetic method. This is not the place for a detailed description of this method and readers who wish to know more are referred—for instance—to an article by the author in the *Handbook of the Science Museum* (Book 3, Very Low Temperatures, 1937). But I shall have to say a few words on the principles.

Let us for this purpose look at the entropy diagram representing the Carnot cycle (Fig. 1). The act of lowering the temperature consists of an adiabatic expansion, an isentropic process. We can carry out such a process because the entropy of a gas depends also on its volume, and by changing this adiabatically and reversibly the temperature must necessarily change. It is because the "disorder" in a gas depends to a great extent on the volume—besides, of course, on temperature—that gases are such an effective means of lowering temperature. Once the gas has been condensed, it has changed into a system of a much higher state of order, and we can no longer alter the temperature effectively by adiabatic changes of volume. Thus there comes a temporary halt on the way down to absolute zero when the possibilities of the gas with the lowest boiling point have been exhausted.

A way out of this situation was proposed by Debye and by Giauque, who pointed out that there are other systems—namely, some paramagnetic salts—in which a great disorder still prevails even at the lowest temperatures attained so far, and that it should be possible to use these systems to penetrate to lower temperatures. The existence of this disorder within the system is proved by the fact that, even at 1° K, these salts exhibit a magnetic susceptibility that is inversely proportional to temperature, i.e. they follow Curie's Law. It can be shown that this law is fulfilled if the elementary dipoles are free to point in any direction, or at least if the directions allowed by the quantum rules are equally probable This means that there are no interaction forces present which will make any one of the directions more probable than the others.

I cannot give here the theoretical explanation for this rather unexpected behaviour which, however, is fully explained by modern theory. Certain ions containing unbalanced electron spins supply the necessary elementary magnets and by building them into a salt containing very many other constituents—generally alums containing much water of crystallisation—the interaction between the dipoles becomes small because of the great mutual distances. The historic salt of this kind was gadolinium sulphate, but later many more have been found.

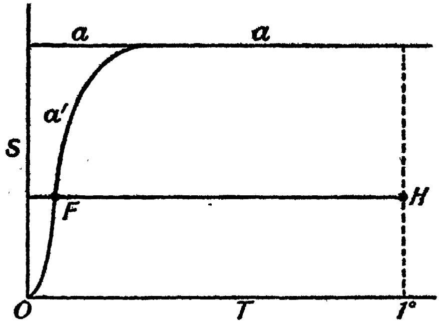

FIG. 3.

There being no force to direct them, the dipoles are oriented at random owing to the influence of thermal motion. Thus we have a high state of disorder present or a big entropy. If, however, we apply a magnetic field, this tries to order the dipoles and thus diminishes the entropy. With such a salt, therefore, we can now effect similar changes as with a gas, the volume change of the latter being replaced only by the change of magnetisation.

In Fig. 3 the entropy of such a salt at low temperatures is plotted.

For sake of simplicity the entropy due to the thermal vibrations of the lattice is neglected altogether, but this very nearly corresponds to the actual facts, as these degrees of freedom have practically died out in the helium region. The entropy in zero field (*a*) is therefore a constant, the only disorder in the system being due to the random distribution of the magnetic dipoles. By magnetising the salt isothermally we diminish the entropy and to do this we have to carry away an amount of heat $Q = T \cdot \Delta S$. In our figure this brings us to the point H. If we now take the field away adiabatically, we move along a line parallel to the T-axis (as S has to remain constant), and we end at the intersection of that parallel with the curve *a*.

In our figure, as we have no such intersection, we would arrive at absolute zero, but according to the third law of thermodynamics, Nernst's Theorem, this cannot actually happen. This law postulates that all systems lose their entropy completely at absolute zero—they pass into a state of perfect order. Thus we have to expect that the curve cannot continue for ever as a parallel to the T-axis, but must bend down somewhere and end in the origin, for instance, as indicated by line a'. Then a demagnetisation does not carry us to absolute zero, but to a temperature corresponding to point F.

The atomistic explanation of the entropy of the paramagnetic salts finally dropping to 0 is that the dipoles are not *absolutely* free, but that there exists interaction forces, albeit very weak ones. Finally, however, they force the system into an ordered state even without an external magnetic field. This will happen in the temperature region where the interaction energy becomes of the order of the thermal energy kT. Thus we can define a characteristic temperature θ as u/k (u = interaction energy) and it will be this order of temperature that we can reach with a demagnetisation. It should be mentioned that the observed values of θ are of the order of 1/100° to 1/10° K.

6. THE MAGNETIC METHOD: II.—The actual experiment consists in magnetising a suitable paramagnetic salt at the lowest temperature that can be reached by evaporating liquid helium (roughly 1° K.), the salt being in heat contact with the bath. After the heat of magnetisation has been carried away to the bath the salt is thermally insulated. The magnetic field is removed and the salt cools. To determine the temperature reached by the salt we must choose some property of the salt that depends on temperature and it is obvious that the simplest one is the magnetic susceptibility of the salt itself. This susceptibility must then be correlated with temperature. We have seen that in the region between 1° K. and 4° K.,

the susceptibility of the salts which are used in the magnetic method, follows Curie's Law. This suggests that an extrapolation of this law might be used for calculating a provisional temperature (denoted by T*) and in fact this has been done by all experimenters working in this field. To give an example: we measure the susceptibility by means of a mutual inductance placed round the salt and the deflection of a ballistic galvanometer (δ) is proportional to the susceptibility. By calibration between 4° K. and 1° K. we find, for instance, $\delta_{mm} = \frac{100 \cdot 0}{T}$. If after demagnetising we observe a deflection of 2000, it therefore follows that $T^* = 0 \cdot 050°$.

However, it is easy to see that such an extrapolation is bound to give wrong results. Curie's law is valid as long as the magnetic dipoles are absolutely free, and we have seen that in this case every demagnetisation would bring us to absolute zero. The fact that this has not happened shows that an extrapolation of Curie's Law is not justified. It is true that, if the final temperatures reached are high compared with the characteristic temperature θ, the T* can be expected not to differ very much from the real temperatures, but when demagnetising from big initial fields we must reach temperatures where the differences between the T* and the real temperatures becomes appreciable. The fact that, in some cases, at very low temperatures the susceptibility has been found even to fall with decreasing temperature after having passed through a maximum is a striking demonstration of this.

It might be thought that we could use a device similar to that used with the gas thermometer which was filled with successively smaller concentrations of gas, and then by an extrapolation to zero concentration obtain the absolute temperature. The analogous procedure here would be to bring a second paramagnetic salt in thermal contact with the first salt and to derive the temperature from the susceptibility of this "thermometric substance" instead of from the salt itself. We could try to approach the ideal state by decreasing the concentration of the magnetic ions. This could be done by forming a mixed crystal between the paramagnetic salt in question and an isomorphous non-paramagnetic salt. We could then measure the T* of the thermometer as a function of the concentration and an extrapolation of the T* to zero concentration would give the absolute temperature.

Theoretically that is possible—at least in some cases—but in practice it would be very difficult. It is quite simple to insert a gas thermometer in a liquefied gas and to measure the effect of decreasing concentration in the gas thermometer on the pressure, as a simple mechanical wall divides both parts of the system completely. But in our case the situation is different. We cannot localise the magnetic fields, and if we tried to measure the susceptibility of our thermometer we would get a *mixtum compositum* with that of the salt that has been used for the cooling. We could only avoid this by having the salt and thermometer so far apart that the mutual influence would be negligible, but at the same time they must be in thermal contact. With the present state of experimental technique that is very difficult and therefore this method cannot be used.

7. THE THERMODYNAMIC SCALE IN THE "MAGNETIC" REGION.—We see therefore that the only possibility left is to establish the thermodynamic scale. After what has been explained it is simple to see how this can be done.

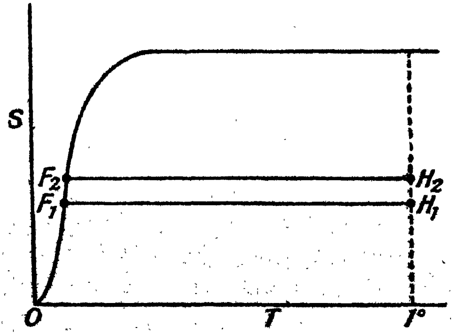

FIG. 4.

To fix our ideas let us assume we want to determine the average temperature of the points F_1 and F_2 (Fig. 4). We know that they correspond to deflections of the galvanometer, δ_1 and δ_2, and that they have been reached by demagnetising from 1° K. and from the fields H_1 and H_2. We have now to find out (1) the difference in heat content, (2) the difference in entropy between F_1 and F_2.

(1) To find the difference in heat content, *i.e.* the amount of heat necessary to heat the sample from F_1 to F_2, we have to measure the specific heat of the salt, which in principle offers no difficulties.

(2) To determine the difference in entropy we have to measure the amount of heat developed in moving from line 1 to line 2 at a temperature known in the absolute scale. For example, this can be done by measuring the heat of magnetisation when increasing the field from H_2 to H_1 isothermally. (An alternative way is to heat the sample at a constant field in the region where the temperature is known, *i.e.* at or above 1° K.)

Thus we see that both Q and ΔS can be measured and therefore the absolute temperature can be determined. It may be useful to consider the procedure in terms of the Carnot cycle by subjecting the system to a change analogous to that in Fig. 1. The only difference between this cycle and that in Fig. 1 is that the change at the lower temperature is not strictly isothermal. This has no consequences in principle but only means that we measure the average temperature of F_1 and F_2, a difference that vanishes altogether by making $F_1 - F_2$ infinitesimal.

So much for the general scheme of procedure. There are many ways of carrying out the actual experimental determination, for instance, those proposed by Giauque, Keesom, Kurti and Simon, de Haas and Wiersma (a full list of the literature on this subject is given in a paper by Kurti and Simon, *Phil. Mag.*, **26**, 840, 1938). The choice between them must be determined by the particular experimental conditions, and there is especially one experimental difficulty which must be discussed—namely, the measurement of the specific heat. For the heat conductivity of these salts is so bad that the normal way of heating, viz. the heating coil, does not give an homogeneous heating and therefore cannot be applied to thermodynamic measurements.

Kurti and Simon have overcome this difficulty by heating the sample with γ-rays. The energy carried by a normal source of γ-rays is very small and moreover only a small percentage of it is absorbed by the salt, but the greatest temperature interval over which we have to heat is only 1°, and thus it is quite possible to carry out the measurement in a reasonable time. For example, a 200 millicurie source of γ-rays, at 5 cm. distance, will in four hours heat iron alum from the lowest temperatures attainable to 1° K. Obviously heating by γ-rays is homogeneous to a very high degree so long as the incident radiation is homogeneous over the whole sample.[1] To ensure this, when working with large samples, two or more sources are placed in suitable positions and rotated round the sample.

I do not wish to discuss this technique of heating in greater detail, but I must mention that it is difficult to determine the absolute amount of heat supplied to the sample, and therefore any method which does not depend on the absolute values of heat supplied has an advantage. A method that fulfils this condition has recently been described by Kurti and Simon and as at the same time it gives a simple illustration of how to establish the thermodynamic scale, I shall describe it briefly.

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[1] An alternative method of heating a sample homogeneously is to mix a small amount of radioactive substance with it. For example, to get the same heating as described above, 1/100 millicurie of polonium has to be mixed with every cubic centimetre of iron alum. In this case most of the heating is done by α and β rays which are all absorbed within the sample. The disadvantage of this method is that it is never possible to stop heating the sample.

Let us assume that by demagnetising from 1° K. and 10,000 gauss we have reached a point corresponding to a galvanometer deflection of 2010. We then carry out two experiments : (1) we place a radioactive source at a given distance from the sample and measure the time necessary to heat it until the deflection has fallen to 1990. We find that this takes 60 sec. ; (2) again we begin at a deflection of 2010, but now we switch on the original field of 10,000 gauss and then begin to heat. We continue to heat for such a time that, after switching off the field, again we end with a deflection of 1990.[1] Let us say that this takes 1000 sec. In the latter case the temperature is known in the absolute scale as by switching on the original field we have arrived at the original temperature [2] of 1°. As the temperatures must be proportional to the amounts of heat which are themselves proportional to the times of heating we now know that $T_x/1° = 60/1000$ or $T_x = 0{\cdot}060°$ K. This temperature corresponds to a $T^* = 0{\cdot}050°$, according to the calibration formula : $\delta = 100{\cdot}0/T^*$.

The thermodynamic scale has been established experimentally by Giauque and McDougall for some gadolinium salts in the region down to 0·3°, and by Kurti and Simon for iron alum down to 0·03°. It has appeared from these experiments that, as had been expected, the T* did not differ very much from the actual temperature down to the order of magnitude of the characteristic temperature θ, but that below this temperature very great differences occurred.

8. SECONDARY THERMOMETERS.—In this way the foundations of the absolute temperature scale in the new region have been laid. It might be supposed that once the scale had been established for one salt it would only be necessary to calibrate a number of secondary thermometers against this salt and the determination of temperature below 1° K. would be accomplished, but this is not so. We have already seen that the simplest secondary thermometer, *i.e.* a salt calibrated against the absolute temperature, is not of much use in systems cooled down with other salts as it is very difficult to avoid

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[1] The correct time can be interpolated from two experiments.

[2] As the temperature has been raised slightly by the heating, a somewhat smaller field than the original has to be applied, the magnitude of which can easily be found out by interpolation between two experiments.

the mutual influence of the two salts. It is true that during the last few years a number of secondary thermometers of the resistance type, to which this objection does not apply, have been developed in different laboratories, but here other difficulties arise. They do not lie so much in finding materials whose resistivity is still sufficiently temperature dependent for them to be used as thermometers, but in ensuring that the temperature shown is that of the system as a whole.

It has been mentioned that the thermal conductivity of the salts used in the magnetic method is very bad indeed, so bad that at very low temperatures equilibrium is not established for several hours. As the salt is always subjected to a heat influx from outside, a thin layer on the outside has a much higher temperature than the bulk of the material, and therefore a thermometer wire wound round the sample will not show the temperature of the inner part of the sample. The difference between the temperature measured and the temperature required will be further increased by the heat influx down the wires leading to the thermometer. As the electrical resistance of the wire at these low temperatures is very sensitive to impurities and mechanical strain, bending or any other deformation can influence the resistance appreciably, and therefore any arrangement in which the thermometer is surrounded by the salt but subjected to deformation is also excluded. It is outside the scope of this paper to discuss how, eventually, it will be possible to overcome these difficulties. At the present stage—although secondary thermometers have some field of application—they cannot be used for all purposes. For some time, therefore, it will be necessary to use as thermometer the susceptibility of the salt which served to attain the low temperature, and to correlate the susceptibility to the absolute temperature in the thermodynamic way described above.

9. STILL LOWER TEMPERATURES.—It has been pointed out by Kurti and Simon and by Gorter that it will be possible to attain still lower temperatures by replacing substances with electron spin paramagnetism by those with nuclear paramagnetism. In this case the interaction energies and thus the characteristic temperatures are obviously much smaller. It is premature to go into details at present as the experimental difficulties have not yet been overcome. But if these lower temperatures are attained the determination of temperature will follow the same lines as those indicated in the electron spin region, and no new difficulties of principle will be encountered.

10. LIMITATIONS OF THE NOTION OF TEMPERATURE.—Another question, however, is whether we are still justified in attaching a

physical meaning to any particular value of the temperature when approaching very near to the absolute zero. Heat is the disordered motion of the atoms or molecules and therefore all thermal properties are of a statistical character. It is obvious that one cannot speak of the temperature of a single atom; more generally, if the number of degrees of freedom of a system becomes very small the fluctuations of the thermal quantities may become big compared with their absolute value, and at any given moment it is meaningless to describe the state of the system by the values they happen to possess.

I will illustrate this by an example from a more familiar field. We can attach a perfectly good meaning to the statement that the death-rate is 1/10 per cent. per month in a town of, say, 100,000 inhabitants, and we can draw conclusions as to the state of health of the town from this figure. But if we consider a small community with only 10 inhabitants the death rate will be zero for several years and then if one of the inhabitants dies it will jump suddenly to 10 per cent. It will then drop to zero again for a long time. Obviously it would be wrong to say [1] that the community is very healthy for a long time and then extremely unhealthy for a short time.

We are faced with a similar situation in thermal systems if they possess only a few degrees of freedom. Thus it is to be expected that we can safely speak of the temperature of a system if it consists only of a sufficient number of components, a condition always fulfilled if, for example, the system is large compared with the wavelengths of visible light, *i.e.* if it can be seen. This expectation would be right according to the classical theory but, as has been pointed out by Planck and by Schaefer, this is no longer true in quantum theory. While in classical theory every component of the system can have any energy, however small, we know now that the energy is subdivided into quanta. It is obvious that this must have the same effect as a reduction of the number of the particles: a particle in zero quantum state does not count at all for our purpose. Thus for every system, of whatever number of particles, there exists a temperature below which it becomes effectively a system of few degrees of freedom. Planck and Schaefer have shown that it is only when the temperature is big compared with $\theta/N^{\frac{1}{3}}$ (where θ is the characteristic temperature [2]) and N the number of atoms in the system) that the system can be regarded as having sufficient degrees of freedom.

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[1] It is quite true, of course, that by averaging over long periods of time one can draw some conclusions in such a system with few degrees of freedom.

[2] As the characteristic temperature we have to take the smallest energy which the degree of freedom can assume divided by Boltzmann's constant. Thus in the case of a paramagnetic salt we arrived at the θ already discussed above. If the energy of the system consists only of the thermal vibrations, we arrive at the Debye θ.

It is of interest to discuss whether in practice we can attain such a temperature. We have here to distinguish two cases: The first is whether it is possible for the system with which we attain the low temperature to become finally a system with an insufficient number of degrees of freedom. Our immediate impression is that this is impossible, as a system of few degrees of freedom only can not possess sufficient heat capacity to be useful for the purpose of reaching low temperatures, and this impression is right: We have seen that we can attain with any process temperatures of the order of magnitude of θ (where θ is the characteristic temperature connected with this process). Combining this fact with Planck-Schaefer's relation, we find, as a necessary condition for such a system to remain one possessing sufficient degrees of freedom, that the number of particles must be large compared with 1 and this is a trivial condition. It follows, incidentally, that, however low a temperature we reach, there will always be a system of a reasonable volume with sufficient degrees of freedom to measure it.

Secondly, is it possible to cool another system to such a temperature that it becomes a system with insufficient degrees of freedom? This is certainly possible. It can easily be calculated that already at 0·01° K. a cube of diamond[1] of 1/10 mm. side will be in such a state, and we can, of course, cool such a piece to 0·01° K. or lower, by putting it in thermal contact with a paramagnetic salt. Naturally there will be very big fluctuations within the diamond and after breaking the thermal contact one can only predict with considerable uncertainty what the state of the insulated piece of diamond will be. What will happen, if we now bring the diamond in contact with a thermometer? This thermometer must necessarily possess many more degrees of freedom than the piece of matter whose temperature it is supposed to measure. Consequently the state of the thermometer will profoundly change the state of the diamond, and one is faced with difficulties very similar to those familiar in discussions on the uncertainty principle.

11. THE LOG T AND THE 1/T SCALE.—The last point to discuss is whether our familiar definition of temperature is the most convenient that could have been chosen. The measurement of temperature differs from the measurement of most other quantities in that it is not sufficient to define the unit but it is also necessary to define what is meant by temperature. The present definition of temper-

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[1] The Debye characteristic temperature of diamond is about 2,000°.

ature is given by the equation (1*b*), $Q_1/T_1 = - Q_2/T_2$, or what is equivalent, the temperature is proportional to the energy of an ideal gas. But other definitions are also possible. For example a new scale τ_1 could be defined in such a way that τ_1 is proportional to log T. In this case a Carnot cycle working between the upper and the lower end of each degree of this scale would have the same efficiency over the whole scale. It would also be possible to define a temperature τ_2 as proportional to 1/T.

It is, of course, not possible to say that one scale is right and the others are wrong, as this is only a matter of definition, but we can discuss whether one scale is more convenient than the others. Both the τ_1 and τ_2 scale would differ from our old T scale in that absolute zero would be shifted to infinity and this particular distortion of the scale would have some advantages for the low temperature physicist. For him the degree between 101° and 100°, for example, is much less extended than that between 2° and 1°, not to speak of that between 1° and 0°. We have also seen that absolute zero is unattainable,[1] a fact which would certainly support the claims of one of the τ scales and for these reasons the adoption of one of the τ scales has lately been sponsored in various quarters.

I do not think, however, that it will be worth while to make the change. First we would have to alter all our formulae to the new scale, and it is simple to imagine the many strange results if we give up the proportionality of the new " temperature " to the kinetic energy. For instance, the specific heat of an ideal gas would vary with the temperature and so on. Also all our tables would have to be recalculated, and in view of the enormous amount of material accumulated by physicists, chemists and engineers the amount of labour involved seems out of all proportion to the advantage which might be gained. But I do not think that we need be afraid of such a change happening. If, as it seems, the c.g.s. system is not adopted in Great Britain and the United States merely because the change over would lead to some inconvenience, it does not appear likely that a different temperature scale will be introduced for which there is infinitely less reason.

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[1] At first sight it might be thought that, as in the τ-scales absolute zero becomes infinite, the unattainability of absolute zero becomes a triviality. This, however, is not true. A new fact—decided by experiment—cannot be made superfluous by a mere mathematical transformation.

HEAVY OXYGEN

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The announcement of the discovery, in the atmosphere, of an isotope of oxygen, mass 18, was made by Giauque and Johnston (*Nature*, 1929, **123**, 318). The evidence, given later (*J. Amer. Chem. Soc.*, 1929, **51**, 1436) consisted in the examination of the atmospheric absorption spectra from the data given by Dieke and Babcock (*Proc. Nat. Acad. Sci.*, 1927, **13**, 670). It was found that the lines of the very faint *A′* band, which appear to be a duplicate of the lines of the *A* band (which are due to oxygen) agree very closely indeed with calculation for the absorption band of an oxygen molecule built up from an atom of mass 16 and one of mass 18. In a second paper (*J. Amer. Chem. Soc.*, 1929, **51**, 3528) from a study of further data of Babcock (*Proc. Nat. Acad. Sci.*, 1929, **15**, 471) the above authors give evidence for an isotope of oxygen mass 17. They estimate, from measurements of the relative intensity of the lines (Babcock, *loc. cit.*, and *Nature*, 1929, **123**, 761), that ^{18}O is present to the extent of 1 part in 1250, and ^{17}O, about 1 part in 10,000. The value for ^{18}O has since been revised.

Many attempts have been made, by different methods, to concentrate the ^{18}O.

The fractional diffusion method of Hertz (*Z. Physik*, 1934, **91**, 810) and Barwich (*ibid.*, 1936, **100**, 167) gives a sufficiently increased concentration for experimental use, but the quantity obtained is small.

Urey and Greiff (*J. Amer. Chem. Soc.*, 1935, **57**, 321) calculated the equilibrium constants and enrichment factors for a number of exchange reactions, and suggested reactions for the separation of isotopes. In the case of water and carbon dioxide, the ^{18}O is concentrated in the latter :

$$2H_2{}^{18}O + C^{16}O_2 \rightleftharpoons C^{18}O_2 + 2H_2{}^{16}O$$

The fractionation factor is 1·047. This result was confirmed by Webster, Wahl and Urey (*J. Chem. Physics*, 1935, **3**, 129) who passed carbon dioxide through water (from which the deuterium had been mostly removed) in spiral wash bottles and then passed the carbon dioxide together with excess hydrogen over a nickel catalyst to give water. It was found that the density of the water obtained from the carbon dioxide was 0·9999892, compared with the density of the water in the wash bottles which was 0·999979. Thus the found difference in density was 10·2 parts per million ± 1 p.p.m. and the calculated difference, using 1·047 as the fractionation factor, 10·3 p.p.m.

Although this method does not appear to have given successful results in the concentration of heavy oxygen, heavy nitrogen has been concentrated by a similar reaction.

The vapour pressure of $H_2{}^{18}O$ differs only slightly from that of $H_2{}^{16}O$. Wahl and Urey (*J. Chem. Physics*, 1935, **3**, 411) find that the ratio of the vapour pressure of $H_2{}^{16}O : H_2{}^{18}O$ is 1·012 at 11·25°, and 1·0077 at 46·35°. By extrapolation they obtain the figure 1·003 at 100°; and by calculation from the results of Lewis and Cornish (*J. Amer. Chem. Soc.*, 1933, **55**, 2616) an average value of about 1·005. While these figures are only approximate, they indicate that very efficient columns are necessary for fractionation. The latter workers, using a 20-foot column, obtained differences of density of 60 to 80 p.p.m. between the water at the top and bottom when equilibrium was reached. It was necessary to run the still for two days in order to reach the stationary state.

Huffman and Urey (*Ind. and Eng. Chem.*, 1937, **29**, 531) obtained a fivefold increase in the concentration of the heavy oxygen in water using a special still, built in seven sections. Each section was 5 feet long and 6 inches in diameter and was fitted with 87 alternately fixed and rotating cones. The object of these cones was to give as large a surface of contact between liquid and vapour as possible to enable rapid equilibrium to be reached. The surface of the cones was kept covered with a film of water by the rotation of the alternate sets, which forced the liquid flowing down the column over the surfaces.

During the fractionation of water, the deuterium concentrates at a greater rate than the heavy oxygen, and it was found by Urey, Pegram and Huffman (*J. Chem. Physics*, 1936, **4**, 623) in one experiment, that during the time the concentration of the heavy oxygen had been increased by three times the natural abundance, that of the deuterium had been increased by about forty times. So that, although ordinary water contains some ten times less deuterium than heavy oxygen, a stage is fairly rapidly reached at which the increased density is due more to the former than the latter.

Heavy oxygen water normalised with respect to hydrogen may be prepared by electrolysis in a divided cell, and recombining the oxygen with cylinder hydrogen (as this hydrogen, prepared by electrolysis of ordinary water, may be from 5 to 10 p.p.m. light, a correction must be applied).

Attempts to concentrate heavy oxygen by the electrolysis of ordinary water have shown that the separation obtained is very small. Thus Selwood, Taylor, Hipple and Bleakney (*J. Amer. Chem. Soc.*, 1935, **57**, 642) found that by electrolysing 117 litres of water down to 1 c.c. the concentration of ^{18}O was increased only by 10 per cent., actually from 0·202 per cent. to 0·222 per cent. This gives a separation factor $\alpha = 1{\cdot}0088$. They calculated that in order to obtain 1 c.c. of 99·5 per cent. $H_2{}^{18}O$, it would be necessary to start with 10^{500} c.c. of ordinary water, which is more than all the water on the surface of the earth.

A number of workers have determined the proportion of ^{18}O to ^{16}O. Thus Bleakney and Hipple (*Physical Rev.*, 1935 (ii), **47**, 800), by mass spectrographic analysis of over 100 samples of oxygen from various sources, obtained an average value of 1 to 500. Considerable differences were found between different samples, and these authors therefore suggest, as also have other workers, that for refined atomic weight determinations, a simultaneous isotopic analysis is also necessary.

The relative proportion of heavy to light oxygen in air and in water has been determined by Jones and Hall (*J. Amer. Chem Soc.*, 1937, **59**, 259) who have made a study of the reaction :

$$2H_2{}^{16}O + {}^{18}O_2 \rightleftarrows 2H_2{}^{18}O + {}^{16}O_2$$

This was carried out by passing clean dry air, mixed with the vapour from water at 90° to 95° C., over a platinum wire heated electrically to 1800° A. The steam was condensed, and the water recirculated until equilibrium was reached in some three days. The water was then found to have increased in density by 7 p.p.m. Other workers, to whom reference may be made in the above paper, have obtained results of the same order, differing only by about ± 1 p.p.m. (*e.g.* Dole, *J. Chem. Physics*, 1936, **4**, 268 ; Morita and Titani, *Bull. Chem. Soc. Japan*, 1936, **11**, 36 ; 414).

Greene and Voskuyl (*J. Amer. Chem. Soc.*, 1936, **58**, 693), in offering an explanation of this difference, point out that the carbon dioxide in the atmosphere is almost certainly in equilibrium with water (*cf.* Webster, Wahl and Urey, see above). Thus the oxygen is heavier in the carbon dioxide than in the water. When plants take up a molecule of carbon dioxide and one of water, they return a molecule of oxygen to the atmosphere and build one atom of oxygen into the plant structure :

$$nCO_2 + nH_2O \longrightarrow (CH_2O)_n + nO_2$$

Of the two atoms in a molecule of oxygen thus returned to the atmosphere by the plant, at least one, and possibly both, must come from the carbon dioxide, and from this it follows that the oxygen in air would be expected to be heavier than that in water. For a discussion of other possibilities, reference may be made to a paper by Dole (*J. Chem. Physics*, 1936, **4**, 272).

The determination of the density of water from various sources has been made by Dole (*loc. cit.*), who electrolysed the different samples and combined the oxygen with a sample of (light) tank hydrogen. He obtained the following figures :

Water from Lake Michigan	$\gamma = -12{\cdot}2$
Water from Atlantic Ocean	$\gamma = -10{\cdot}6$
Water vapour from atmosphere	$\gamma = -13{\cdot}7$

the differences in density being due to the oxygen isotope. It is of interest to note that the water from the lake is intermediate in density between that of the water from the sea and air ; this may be due to concentration of the heavier isotope by evaporation of the sea water. The same author (*J. Amer. Chem. Soc.*, 1936, **58**, 580) also calls attention to the errors which may be made, in the determination of deuterium in compounds, by combustion to give water, unless due allowance is made for differences between oxygen samples.

The determination of the proportion of heavy oxygen in water may be made by density measurements. An accuracy of the order of one part per million may be obtained after careful purification of the water. In the case of heavy oxygen in organic compounds, this may be converted to water by combustion with excess hydrogen, using a cracking surface of granulated quartz coated with platinum followed by a nickel catalyst containing 2 per cent. thorium (*cf.* Russell and Fulton, *Ind. and Eng. Chem., Anal. Ed.*, 1933, **5**, 384).

Lewis and Luten (*J. Amer. Chem. Soc.*, 1933, **55**, 5061) give the refractive index of heavy oxygen water, determined with a sample containing 0·5 per cent ^{18}O, normal with respect to hydrogen :

$$\Delta n = 0{\cdot}0008y,$$

where y is the increase in mole fraction of $H_2{}^{18}O$ or atom fraction of ^{18}O over its value in ordinary water and Δn is the difference in refractive index at 25° between the given sample and ordinary water. As the refractive index of water containing excess deuterium has also been determined (Luten, *Physical Rev.*, 1934, **45**, 161), the above authors give an equation for determining the deuterium and ^{18}O in a given sample of water, knowing the density and refractive index.

Cohn and Urey (*J. Amer. Chem. Soc.*, 1938, **60**, 679) determine the ^{18}O using a mass spectrograph as described by Bleakney (*Phys. Rev.*, 1932, **40**, 496). A known weight of the water to be analysed is sealed in a tube with a known volume of carbon dioxide, and shaken at room temperature until equilibrium is reached (2–3 hours). The proportion of ^{18}O in the carbon dioxide is then readily determined : the method has the advantage that very careful purification of the water is unnecessary.

It was soon recognised that heavy oxygen would be of great help in investigating the mechanism of certain types of reactions, and a number of workers have examined the possibility of exchange of oxygen in water and organic compounds, to find the types of compounds which would exchange, and the conditions under which the reaction would take place.

Some examples are given below.

COHN AND UREY (*J. Amer. Chem. Soc.*, 1938, **60**, 679)

Compound.	Temp. °C.	Time.	Result.
Methyl alcohol	25	24 hours	No exchange
	100	7 hours	No exchange
Glycerol	25	2 weeks	No exchange
	100	6 hours	No exchange
Phenol	25	44 hours	No exchange
Butyric acid	25	2 weeks	No exchange
	100	24 hours	No exchange
Monochloracetic acid	25	24 hours	Partial exchange
Trichloracetic acid	25	42 hours	Complete exchange
Furmaric acid	25	44 hours	No exchange
Succinic acid	25	44 hours	No exchange
Amyl acetate	25	44 hours	No exchange
Acetaldehyde	25	24 hours	Complete exchange
Acetone	25	24 hours	No exchange
	100	24 hours	Partial exchange
Urea	25	54 hours	No exchange

ROBERTS (*J. Chem. Phys.*, 1938, **6**, 294)

Substance.	Catalyst.	Time.	Temp. °C.	Number of Oxygens Exchanged.
Methyl alcohol	0·1 N–HCl	24 hours	25	0
Methyl alcohol	0·1 N–NaOH	48 hours	25	0
Nitrobenzene	0·03 N–HCl	24 hours	25	0
Nitrobenzene	0·03 N–NaOH	24 hours	25	0
Potassium acetate		48 hours	25	0
Acetic acid	0·1 N–HCl	40 days	25	2
Benzoic acid		4 hours	100	2
Benzoic acid	0·1 N–HCl	4 hours	100	2

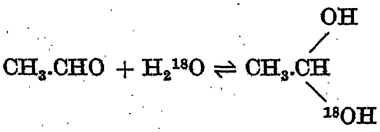
Herbert and Lauder (*Trans. Faraday Soc.*, 1938, **34**, 1219)

Substance.	Time (hours).	Temp.	Exchange.
Acetic acid	0·1 to 166	25	0
Potassium acetate	0·1 to 118	25	0
Acetamide	0·1 to 71	25	0

Koizumi and Titani (*Bull. Chem. Soc. Japan*, 1938, **13**, 463, 607)

Substance.	Time (hours).	Temp. °C.	Number of Oxygens Exchanged.
Glucose	2 to 48	100	1
Benzaldehyde	1 to 2	110	1
Benzoic acid	1 to 5	130	1
Phenol	24 to 48	100	0·1
Succinic acid	1	130	2
Succinic acid	2 to 5	130	4
Maleic acid	20 to 45	100	4
Fumaric acid	3 to 45	100	1·5
Phthalic acid	20	100	0·2
Terephthalic acid	20	100	0·3

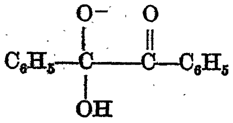
Herbert and Lauder (*Trans. Faraday Soc.*, 1938, **34**, 432), in a study of the interchange of oxygen between water and acetaldehyde, find that there is a rapid exchange, equilibrium being reached in about 20 hours at room temperature (20° C.).



$$CH_3.CHO + H_2{}^{18}O \rightleftharpoons CH_3.CH\begin{cases} OH \\ {}^{18}OH \end{cases}$$

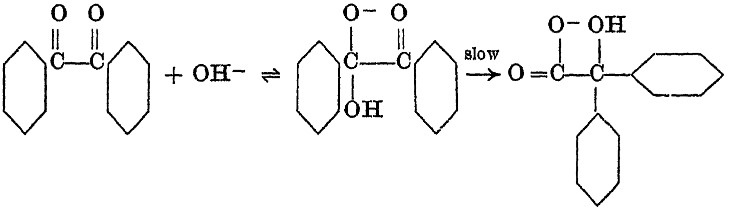
Cohn and Urey (*loc. cit.*) in a more complete investigation of the exchange between heavy water and acetone, find that the reaction is catalysed by hydrochloric acid, sodium hydroxide and glass beads, and with 0·002 N–HCl or 0·005 N–NaOH is complete in one hour at 25°. A detailed kinetic study showed that the reaction does not proceed through the enolic form.

Roberts and Urey (*J. Amer. Chem. Soc.*, 1938, **60**, 880) have used heavy oxygen water in a study of the benzilic acid rearrangement and find that the exchange is much more rapid in alkaline solution than in neutral. A kinetic study of the reaction by Westheimer (*J. Amer. Chem. Soc.*, 1936, **58**, 2209) indicated that the reaction must take place through the intermediate ion

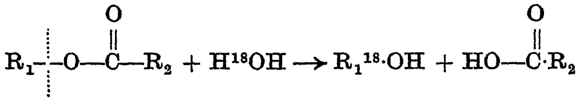


$$C_6H_5{-}\underset{OH}{\overset{O^-}{C}}{-}\overset{O}{\overset{\|}{C}}{-}C_6H_5$$

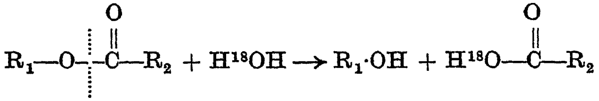
suggested by Ingold (*Chem. Soc. Annual Reports*, 1928, **25**, 124). Of the three mechanisms which are suggested for the rearrangement, and which cannot be distinguished kinetically, the reaction with heavy water indicates that the correct one is the rapid reversible addition of hydroxyl ion which is followed by the rearrangement, which is the rate controlling step :



The most interesting organic problems which have been attacked by means of heavy oxygen water, are those of esterification and hydrolysis. The hydrolysis experiment has been carried out by Polanyi and Szabo (*Trans. Faraday Soc.*, 1934, **30**, 508). The reaction might take place in two ways (*a*) and (*b*)

(*a*)


$$R_1 \vdots O-\overset{O}{\overset{\|}{C}}-R_2 + H^{18}OH \rightarrow R_1{}^{18}\cdot OH + HO-\overset{O}{\overset{\|}{C}}\cdot R_2$$

(*b*)


$$R_1-O \vdots \overset{O}{\overset{\|}{C}}-R_2 + H^{18}OH \rightarrow R_1\cdot OH + H^{18}O-\overset{O}{\overset{\|}{C}}-R_2$$

If the break occurs as in (*a*), a heavy alcohol will be produced, while if it is as in (*b*), the alcohol will be light. Primary amyl acetate was hydrolysed with heavy water in alkaline solution. The alcohol, collected and passed over bauxite at 400°, gave water and pentene. The water obtained was light, showing that reaction (*b*) is the correct one, and it is the acid oxygen bond which breaks, and the oxygen in the alcohol comes from the ester and not the water. The experiment also shows that amyl alcohol does not exchange with heavy water when heated in alkaline solution for two days at 70°, the conditions of the experiment.

The esterification of a carboxylic acid (benzoic acid) with heavy methyl alcohol, has been carried out by Roberts and Urey (*J. Amer. Chem. Soc.*, 1938, **60**, 2391). This reaction may be represented in two ways (*c*) and (*d*).

$$(c)\quad C_6H_5.CO\boxed{OH + H}{}^{18}OCH_3 \rightarrow C_6H_5.CO^{18}OCH_3 + H_2O$$

$$(d)\quad C_6H_5.COO\boxed{H + H^{18}O}CH_3 \rightarrow C_6H_5.COOCH_3 + H_2{}^{18}O$$

Under the conditions of the experiment, it was known that methyl alcohol and methyl benzoate do not exchange with heavy water, and that benzoic acid exchanges slowly. The water recovered from the esterification was light, showing that reaction (*c*) takes place, and that the water formed originates from the benzoic acid.

The exchange of potassium phosphate with heavy water has been studied by Blumenthal and Herbert (*Trans. Faraday Soc.*, 1937, **33**, 849). The exchange was found to be complete in a few hours at room temperature:

$$K_3P^{16}O_4 + 4H_2{}^{18}O \rightleftharpoons K_3P^{18}O_4 + 4H_2{}^{16}O$$

Sodium sulphate has been found not to exchange in neutral solution (Datta, Day and Ingold, *J. Chem. Soc.*, 1937, 1968), but complete exchange takes place in the presence of hydroxyl ions in 24 hours at 100°.

Potassium sulphate also does not exchange in 100 hours at 180° (Titani and Goto, *Bull. Chem. Soc. Japan*, 1938, **13**, 667), but with the addition of a little potassium hydrogen sulphate exchange takes place in 20 hours at 180°.

Aten and Hevesy (*Nature*, 1938, **142**, 952) have used heavy sodium sulphate to study the problem of whether the oxygen in sulphate exchanges, in the body, with other oxygen atoms present. The heavy sodium sulphate was injected into a rabbit and the urine collected for 24 hours; the sulphate was recovered as barium sulphate. This was reduced with purified carbon at 900° and the product, mixed with excess hydrogen, passed over a nickel catalyst to give water. After allowing for the ordinary sulphate secreted by the rabbit, the density of the water obtained shows that most of the sulphate administered leaves the body unchanged.

An artificial atmosphere containing ^{18}O + 300 p.p.m. heavy has been used to study isotopic exchange in animal respiration (Day and Sheel, *Nature*, 1938, **142**, 917). The carbon dioxide recovered, gave water which had a density of + 40 p.p.m. This experiment supports the view that oxygen enters directly into carbon oxidation and is exhaled as carbon dioxide. Mills and Urey (*J. Amer. Chem. Soc.*, 1939, **61**, 534) who find that at 30° the exchange between heavy carbon dioxide and water is nearly complete in 7 minutes, suggest that this may account for the low value obtained.

A NEW CONCEPT FOR CAUSAL EMBRYOLOGY: THE MORPHOGENETIC POTENTIAL

By ALBERT M. DALCQ

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In the period since the war, our knowledge of the early stages of development has been greatly advanced (especially for the Amphibia) by certain discoveries. Spemann's experiments led to the concept of an Organisation Centre, with its various corollaries, and to the notion that the induction of the ectodermal structures, particularly of the nerve-cord, is due to the transfer of one or more substances from the primitive gut-roof into the overlying layer. Vogt's intra vital staining experiments established the exact position, previous to gastrulation, of the chief materials which later build up the main organs of the embryo, and allowed him to draw the celebrated maps of the presumptive rudiments. It may be said, without any exaggeration, that these two parallel advances have definitely brought causal Embryology to the level of an exact discipline.

Nevertheless, it is the law of scientific progress—and K. von Baer, the founder of modern embryology, was already conscious of it—that each hardly won solution leaves our minds still unsatisfied. In spite of these fundamental achievements, our descriptive and our experimental knowledge were not clearly related with each other; regulation could not be understood without admitting the organiser as an harmonic equipotential system, which is a merely verbal explanation; data concerning the young gastrula were difficult to reconcile with some results obtained on the freshly fertilised egg, *e.g.* the well-known Schultze's experiment; again, the cog-wheels of Ontogeny, as revealed in the Amphibia, were probably present in other Vertebrates but rather difficult to transpose to other types of animal organisation (the Invertebrates). In my opinion, these four difficulties have now been largely solved by several lines of researches converging into the Concept of *Morphogenetic Potential.* My purpose is here to follow its own Ontogeny and to define as far as possible the new problems that are thereby substituted for the old ones.

In 1931, inspired by the late Professor Albert Brachet, under whose guidance it had been my privilege to study, I endeavoured to get a personal insight into the organisation of the Amphibian egg. At that time it was not definitely settled whether the presumptive neural territory of an Anuran egg could or could not form a neural plate when deprived of the inductor action of the primitive gut-roof. It seemed to me a crucial experiment to cut the blastula or the very young gastrula right across at about the lower limit of the neural territory, to turn round the animal pole cupola thus isolated 180° and make it heal on again to the lips of the fresh wound. The presumptive neural rudiment would, in this way, be removed to some distance from the inductor material, and would nevertheless be submitted to the strains and stresses inherent in gastrulation. The result was negative but, contrary to what Spemann had obtained by the same technique on the newt egg, when the section was made somewhat lower so as to include all the organiser territory, double embryos were regularly obtained. My attention being necessarily drawn to their structure, the analysis was facilitated by the advantages of the egg of the *Discoglossus pictus*, the "painted frog" of the Mediterranean region. I must refrain from describing here many interesting features, such as the kinematic aspects of gastrulation in inverted embryos, the gradation of results according to the stage operated and to the level of the cut, the sometimes complicated fate of the various rudiments, etc. The most significant facts, for the point of view here discussed, were, in the order I observed them: (1) a balanced relation between the auditory vesicles of the twins, a larger right one of one member corresponding to a smaller right one of the other, and vice versa; (2) a frequent dislocation of the pronephros rudiment between the twinned embryos; (3) sporadic correspondences between the noto-chord, somites, heart, gills, side-plate mesoderm, etc., forcing the conclusion that the rudiments had a noticeable tendency to follow their original fate.

The results concerning the pronephros are to be especially emphasised, for the normal presence of three nephrons, with their three nephrostomes, allowed the beginning of a quantitative approach. Plotting the data of classical embryology and of Vogt's results, the nephrons may be thought of as three tiny, deep-seated lumps of presumptive material, at the boundary between the somites and the lateral plate material. It is easily understood that a latitudinal cut may involve these nephron rudiments, or the field surrounding them, to various degrees, and realise a total or partial transfer of the nephritic material. But other cuts, performed in a definite ascending direction, would presumably divide the pronephritic field without dislocating it, and afford embryos with more complete pronephritic structures. Both kinds of results have been found by appropriate operation.

The conclusion of this first line of research was that presumptive territories have more than a topographical value. The "*Bahnung*" (of Vogt) is more than a hypothesis. Each rudiment has a "personal inclination" striving to realise its normal fate. But this vague notion must not be rigidified into a predetermination or a germinal localisation. Most of the rudiments can, after excision, form again *de novo* out of the adjacent material. This has been demonstrated by direct operations on the pronephros material. It is impossible to obtain an embryo lacking its pronephros. If the "inclinations" are so clearly revealed by the translocations it depends entirely on the fact that the method respects the relations of the anlagen to the co-ordinates of the field-gradient system.

This remark is at once a reply to possible objections concerning the experiments described above and a transition to the field-gradient conception which has proved so illuminating for the analysis of early development. It is not necessary to retrace here how the brilliant idea of Boveri has led to the field concepts of to-day, thanks to the regeneration studies of Child, the fine researches of Harrison on limb-polarity, the theoretical formulations of Weiss, the masterly experiments of Hörstadius on sea-urchin blastomeres, and many other important steps. This evolution of the biological thought has been well depicted by Huxley and de Beer in their excellent text-book of Embryology.

In Brussels, our conversion to this new current of ideas happened only progressively. Professor Brachet had been, for serious reasons, a believer in germinal localisations. The "inclinations" which we have now arrived at, are still germane to that topographical representation. In my studies on Ascidian merogony I adhered as long as possible to the preformation of some main rudiments in the cytoplasm. But when I was able to approach the question in a quantitative way, it appeared necessary to give up any qualitative preformation for the sake of a truly dynamic interpretation. In the meantime, I had induced Pasteels to unravel the presumptive organisation and the kinematics of telolecithic eggs (birds, reptiles, etc.) and this successful study had drawn the attention of my co-worker to the importance of the yolk. He was thus led to examine once more the results of maintained inversion of the young frog egg. In 1929, Penners and Schleip had published a most valuable analysis of the intricate Schultze's experiment, and detected the morphogenetic influence of yolk masses, but they had been unable to discover the conditions governing the formation of a new embryonic axis.

I have no space to describe here the two suggestive results obtained by Oppenheimer on *Fundulus* and by Vintemberger on *Rana fusca*, nor the detail of Pasteel's experiments. The decisive factor in his advance was the partial reversal of the egg, placed in different oblique conditions. It was thereby recognised that morphogenesis is the result of an interaction between two main constituents of the egg, the vitelline gradient, and the field of the grey crescent which exists in the cortex.

Given a mass of yolk in a definite position relative to the grey crescent, when two opposite blastopore lips are formed, the one turned towards the grey crescent will be distinctly accelerated compared to the opposite one. If the axis of gastrulation is perpendicular on the primitive dorso-ventral axis of the egg, the half turned to the grey crescent will be larger, which means that the invagination has been more active on that side. It thus appears that the morphogenetic activity is, in some way, a function of the product $c.v$, *i.e.* the intensity of the cortical field multiplied by the concentration of the yolk at any point in the egg.

Inspired by these observations, Pasteels and I felt that it might be possible to interpret the normal distribution of the rudiments. A fertilized frog egg shows, from the stage of pronuclei copulation, a distinct grey crescent with its focus about the equator. The apex of the yolk gradient is, of course, at the vegetal pole. Now, if we consider a chemical reaction which is a function of c and v (directly or indirectly), the locus where the greatest synthesis will occur will be half-way between the focus and the vegetative pole. As soon as a given threshold is reached (and are not most biological processes ruled by a threshold ?) that territory will begin to invaginate. But very soon the same condition will be realised in adjacent regions, superior, lateral, inferior ones. Accordingly the blastopore furrow will be enlarged, vegetal material will migrate upwards, more animal cells will be tucked round the still large blastopore lip. The features of gastrulation are nothing but the dynamic expression of the concentric or at least focalised waves of cv synthesis successively attaining the critical threshold.

But now you cannot suppose that the fate of the materials will be indifferent to the fact that they were, from the beginning, laden with yolk or impregnated by the cortical field. This means that the ratio $\frac{c}{v}$ will have some importance. Indeed, if you search for the probable direction of lines with a constant $\frac{c}{v}$, you find that any step from the cortical focus must be accompanied by some elevation on the axis, to compensate the yolk denominator by reduction.

This is the direction of the limits between the presumptive areas of noto-chord and somites, somites and side-plates, side-plates and endoderm. These frontiers correspond each to a definite $\frac{c}{v}$ threshold. Now the main data of the Vogt plan are: (1) the place where invagination begins and comes to an end, both explained by the *cv* threshold; (2) the boundaries between the somites, indicating the general metamerisation, which corresponds to successive waves of the *cv* reaction; (3) the separation between the internal rudiments just interpreted by the $\frac{c}{v}$ ratio; and (4) the extent of the ectodermal induced regions, neural plate, neural crest and epidermis. For this last point, induction is essential. It is known to be exerted by the prechordal plate for cephalic structures, by noto-chord and somites for the nerve-chord, by more lateral mesoderm for epidermis. This intensity of induction does not entirely depend on the *cv* reaction, for the pharyngeal material, with the highest and earliest amount of that complex, is not the strongest inductor. Also materials with an equally high ratio have a different inducing power. But this power is dependent on both product and ratio. The most active material, the prechordal plate, is the region of highest $\frac{c}{v}$ with a relatively high *cv*. Anteriorly, the ratio falls rapidly but the product grows: special induction of the facial territory. Posterior to the prechordal plate (above it on the plan) the product falls towards the animal and ventral regions, whereas the ratio, in more and more lateral direction, successively descends under the respective threshold.

The morphogenetic potential of a material is the sum of its morphogenetic qualities resulting from the *cv* product and $\frac{c}{v}$ ratio, and the equilibrium of these resulting substances with the adjacent territories. It translates, in our actual state of knowledge, the existence of each territory into terms of definite substances acting by their relative concentration. These substances are not always firmly bound to their cytoplasmic substrate. Induction occurs wherever the difference of potential allows a diffusion of the substances and all that that implies.

Our definition of the concept being thus attained, I should wish to justify its validity, show its advantages and demonstrate its heuristic value.

It could be objected that the morphogenetic potential is only an unnecessary duplicate of Driesch's concepts of potency and potentiality. But these expressions, although still of some usefulness,—*e.g.* for the activity of isolated blastomeres, are charged with the whole background of Driesch's vitalistic interpretation of regulation. They imply that a definite territory has two possible kinds of activity, the normal one and some other one inspired by the " purpose " of regulation. On the other hand, they merely refer to the original situation, while the morphogentic potential includes that situation and all the events occurring on account of the subsequent reactions. It is a four-dimensional concept in the full sense advocated by Woodger. Or one may say that it is a short concise translation of a complex chemical situation, modifiable by any sufficient difference of concentration.

Let us consider the recent results obtained by Yamada, in Vogt's laboratory, by a remarkable series of grafts performed on late gastrulæ and early neurulæ of the newt. Parts of the mesoderm were transplanted to various regions, in order to test the degree of determination. This is not nearly so advanced as was previously imagined. Most of the grafts developed according to their new position, but some yielded really strange results. When somite material was added to the ventral mesoderm of the cœlomic lining, it formed there typical pronephros tubules. But if the graft included noto-chord material, it was transformed into a complex of all the mesodermal organs: noto-chord, somites, pronephros, side-plates, etc. Vogt, searching desperately for an interpretation, has spoken of " Selbst-induktion." In terms of Morphogenetic Potential, the interpretation is evident. In most of the grafts, the local substances characterising the potential invade the added material, and afford an integral assimilation. When, however, the difference of potential attains a certain value in favour of the grafts, the resulting equilibrium corresponds to an intermediate level of the potential scales, that is, for example, the pronephros level.

It would also be interesting to consider from the same point of view the recent investigation of the Dutch embryologist Raven. This worker has submitted to a thorough analysis the well-known experiment of Spemann and Geinitz, who implanted a piece of presumptive ectoderm into the organizer, took it out of this site after some hours, and showed that it had acquired the power of induction. One of the points now revealed by Raven is that a piece of tissue secondarily grafted may in certain cases remain superficial, become neuralised, and impose on the underlying material a transformation into noto-chord and somitic mesoderm. This back-induction is evidently due to a new equilibrium of the Morphogenetic Potential, by diffusion of the special substances brought with the graft. This seems to indicate that, at least in the very first stages of morphogenesis, there is no difference between the morphogenetic substances of the mesodermal layer and those of the induced material. But probably those substances are soon firmly bound to the cells, especially to the neural ones. Some other strange data of Raven also become plainly intelligible in terms of Morphogenetic Potential.

An analogous situation should, indeed, be discussed for each graft of the blastopore lip, in the original experiment of Spemann. The normal scale of potentials must necessarily be restored to obtain the observed harmonisation of the structures in the secondary embryonic axis. This remark has led us to examine the relations as they exist in typical cases of regulation. It has thus been recognised that the functional basis of regulation is the automatic restoring of the typical gradient-system inside the morphogenetic potential field. On the one hand, this constancy of the field and gradient decrements is the core of Child's theory. On the other hand, we are able to show the true Ontogeny of that gradient system, which means an interesting transition between causal Embryology and the analysis of regeneration. If we come back to the utilisation of the new concept for positive research, two kinds of results are at least to be mentioned. By submitting one-half of the organisation Centre to a strong U.V. irradiation, considerable modifications may be obtained in the form and size of the mesodermal organs. They are easily to be understood by a local elevation or, more frequently, a lowering of the potential (Brandes). The second result concerns the cortical field. What its precise nature is has hitherto not been determined. But a skilful analysis of J. Brachet has shown that, in the first hours after fertilization, a subcortical distribution of sulphydryl groups may be demonstrated. This other field is most probably correlated with the cortical one which we have till now alone considered. This is certainly an important hint that the chemical nature and the physical properties of the fundamental morphogenetic substances ought to be most accurately studied. But whatever may be discovered in that direction, we shall need none the less the synthetic notions described in this paper.

It is time to show, finally, that their interest is not restricted to Amphibians (from which many other arguments could be derived in favour of them). For Vertebrates, it is now well established especially by the researches of Pasteels on telolecithic eggs that there is, even in widely separated phylogenetic groups, a true unity of plan and especially of morphogenetic movements. The differences are mainly of a quantitative order, and they may be conceived of as a progressive elevation of the morphogenetic Potential, a steepening of the field gradient, and accordingly a still larger cerebral organisation. The mutations responsible for the successive steps of vertebrate Evolution must have concerned the yolk gradient and especially the cortical field. For prochordates, the rather intricate results obtained by merogony have received a satisfactory interpretation, at least for the present, by an application of the above defined concepts.

For other groups, Echinoderms, Insects, Spiralia, the representation is certainly different, but it also has a binary basis. And especially for Echinoderms a definition of morphogenetic potential may be given in terms almost identical to the Chordate definition.

It is consequently not exaggerated to assert that the concept described will bring some fresh ideas into the Evolution problem. It seems to be a useful tool for thinking out development in its joint actual and historical aspects. It helps us to understand how qualitative differences, apparently irreducible, have emerged from merely quantitative differences. This advance brings the embryologist into a position extraordinarily similar to that of the physicist, who builds up the qualities of the atoms from numerical variations in the electrons and positrons. It seems to me certain that such a point of view is extremely favourable to scientific progress.

EROSION AND SOIL CONSERVATION IN THEORY AND PRACTICE

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THE major problems which harass the hydraulic and the agricultural engineer in these days are those of erosion, the transport of eroded materials by streams and the silting up of estuaries and lower reaches where the soil is re-deposited. The subject seems to have first engaged the attention of scientists some fifty years ago, when Du Bois, a French engineer, found that the mass of silt carried by a stream could be expressed in terms of the shear exerted by the stream on its bed, which he equated empirically to the product of the specific weight of the fluid—equal to 1, on the metric system, for water—the depth of the stream, and the slope of its bed. A number of investigators have followed up this idea, but it really gives far too simple a picture of the processes going on. No two rivers give the same Du Bois coefficient—the factor relating shear to silt load—while in a single stream the factor varies from time to time.

To get a true picture of the dynamics of the phenomenon we need to delve more deeply into the mechanism by which individual particles are carried up from the bed into the stream. On the bed itself there is, of course, a frictional force exerted through the boundary layer of fluid which bathes it. The value of this force on unit area is determined by the viscosity of the fluid times the gradient of velocity at the bed. The extent to which the shearing force can shift an individual particle depends on the cohesion which subsists between that particle and its fellows, and on the pore space in the soil, for this latter largely decides the ease with which the fluid can permeate the soil. Once the fluid has invested the particle in this way, it stands a much greater chance of overcoming its immobility, since the force then becomes a more direct push on the particle instead of a rubbing of its top surface. If the current can adequately penetrate beneath the grain, another effect, the Magnus effect, comes into play, tending to lift the particle into the main stream. Thus, suppose there is a large grain resting above the general level of the bed but supported by it. This is undermined by the current, but owing to the restricted space beneath and the prevailing velocity gradient the speed of the fluid below the grain is less than that which passes over it. It is well known that in such a situation a vertical force originates directed towards that side of the obstacle where the speed is less, that is, there is now a tendency to lift the grain into the current. If this lift exceeds the weight of the grain in the fluid, it will rise until it comes to a place where the diminishing velocity gradient and consequent reduced lift balances the force of gravitation on it. All the time it is being impelled downstream. In a static equilibrium, it would remain at this level as long as the velocity gradient there remained constant, but in fact it overshoots this position, and falls again to repeat the evolution *ad inf.* While then, the largest pebbles are rolled along the bottom the moderate-sized ones execute a series of long or short hops. (Still smaller ones remain in suspension all the time in a turbulent stream through a process to be described shortly.) The height and length of these jumps naturally depend on the specific gravity and size of the grains so that when they are all more or less of a size, the height of this "saltation zone" as it is called (from Fr. *sauter*) is fairly definite. In a river or on the desert, it does not often reach a great height, but over snow-covered terrain the light snow flakes readily hop to a height of several feet. Arctic explorers relate that in a wind a dog team at a short distance may be invisible while the driver's head and shoulders may be seen emerging above the wraith of driving snow. Often too this leaping by well-graded sand grains leads to the formation of regular ripples on a mobile bed, for they tend to rise and fall at definite places. Once these ridges are established they act as jumping-off and landing-places for all succeeding grains as long as the stream or wind speed does not vary.

If the stream is running above the critical Reynolds' number all the finer grains are held in permanent suspension by turbulent mixing. On the mixing theory the particles in a given stratum travel on the average a certain horizontal distance, called "mixing-length," before merging their identity in the stratum next above or below. (This division into strata is quite arbitrary, but, in the theory, is carried to infinitesimal limits.) This mixing length is an inverse measure of the degree of turbulence, and obviously depends on the vertical components of velocity fluctuations such as subsist in turbulent flow. The theory is formally similar to that of molecular diffusion. A dye let into the bed of the stream would gradually infuse the whole fluid if it were kept stationary, or moving at such a slow speed that streamline flow persisted. In such a case the diffusing agency would be the kinetic or Brownian motion of the molecules themselves and would be interminably slow compared to that in turbulence. Only very light microscopic particles can be visibly affected by Brownian motion, but when turbulence sets in the fluctuations of velocity transverse to the stream can carry fair-sized grains to considerable heights above the bed. In both cases, the force which limits their upward excursions is gravity. Eventually they reach a mean distribution such that the concentration of particles varies exponentially with height above the bed, at least over that part of the stream wherein the mixing length is constant. This was first shown for turbulence by Hurst. It had been demonstrated for molecular distribution earlier in the classical experiments of Perrin on colloidal suspensions. Hurst used, not a channel, but a suspension of graded sand in water contained in a tall cylinder. The sand was kept in suspension by a stirrer which agitated the water into rotary and at the same time turbulent motion. He found by removing pipette samples at different levels in the tank that the logarithm of the silt concentration was actually proportional to distance above the floor of the vessel, as theory demanded, and that the degree of turbulence was roughly represented by the square of the speed of stirring. The author has repeated Hurst's measurements with a mixed sand in a rectangular glass tank and shown by mechanical analysis of the samples that the logarithmic distribution for *each* size still holds for the type of sand one would find in a natural river, but, of course, the coefficient connecting the two variables (log. concentration and height) is not the same for each size, the heavier ones congregating more in the lower strata of the river. It was also shown by passing thin beams of light athwart the tank to fall on a photoelectric cell on the far side that the light cut off by the suspension could also act as a measure of the silt load carried in suspension at the level of the beam. This was an important conclusion from the point of view of the channel experiments to be described next.

The hydraulic engineer is, of course, concerned with the total quantity of silt carried in suspension. This will depend largely on conditions near the bed, and we may group the relevant influences as follows;

(*a*) the velocity gradient perpendicular to the stream, and, in particular, its value at the bed;

(*b*) the degree of turbulence;

(*c*) the silt carried; the size, shape and specific gravity of the grains.

(*d*) the configuration of the bed, its slope and roughness.

In a natural stream all these factors intervene in a way which does not allow of the sorting out of their respective contributions. The author therefore proposed to study the transport of a loosely compacted sediment formed into a bed of as nearly as possible spherical grains of the same size and specific gravity, before passing on to natural silt. The Reynolds' number of the flow was to start from below the critical and eventually exceed it. Apart from this, only the first factor would vary in a given experiment. The velocity gradient was to be measured by a calibrated hot-wire anemometer and the distribution of the silt across vertical sections of the channel was to be measured by the turbidimeter just mentioned, *i.e.* by the use of a narrow horizontal beam of light and a photoelectric cell. The channel was constructed with plate glass sides 6 ft. long extending between two reservoirs of 10 cu. ft. capacity. The experimental portion was 1 ft. wide and allowed a maximum water depth of the same value. The two reservoirs were connected through a 3-in. pipe and a 2-h.p. centrifugal pump, so that the water after spilling over a weir in the downstream reservoir was returned to the upstream one where it again passed over a weir before entering the channel. It was essential that the mounting of the lamp and photoelectric cell should be such that they could be adjusted both vertically and horizontally without losing orientation. Fig. 1 shows a transverse section of the channel with the beam of light (chain line) for measuring silt concentrations. Light from the lamp, A, passes through the circular and cylindrical lenses L, L, and the adjustable slit S_1 and

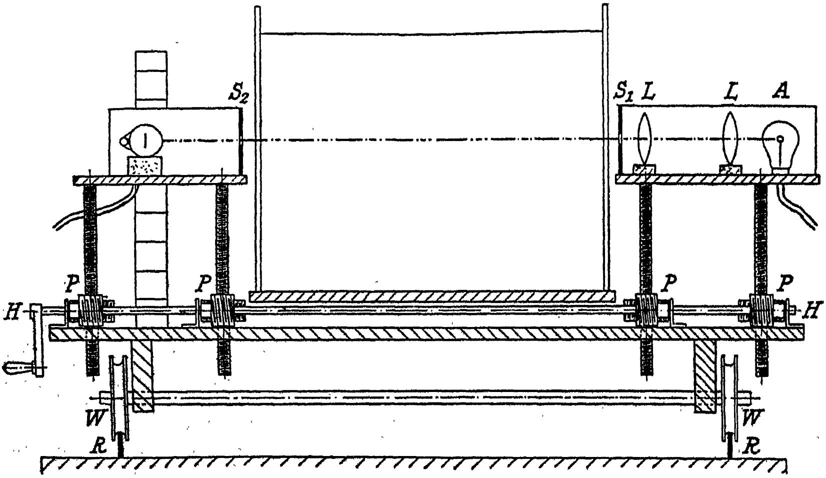

FIG. 1.—Apparatus for measurement of silt concentration.

crosses the channel as a narrow beam to fall on the cell after passing a second slit S_2. By means of wheels rolling on rails, R, R, the whole apparatus could be moved along parallel to the axis of the channel. A handle rotating a long screw H, H, served to raise or lower the pillars P, P, P, P, through worm gears and so change the level of the light beam relative to the water without loss of orientation of beam and cell.

It had previously been shown that the extinction of light by silt and hence the defect of photoelectric current was proportional to the total projected area of the intervening particles, *i.e.* to Σnd^2, where n is the number of particles of diameter d, provided the concentration of the silt were not too great. This calibration was carried out by interposing a glass-walled tank containing various numbers of uniform particles, maintained in suspension by continual stirring, into the beam of light.

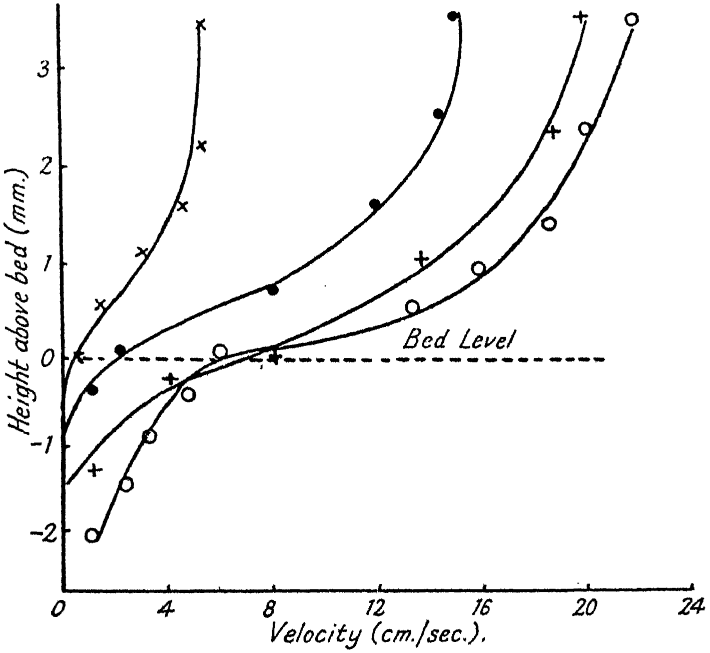

FIG. 2.—Velocities in and above soil bed in channel.

A typical set of results showing the silt concentrations and velocity gradients for mean channel speeds of 5, 15, 20 and 25 cm. per sec. respectively is shown in Fig. 2, with a bed formed of loosely compacted soil laid on the floor of the channel. In this particular case, the water readily penetrates the original bed. Within the saltation zone, the velocity gradient changes from a positive to a negative slope, indicating that within the mobile part of the bed the grains are rolling and jumping over each other in a manner that befits the action of a shear on a quasi-solid substance.

As far as silt concentration measurements in the channel go they confirm the logarithmic distribution of theory except within a centimetre of the bed, where, owing to the steep velocity gradient, the mixing length is not constant. It is, in fact, well known from aerodynamic investigations that a thin stratum of fluid in contact with a solid boundary is in streamline flow even when the main flow outside is turbulent. This non-turbulent boundary layer still persists when the "solid boundary" is changeable as it is in a river bed. Outside this region, which is barely penetrable by our instruments owing to its tenuity, there is another of extreme turbulence which eventually decreases in intensity to that characteristic of the main stream. Data deduced from measurements of the silt concentration in close proximity to the bed indicate that in the first few millimetres above the saltation zone, the mixing length rises in approximately linear fashion to a value which remains nearly constant until the free surface of the stream is approached.

The author has also carried out an investigation of the velocity and silt distribution in a section of a natural river, to wit the Tyne at Newcastle, where it is tidal and is not only very turbulent at the half-ebb but carries a measurable quantity of material—practically all inorganic—in suspension. A vertical traverse of the stream was made beside the piers of the Swing Bridge. The velocity was measured by an Amsler meter. This is of the torpedo form, the revolutions of the pusher screw when exposed to the current of the river being communicated to a counter on the dock above. For silt estimation a sampling bottle in a loaded can was let down at the same spot to various prescribed depths. The can filled from the top through an opening from which the cork was drawn by a cord when the bottle had reached the desired depth. To let the water with its suspended silt into the bottle with the least possible disturbance the air could only be driven out slowly through a vent as the water entered to take its place. The samples were submitted to mechanical analysis in a photoelectric sedimentation apparatus which has been described before (this journal, XXXI, p. 463) and the masses corresponding to each size plotted against depth (Fig. 3). It will be observed that these plots are nearly straight lines.

In a second experiment, an attempt was made to determine the silt concentration *in situ* using a photoelectric turbidimeter. To a piece of wood was fixed at one end a 6-volt lamp in a focussing tube

—an ordinary galvanometer lamp and lens unit rendered watertight with a covering of pitch at the joints—and at the other end a photoelectric cell in a watertight case. To the centre of this wooden bar was fixed another at right angles with a heavy weight on the end. When this T-piece was lowered into the water the sinker kept it in a vertical position with the aid of guy ropes to counteract the force of the current which tended to deflect the apparatus. In this way the beam of light and the cross-bar remained horizontal. Connections led to a battery for the lamp supply and to a sensitive galvanometer for the photoelectric current on *terra firma*. (The research was not completed without the usual vicissitudes which seem to accompany most of the excursions of the physicist outside his laboratory. A series of results would have to be given up to with-

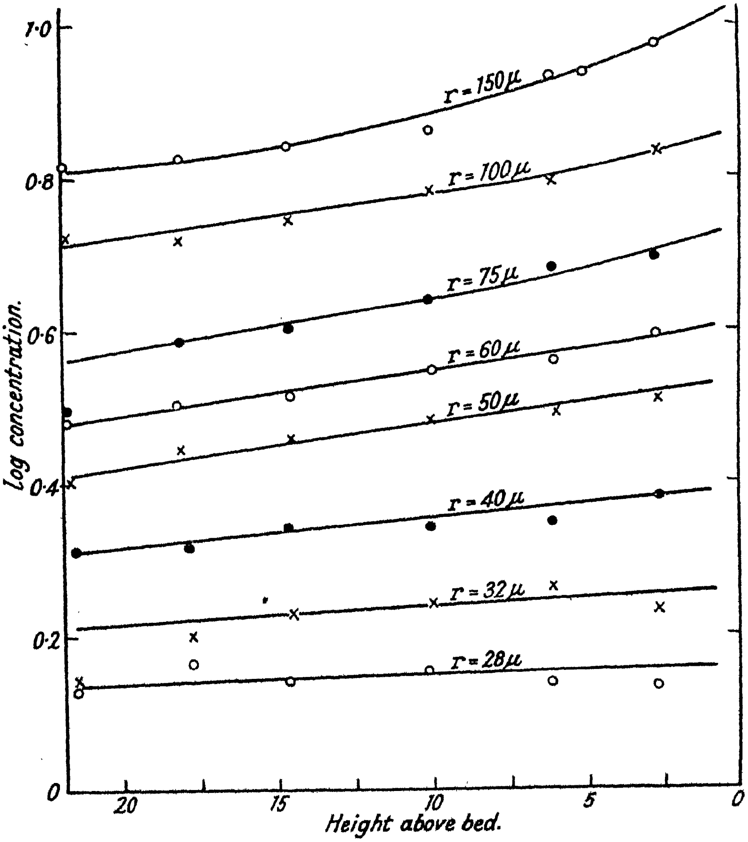

FIG. 3.—Variation in silt concentration against depth for various particles, radius (r).

draw the apparatus from the river at the warning of the approach of a vessel; on one occasion neglect to take this precaution in time led to the apparatus being driven by the wash of a steamer beneath the piers of the bridge from which it could not be rescued—minus cell—until low tide; but eventually some uninterrupted series were obtained.) Measurements with this apparatus, correlated with velocity measurements, appear on the same traverse (y) on Fig. 4. Again the logarithmic law is approximately upheld to within a few feet of the bed of the river, which was 20 ft. deep at the time. The values very close to the surface are not included as they were affected by scattered daylight entering the apparatus.

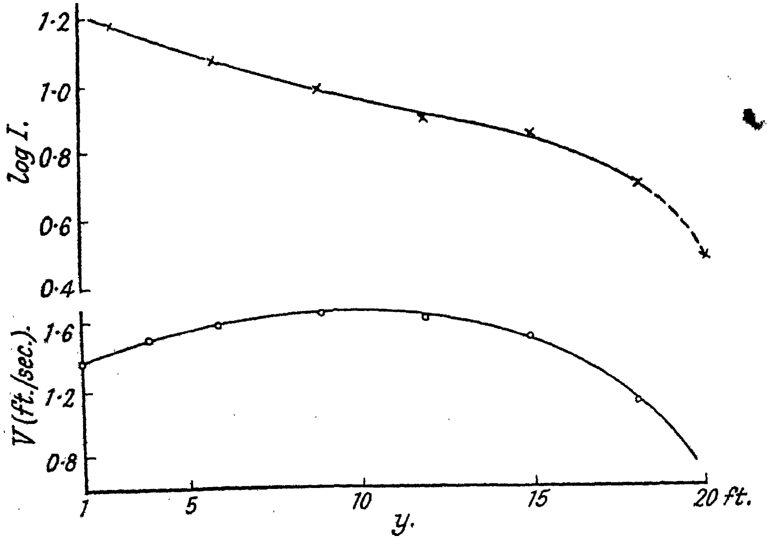

Fig. 4.—Silt concentrations (*above*) and velocities (*below*) on vertical section of the river Tyne.

The work of the engineer demands rather a knowledge of the total silt load carried by a stream past a certain point from time to time. This information can be derived with sufficient approximation for his purpose by the use of the two pieces of apparatus just cited, viz. (*a*) a current meter set where it is likely to record the mean current and (*b*) a pair of turbidimeters set at two levels in the stream, say, at depths of one-third and two-thirds of the total depth respectively. Two assumptions must be made (1) that the current meter reads the mean velocity at any time; for this purpose it is probably best to set the meter at one-third of the total depth; (2) that the silt distribution is exponential; the coefficient required to derive the total silt load from the reading of one of the turbidimeters is then known. This, multiplied by the mean current, then gives the silt transported past the observation station in unit time. The photoelectric cells will have been previously calibrated so as to read the actual mass of solid within the confines of the light beam at any instant. By the use of a number of such stations in the streams serving a watershed, it should be possible to correlate rainfall and soil conditions on the watershed with the daily amount of material eroded from it.

These points concern not only the river and harbour engineer but also the farmer. It is a matter of common knowledge that large tracts of farming land have been permanently lost to cultivation in Africa and America by over-cultivation and failure to consolidate the land. Manure not only enriches the soil but in the form of organic humus helps it to resist erosion. The extent to which different climatic and soil conditions affect erosion is only now being studied on a quantitative basis. For instance, some agriculturists think that the presence of fine colloidal material among the grosser soil crumbs helps to resist erosion, whereas others claim to find equal losses for soils exposed to the same weather but having widely differing colloid contents. Burning scrub to free an area for subsequent cultivation—a practice among the natives of Africa—is, however, generally agreed to be a pernicious usage. Fortunately the problem does not worry the British farmer at the moment, but, in the United States, work on the measurement of erosion, with or without the exercise of preventive measures, is a pressing need and is being taken up by the Soil Conservation Service of the Department of Agriculture in experimental projects all over the country.

These are in the main of two types. In the one the rainfall on straight slopes is correlated with the retained soil moisture and the amount of silt removed by it; in the other the run-off from a specially fashioned terrain is correlated with the rainfall on the whole of a small watershed of which it forms a part, in order that the efficacy of schemes for soil conservation may be studied. One of the most up-to-date of the former stations is to be found at Marlborough, New Jersey. The author had the privilege of inspecting the work there in the summer of 1938. Unfortunately, his visit preceded by one day the arrival of the great hurricane which broke several weeks of low rainfall; a day or two later the run-off would have been more spectacular, though less comfortable to scrutinise. Here a field is divided up into strips running down a gentle slope to the water and soil catchment bins. At the bottom of the slope a 6-in. brick course resting on a metal sill runs right across and leads to a large rectangular tank. At the far side of this tank the upper portion of the water overflows through a Gieb diviser to another circular tank. The diviser is merely a set of vertical slots of equal area and of any odd number between 5 and 11 according to circumstances, of which all but the centre one lead to waste. The silt sample from the remaining slot settles in the second tank, where it is collected and weighed after every storm potent enough to cause run-off. Fig. 5 shows several strips at the Beemerville Station located at Sussex, N.J., and the apparatus at the bottom of one of them. Fig. 6 shows the same apparatus from above with the lid of the first tank removed and some of the slots of the diviser made visible by the light shining through. The object of the first tank is to catch the heavy particles of grit, sand, etc., before the silt passes on, since from the farmer's point of view it is the loss of fine silt and colloidal material that is detrimental to the soil; the grit need not be reckoned. (One should add that the soil in this part of New Jersey is a friable sandy loam, easily penetrated by rain, which becomes a network of little gulleys after a light rainfall wherever it has no crop cover to protect it; *cf.* Fig. 7.)

The strips on the station vary in both length and pre-treatment. Some have been manured while others are practically virgin soil. Some have had a cover crop, subsequently dug in. Yet others have had both treatments. The acidity of the soils have been reduced to the same value (pH = 6·5) by liming. The experiments are in duplicate and there are "dummy" strips left between the experimental ones, which are cut off by low metal barriers from infiltration from the sides (*cf.* Fig. 5). It must not be assumed that if the length of a plot is doubled twice as much run-off will ensue. The water falling higher up the slope may sink in and not cause so much erosion as if it had but a short distance to trickle down to the tanks. Naturally the prevailing moisture content of the soil before the onset of further rain materially influences the amount of damage done. To keep a record of this, moisture meters of the Richards type are embedded in the soil at the upper end of several plots. These consist of 6-in. cups of light porous clay which swell or contract as the surrounding earth changes its moisture content. These tensiometers are connected to pressure gauges above the ground in the form of simple liquid manometers or automatic and continuously recording aneroids. The research has not yet proceeded long enough to enable results to be reported, but it is expected that after a few years' working, such stations will give the first detailed statistics of "sheet" erosion such as occurs on gently sloping agricultural land, as opposed to that more persistent erosion encountered in rivers and canals.

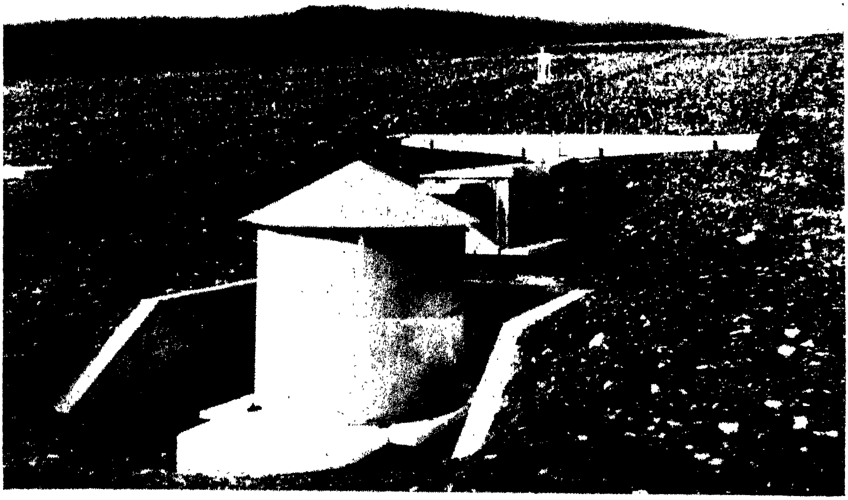

FIG. 5.

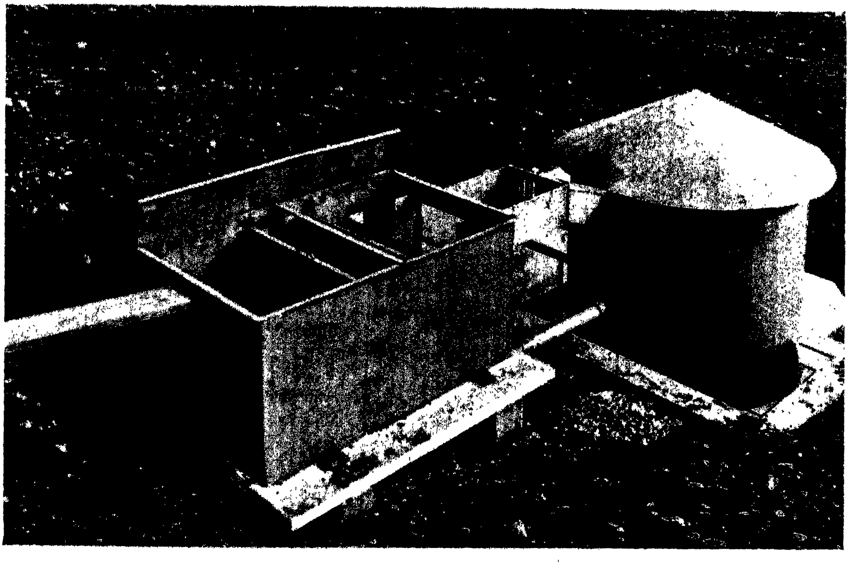

FIG. 6.
Silt meters on experimental plots, New Jersey.

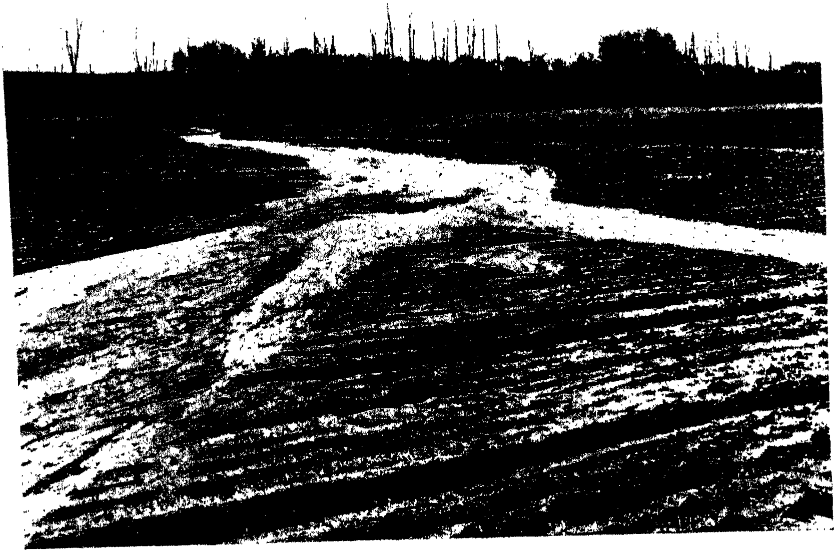

FIG. 7.—Sheet erosion, New Jersey.

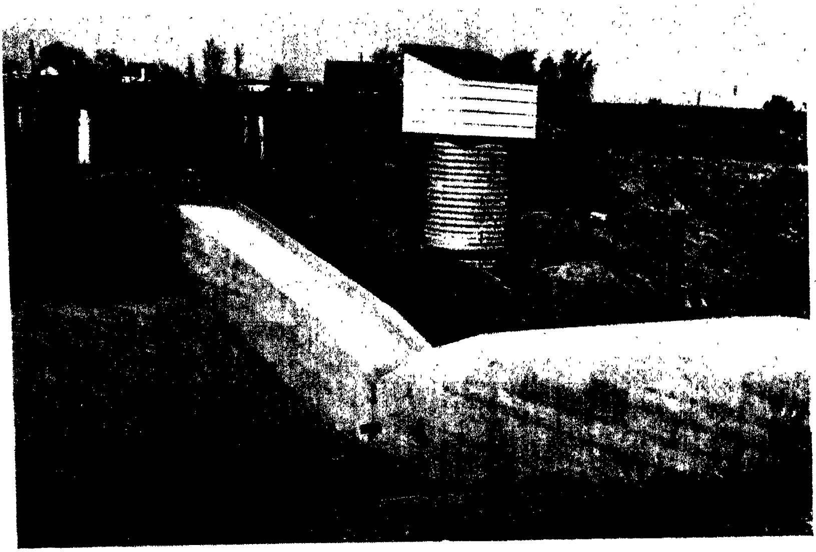

FIG. 8.—Weir and run-off meter, New Jersey.

The second type of soil conservation project—of which a recent example is to be seen at Freehold, N.J.—is concerned with the measurement of run-off from a number of watersheds of moderate size (*c.* 100 acres). Formerly estimates were made in relation to a large watershed in which a variety of conditions existed, *e.g.* that of the Missouri, or else from an artificial and stereotyped plot of laboratory size, just an acre or two. The object of this new research is to measure the effect on the watershed of schemes to improve the water-retaining capacity of the land and to protect it against erosion. All the water running off the land must be made to pass down through a single channel wherein a concrete weir is erected. Some of these weirs have a simple notch with sides inclined at constant angles, but in others the inclination of the sides of the V increases towards the vertex in order to increase the sensitivity of the instruments when recording low rates of flow (Fig. 8). On the up-stream side of the weir an instrument is housed which gives a continuous record of the position of a float on the surface of the water. A weir of the same shape will have been previously calibrated in a hydraulic laboratory so that the current over the weir corresponding to each position of the float is known. A number of rain gauges are dotted about the watershed in order that run-off may be compared with precipitation. At present only water losses are measured at this station, but it is proposed later to install photoelectric turbidimeters in the stream just above the notches so that silt loss from time to time may be recorded as well.

It is intended to collect data from these stations—which are more natural than the laboratory-like strips of the first projects, though each gives its quota of scientific information—which shall be of use in advising farmers confronted with losses on soils and terrains similar to one or other of the controlled watersheds. Only four amelioration schemes are being studied at the Freehold Project, but large numbers are distributed up and down the States, covering all types and sizes up to that of the largest farm or ranch.

The chief practices adopted to combat sheet erosion on farm land comprise (1) the planting of cover crops when the field would otherwise be lying fallow, (2) strip cultivation, (3) ploughing along the contour, with terracing where necessary. Erosion is most active where there is nothing to break the flow of water or the passage of wind, so that the full shearing action of the fluid is felt on the surface of the soil itself. Thickly sown grass or some similar plant will impede the velocity of flow over the ground itself; then the paramount fall in velocity which gives rise to the shear is experienced by the upper parts of the stems of the crop where it cannot do much harm. At the same time, the water on the ground itself cannot run away so fast and stands a greater chance of percolating to the roots which assist the conservation process by holding the soil together. If, however, the plants are tall and form dense undergrowth the plots may become waterlogged and suffer from a superfluity of moisture. What the engineer requires is a scheme which will keep the shearing force on the ground small and allow moderate percolation while not impeding unduly the run-off above after a heavy storm. In a river this may be done by erecting a series of check dams or shallow weirs at short intervals. In this way the stream is let down a sort of gentle staircase. So the main flow is unobstructed, while the water on the bed is almost stagnated. Such devices are employed on a number of alpine rivers on the Continent of Europe.

The second palliative for sheet erosion, that of strip farming, is intended to protect land under more open crops such as roots and tubers, by interplanting with alternate strips of a better cover such as grass or corn. It has at best but a partial success and certainly is useless on steep fields. In the latter case, it is better to throw up shallow terraces along the contours of the hill, and plough in the space between so that the furrows likewise run along the contour. Towards the end of the last century, Priestly Mangum introduced the broad shallow type of terrace which is named after him. It is about 20 ft. broad and a foot high in front, being made by throwing up a levee of earth to direct run-off to the ends of the field where that which has escaped absorption runs away in drains. Another type of terrace, the Nichols terrace, has a definite ditch on the uphill side of the levee and produces superior drainage but inferior percolation to the Mangum terrace. Both, of course, conserve the soil in preventing rapid drainage directly downhill. The terraces are cut by a special rotary disc plough, which throws all the earth removed from the ground to one side thus making mound and ditch in one in the same operation. At the experimental hydrology stations which we have described all the effluent from terraces is naturally passed to the single outlet, over the weir.

The economics of the amelioration schemes as carried out in the United States are interesting. The experimental grounds on which the plots are located are not owned by the State in which they lie. A co-operative agreement is drawn up between the State and the farmer, by which he consents to follow a schedule of cultivation for a number of years (at least five), to admit officers of the Soil Conservation Service when they require to attend to the instruments and to use only ferilisers prescribed by the Department. In return he gets the services of their engineers for making terraces, drainage culverts and every appurtenance of the system of erosion control and measurement which they deem necessary for his land. Most of the unskilled labour for these constructions is supplied from the camps of lads which under the name of Civilian Conservation Corps have been set up in many localities to take on public works and to help to relieve unemployment. Their services are available without charge to the officers of the Soil Conservation Service and apparently are paid for by the Federal Government. The farmer, of course, supplies the necessary seed to fulfil the scheme and reaps the crops himself but there is often a clause inserted in the contract to prohibit autumn ploughing as erosion is rife at that season and downhill furrows particularly will aggravate it.

Figs. 1 and 4 are reproduced by permission of the Royal Society; Figs. 5–8 by permission of the U.S. Department of Agriculture, Soil Conservation Service. I am greatly indebted to Messrs. Barnhardt, Knoblauch and Krimgold of the Soil Conservation Service for taking me on a tour of inspection of the New Jersey Projects in September last and for supplying the photographs reproduced in this article; and finally, to the Research Committee of King's College, Newcastle, for a grant which made this visit possible.

RECENT ADVANCES IN SCIENCE

MATHEMATICS. By J. H. C. WHITEHEAD, M.A., Balliol College, Oxford.

THE FOUNDATIONS OF DIFFERENTIAL GEOMETRY : II.—The first of these two articles contained a brief account of the origins and nature of Riemannian geometry. Among other things I tried to explain as clearly as possible the radical change in philosophic ideas about geometry due to the discovery of Relativity. What may be called the Euclid-Klein concept of geometry, that is the study of equivalence under a given group of transformations, was replaced by the idea that a space has an intrinsic structure, consisting of a set of relations which may be, but in general are not, defined in terms of a transformation group. In the case of Riemannian geometry this structure is determined by a quadratic differential form, whose coefficients are subject to a certain transformation law when the co-ordinates are changed. Thus the formal basis of Riemannian geometry is a set of functions $g_{ij}(x^1, \ldots, x^n)$ associated with co-ordinates $x^1, \ldots, x^n$, together with a rule by which one passes to the corresponding functions $h_{ab}(y^1, \ldots y^n)$ associated with co-ordinates $y^1, \ldots, y^n$. We recall that this transformation law is

$$h_{ab} = g_{ij}\frac{\partial x^i}{\partial y^a}\frac{\partial x^j}{\partial y^b},$$

where the repeated indices imply summation for $i, j = 1, \ldots, n$. The purpose of this article is to describe other geometries based on the same idea, namely a set of functions associated with each co-ordinate system, the sets corresponding to two given co-ordinate systems being related in a way which is describable by some mathematical formula.

The first geometries of this kind to be discovered were all natural generalisations of familiar geometries, in the same way that Riemannian geometry is a generalisation of Euclidean geometry. The simplest of them, which is even simpler than Riemannian geometry, is the generalised affine geometry. The classical or " flat " n-dimensional affine geometry may be described as n-dimensional projective geometry with a special $(n - 1)$-plane " at infinity," but no special "quadric at infinity." Thus everything in Euclidean geometry which depends only on notions of linearity and parallelism belongs to affine geometry, but nothing which involves distance, except that lengths may be compared on the same or on two parallel lines. In much the same way the theorems of Riemannian geometry can be divided into those which depend on the metric itself and those which depend only on the functions

$$(1)\quad \Gamma^i_{jk} = \tfrac{1}{2}g^{ip}(\partial_j g_{pk} + \partial_k g_{jp} - \partial_p g_{jk}),$$

where ∂_i stands for the operator $\partial/\partial x^i$, and on their transformation law, which is given in (3) below. The latter theorems belong to generalised affine geometry. They include theorems on the incidence properties of geodesics and the theorem that the geodesics through a given point are given by equations of the form

$$(2)\quad x^i = f^i(y^1, \ldots, y^n),$$

where $y^i = p^i t$ and $p^1, \ldots, p^n$ are constant on each geodesic. More generally (generalised) affine geometry is the theory of an arbitrary *affine connection*, which is given in each co-ordinate system by a set of functions Γ^i_{jk}, called its *components*, with the transformation law

$$(3)\quad \bar{\Gamma}^a_{bc} = \left(\Gamma^i_{jk}\frac{\partial x^j}{\partial \bar{x}^b}\frac{\partial x^k}{\partial \bar{x}^c} + \frac{\partial^2 x^i}{\partial \bar{x}^b \partial \bar{x}^c}\right)\frac{\partial \bar{x}^a}{\partial x^i},$$

where Γ^i_{jk} and $\bar{\Gamma}^a_{bc}$ are its components in co-ordinates $x^1, \ldots, x^n$ and $\bar{x}^1, \ldots, \bar{x}^n$ respectively. It may be verified that the equations (1) are equivalent to

$$(4)\quad \partial_k g_{ij} - g_{ip}\Gamma^p_{jk} - g_{pj}\Gamma^p_{ik} = 0.$$

If we start with arbitrary functions Γ^i_{jk} these may be regarded as differential equations in which g_{ij} are the unknowns. In general the equations (4) have no solution. Therefore the generalised affine geometry is more general than Riemannian geometry, since the theory of a particular affine connection may or may not be the affine, or non-metric part of a Riemannian geometry. The formal development of the theory consists largely of the definition and classification of tensor invariants. The fundamental tensor in affine geometry is the curvature tensor, and other tensors may be obtained from this by co-variant differentiation and similar processes.

These theories depend for their geometric content mainly on the properties of the geodesics, or paths as they are usually called in affine geometry, and on parallel displacement, which was discovered by T. Levi-Civita in 1917 and, independently, by J. A. Schouten in 1918. If C is a curve given by parametric equations

$x^i = x^i(t)$, the parallel displacement of a vector $\xi(t)$ along the curve C is defined by the equations

$$(5)\quad \frac{d\xi^i}{dt} + \xi^j \Gamma^i_{jk} \frac{dx^k}{dt} = 0,$$

where the functions $x^i(t)$ are substituted for the variables x^i in the functions Γ^i_{jk}. It is easily verified, first that the solutions of these equations corresponding to given initial values are not altered by a transformation to some other parameter $s = s(t)$; secondly, because of the transformation law (3), that if any set of solutions are treated as the components of a contra-variant vector, the quantities represented by the left hand side of (5) are also the components of a contra-variant vector, and hence that (5) are invariant equations; and finally that any set of solutions vary linearly with their values at any given point. Thus they may be written in the form

$$\xi^i(Q) = B^i_j(Q, P)\xi^j(P),$$

where P and Q are given points on the curve and $B^i_j(P, P) = \delta^i_j$ ($= 1$ or 0 according as $i = j$ or $i \neq j$). The vectors $\xi(P)$ and $\xi(Q)$ are said to be *parallel with respect to the curve* C. If P, Q, R are any points on C this parallelism, always with respect to the curve C, satisfies the familiar conditions;

(i) *if* $\xi(P)$ *is parallel to* $\xi(Q)$ *and* $\xi(Q)$ *is parallel to* $\xi(R)$, *then* $\xi(P)$ *is parallel to* $\xi(R)$,

(ii) *if* $\xi(P)$ *is parallel to* $\xi(Q)$, *then* $\xi(Q)$ *is parallel to* $\xi(P)$,

which may be expressed formally as;

(i) $B^i_j(R, Q)B^j_k(Q, P) = B^i_k(R, P)$,

whence, writing $R = P$, we have

(ii) $B^i_j(P, Q)B^j_k(Q, P) = \delta^i_k$.

In this last equation it must be understood that the parallel displacement from Q to P is back along the same arc as in the displacement from P to Q. When displaced round a closed curve back to its starting point a vector will not, in general, return with its initial value. If some vector at a point P does return with its initial value when displaced round every closed curve beginning and ending at P, then the partial differential equations

(6) $\partial_k \xi^i + \xi^j \Gamma^i_{jk} = 0$

have a set of solutions. A set of solutions are the components of a *parallel field* of vectors, which consists of a vector $\xi(P)$, defined at each point P, the vectors $\xi(P)$ and $\xi(Q)$ being parallel with respect to every curve joining P to Q. If the equations (6) are completely integrable, meaning that they have solutions satisfying given initial conditions, they can be solved to give n linearly independent parallel fields. Then to every vector $\xi(\mathrm{P})$ corresponds a unique parallel vector $\xi(\mathrm{Q})$.

Associated with the equations (5) are the equations

$$\frac{du_j}{dt} - u_i\Gamma^i_{jk}\frac{dx^k}{dt} = 0$$

for the parallel displacement of a co-variant vector along a given curve, and to (6) correspond the partial differential equations

$$(7)\ \partial_k u_j - u_i\Gamma^i_{jk} = 0.$$

If the equations (6) are completely integrable so are the equations (7). If also $\Gamma^i_{jk} = \Gamma^i_{kj}$ we have $\partial_k u_j = \partial_j u_k$ and it follows that the partial differential equations $\partial_j y = u_j$ can be solved for y, whence

$$(8)\ \frac{\partial^2 y}{\partial x^j \partial x^k} - \frac{\partial y}{\partial x^i}\Gamma^i_{jk} = 0.$$

If $y^1, \ldots, y^n$ are solutions of these equations such that the Jacobian $|\partial_j y^a|$ is not zero, the equations $y^a = y^a(x^1, \ldots, x^n)$ determine a transformation to co-ordinates in which all the components of the affine connection are zero. In this case the connection is said to be "flat," and co-ordinates in which all its components vanish are called Cartesian. The Cartesian co-ordinate systems are those, and only those, which are related to a given one by linear transformations. Therefore the local geometry of a flat affine connection is the classical affine geometry. For this to be the case it is necessary and sufficient that

$$(a)\ \Gamma^i_{jk} = \Gamma^i_{kj}, \qquad (b)\ \mathrm{B}^i_{jkl} = 0,$$

where B^i_{jkl} are the components of the curvature tensor, given by

$$\mathrm{B}^i_{jkl} = \partial_l\Gamma^i_{jk} - \partial_k\Gamma^i_{jl} + \Gamma^i_{pl}\Gamma^p_{jk} - \Gamma^i_{pk}\Gamma^p_{jl}.$$

For these are the integrability conditions of the equations (8).

In theorems such as the one just described it is necessary to insist on the local nature of the result. For two essentially different spaces may have the same differential geometry, as in the case of a cylinder and a plane. What is more interesting is that there are closed surfaces with a locally Euclidean geometry. Such a surface, in elliptic 3-space, was discovered by W. K. Clifford in connection with the "Clifford parallelism" described in an earlier article (on the theory of groups). It is easy to give a similar example in Euclidean 4-space, referred to rectangular Cartesian co-ordinates x, y, z, t, namely the surface given by

$$\mathrm{X} = e^{iu}, \qquad \mathrm{Y} = e^{iv},$$

where

$$\mathrm{X} = x + iy, \quad \mathrm{Y} = z + it.$$

The idea of displacement leads to a more general and more picturesque definition of a "generalised geometry," which is, however, more difficult to state precisely. Let G stand for some kind of geometry (*e.g.* Euclidean, affine, projective or elliptic) and let M be an n-dimensional manifold. With each point P in M we associate an n-dimensional space S(P) with the geometry G. In the case of affine or Riemannian geometry S(P) is the "tangent space," consisting of all the contra-variant vectors at P, with the Euclidean metric determined by the tensor g_{ij} if M is Riemannian, G then being Euclidean geometry. In generalised projective geometry S(P) is an n-dimensional projective space, and similarly for the other kinds of geometry. The spaces S(P) are to be related to M in such a way as to justify the remark that the geometry of M near P is approximately that of S(P). In affine geometry this relation is inherent in the fact that the transformations of co-ordinates are defined by functions with continuous derivatives, and are therefore linear to a first approximation. In general the exact definition of this relation is a difficult problem which, in the case of projective geometry for example, was solved only after many years of trial and error.

We now require that, to each oriented arc PQ, in M, shall correspond a transformation of S(P) into S(Q), usually defined in terms of an invariant set of differential equations, which is to be an isomorphism with respect to the geometry G. As in the parallel displacement the transformation S(Q)→S(P), determined by the same arc with the opposite orientation, is to be the inverse of S(P)→S(Q), and if two arcs PQ and QR are laid end to end the transformation S(P)→S(R), determined by the arc PQ + QR, is to be the resultant of S(P)→S(Q) followed by S(Q)→S(R). In particular, to each closed curve beginning and ending at P, there corresponds an automorphism of the space S(P). The set of all such automorphisms is a group, called the *holonomic group*. In supplying the details of this very general and rather vague outline we can give the scheme various twists. One can, for example, proceed along the lines of parallel displacement, imagining the space S(P) to be "carried along" the arc PQ into the space S(Q). But one can also start from a concept which is the basis of E. Cartan's work in this field. He imagines an observer at each point P, and what I have called a displacement he takes to be a process by which the observer at P telegraphs his co-ordinates back to headquarters at O, along a curve PO. If the holonomic group consists of the identical transformation alone the message will be independent of the arc PO. In any case, each point in M may be joined to O by a unique curve, and headquarters can therefore map each space S(P) in S(O). In particular, it can pin-prick the position of each observer. In general, and here the relation between S(P) and M is the crux of the matter, conflicting accounts of their "infinitesimal neighbourhoods" will be received from near-by observers P and Q. But if the holonomic group reduces to the identity, headquarters will receive a coherent account of each neighbourhood and may therefore identify M with S(O). The geometry of M is then G itself.

The preceding paragraph may seem a little clearer if, by way of example, we apply the two processes to plane geometry. Here each space S(P) may be identified with M, natural co-ordinates (X_P, Y_P) for S(P), with their origin at P, being obtained by a translation from Cartesian co-ordinates (x, y) for M. The equations for parallel displacement, namely $dX = dY = 0$, lead, naturally enough, to the equations $X_Q = X_P$, $Y_Q = Y_P$, relating parallel vectors at P and Q. On the other hand the equations

$$dX + dx = dY + dy = 0$$

lead to

$$X_P = X_O - x, \; Y_P = Y_O - x,$$

where O is the origin of the co-ordinates (x, y). Writing $X_P = Y_P = 0$ we have the equations $X_O = x$, $Y_O = y$, which express the identity of M and S(O).

Generalised geometries of this type, other than Riemannian geometry, were first studied by G. Hessenberg (*Math. Annalen*, **78** (1918), 199) and H. Weyl (*Math. Zeit.*, **2** (1918), 384–411). Weyl made effective use of affine geometry in producing the first unified field theory, that is to say a purely geometrical description of the combined gravitational and electro-magnetic fields. In such a geometry there must be an invariant cone at each point, corresponding to the light cone, and a co-variant vector (the vector potential), to which may be added an arbitrary gradient. Weyl's theory was based upon an affine connection for which, not the equations (4), but the equations

$$g_{ij,k} = \psi_k g_{ij}$$

have a solution, with $|g_{ij}| \neq 0$, where $g_{ij,k}$ stands for the left-hand side of (4) and ψ_k is the vector potential. The tensor g_{ij} may be replaced by ϱg_{ij}, where ϱ is any scalar function, which has the effect of replacing ψ_k by $\psi_k + \partial_k \log \varrho$. Though of first-rate importance both to mathematics and to physics, Weyl's geometry did not explain all the observed facts, and many alternatives have since been proposed. The importance of generalised projective geometry for these theories may be seen from O. Veblen's monograph *Projektive Relativitätstheorie* (Berlin, 1935), and from recent papers by J. A. Schouten, D. van Dantzig and others.

The purely mathematical theories described here were conceived and worked out mainly by Weyl, Cartan, Veblen, T. Y. Thomas, L. P. Eisenhart and others of the Princeton school, and by J. A. Schouten, D. van Dantzig and their collaborators and pupils. These two articles are based on a Cambridge Tract, *The Foundations of Differential Geometry*, by Veblen and myself (Cambridge, 1932), which is a companion to Veblen's tract, *The Invariants of Quadratic Differential Forms* (Cambridge, 2nd edition, 1933). A detailed account of some of the geometries described here is to be found in the book, *Differential Invariants of Generalised Spaces* (Cambridge, 1934) by T. Y. Thomas.

ASTRONOMY. By A. Hunter, Ph.D., F.R.A.S., Royal Observatory, Greenwich.

The Sun and the Weather.—The possibility of a correlation between solar conditions and terrestrial weather has recently been discussed from two points of view in Science Progress (XXXIII, pp. 15 and 81, July, 1938). The complexity of the problem has been recently emphasised by the publication of two papers bearing on the two aspects of the subject.

The first concerns the relation between solar activity and the widths of tree rings. In the *Monthly Weather Review*, Washington, **65**, 175, 1937, F. P. Keen reports on " Climatic Cycles in Eastern Oregon as indicated by Tree Rings." The work was undertaken primarily for entomological purposes, to provide some indication of the trend of meteorological conditions in the past few centuries, but a by-product of the research is an interesting graph showing how the growth rate of the ponderosa pine has varied from the normal since A.D. 1268. This graph is based on micrometer measurements of the annual radial growth of 1240 trees taken from 44 different regions in Eastern Oregon and Northern California. By analysing data collected from such a wide area, the local effects of insect defoliation and forest fires can be at least partially eliminated.

The annual growth of a tree depends largely on temperature and precipitation. In the semi-arid region studied, mean annual temperatures fluctuate only slightly, and the extremely variable precipitation becomes the dominating influence. It is not surprising, then, that a coefficient of correlation of $+ 0{\cdot}50 \pm 0{\cdot}09$ is found between fluctuations in annual ring-width on the one hand and differences in seasonal rainfall on the other. This correlation extends back, of course, only to the beginnings of meteorological observations in this region some seventy years ago, but the coefficient is significant enough to make it possible to use the rings with confidence for extrapolating back still further.

An analysis of the fluctuations in ring-width shows no evidence for the existence of predictable cycles. The fluctuations cannot be effaced by plotting smoothed averages over five, eleven and twenty-three years, and it is concluded that, as far as can be determined at the present time, there is nothing to indicate that any apparent cycles are more than chance variations. The implication is, of course, that the rainfall, too, shows no detectable tendency to vary in the sunspot cycle, at least since 1500, when the observations can be said to become fairly reliable.

The discrepancy between this work and that of Douglass (*Carnegie Inst. Wash.*, Publ. 289, **1**, 1919; **2**, 1928), who finds a definite periodicity in ring-widths, remains to be explained. It illustrates yet again the complexity of the problem when two such careful pieces of work can point to diametrically opposite results. There is this, however, to be said: in Keen's work, the author expresses his opinion that the predominant variable is the rainfall. Now it is known from more direct evidence that there exists little or no tendency for rainfall, at least in temperate regions, to vary cyclically with the sunspot period. Accordingly we should not expect to find any such cyclic variation exhibited in those tree-rings whose widths are closely correlated to rainfall and to nothing else. This in turn suggests that the Douglass ring-widths are affected by another parameter which *is* a function of solar activity. What this parameter is will presumably not emerge until the two sets of data are put on a comparative basis. Or again, it may be that the Douglass trees are, so to speak, epitomes of their climatic environment—sunshine, rainfall, temperature, cloudiness, wind, ultra-violet radiation, etc., in each of which, taken separately, fluctuations in the sunspot cycle are masked by random variations. These random variations, not being common to all, are diminished in the mean, whereas the tendency to exhibit the sunspot cycle is enhanced. In this case, of course, Keen's measurements of his tree-rings would be no more successful in detecting the solar cycle than the routine observations of the separate meteorological factors themselves. Here again, we need comparative observations in different regions to settle the matter. Meanwhile, it is certain that the sunspot cycle cannot at present be used to *predict* weather changes of any sort.

The subject of the other paper is the "solar constant," *i.e.* the intensity of the sun's energy radiation at the mean distance of the earth. The term is to some extent a misnomer, since the quantity measured appears to vary somewhat.

The experimental work necessary for the determination of the solar constant demands extreme care on account of the many disturbing factors involved. The measurement falls into two parts: first, the determination of the rate at which energy falls on unit area of the earth's surface; and second, the determination of the amount of energy abstracted by the atmosphere. It is this second determination which offers the main problem. The importance of the correction will be realised from the fact that only about 70 per cent. of the initial radiation gets through to sea-level even at noon on the equator. It will be noticed that the first process measures the integrated energy over the whole spectrum; the instrument employed, the pyrheliometer, utilises a beam of sunlight of known cross-section, which is completely absorbed by a blackened silver disc whose rate of rise of temperature is then noted. After allowance has been made for the effects of conduction, convection and re-radiation, this quantity, in conjunction with the thermal capacity of the receiver, will yield the value required. The correction measured by the second process, however, cannot be determined *en bloc*, because the absorption coefficient of the atmosphere is a function of wave-length. Accordingly, a spectrobolometer is used to measure, wave-length by wave-length, the energy received when the sun is at various altitudes, *i.e.* when the absorption is due to varying lengths of path traversed by the light. A process of integration then gives the total depletion for which the atmosphere is to be held accountable at the particular time at which the pyrheliometer measurement was made.

Measurements of this sort are now carried out by the Smithsonian Institution at three stations selected for their high altitude and freedom from water-vapour and dust: Table Mountain, California, Mount Montezuma, Chile, and Mount St. Catherine, Egypt. From intercomparison of the results obtained at these stations and other similar ones used during the past thirty-five years, Abbot (*Smithson. Misc. Coll.*, **94**, Nos. 10 and 12, 1935) claims that the solar constant exhibits variations analysable into twelve periods, the most conspicuous of which is one of twenty-three years. On these cyclic variations may be superposed random variations in which the value alters by 3 or 4 per cent. in the course of a few days. Many of these periods are also found by Abbot in the temperature figures for Washington, Berlin, Adelaide, Cape Town, and Greenwich.

In a recent paper (*Quarterly Journ. Roy. Met. Soc.*, **64**, 459, 1938), however, Miss Paranjpe examines statistically the data on which Abbot bases his claims, and concludes that there is no direct evidence of any variation in the solar constant, that the periodicities in the observed values are neither mathematically nor physically significant, and that the computations fail to indicate any dependence of weather changes on the observed variations. The evidence she adduces is weighty. For differentiating between errors of observation or computation and real variations of the solar constant, observations made at the various stations scattered over the earth's surface are compared statistically. The correlation coefficients obtained between the observations on days when atmospheric conditions were described as favourable are so small that no common cause, such as changes in the solar constant itself, suggests itself as controlling the variations over all the stations. Where a high correlation coefficient is found, it is traced to an illegitimate process of "correcting" the results from one station when they fail to agree with those from another. Abbot's most recent results are based on visual inspection of the curves from the various stations. Statistical treatment of the data on which these curves are based yields a correlation coefficient less than its own probable error. In the same way, Abbot's method of searching for periodicity in the solar constant variations and in the meteorological data is criticised as being far from precise. Moreover, the removal of his periodicities from the observed values makes no significant difference to the standard deviation.

These conclusions cannot, of course, detract from the value of the observational results. It is only the interpretation of the variations which is in doubt. In this connection it is interesting to notice that both the probable error and the percentage scatter of the results have decreased continuously since the earliest observations, except for occasions where such phenomena as volcanic eruptions have caused understandable variations in atmospheric transmissibility. This decrease is no doubt due to the advances made by Abbot and his co-workers in solving the very difficult technical problems with which the observations are beset. Whether or not the residual variations are real or instrumental is evidently still a matter for controversy. It cannot be denied, however, that early hopes that long-range weather forecasting may become possible through measurements of the solar constant, are becoming less and less likely of fulfilment.

THE SUPERGIANT ε AURIGÆ.—Variability in stellar brightness is a far more widespread phenomenon than was appreciated even a score of years ago. It is not, however, an overstatement to say that where true variability is concerned, none of the many types of light variation, periodic or irregular, has been really satisfactorily explained. That is not to say that we know nothing about the variations: for instance, the connection between period and luminosity in cluster variables is now well established, and has indeed proved our most powerful means of probing intergalactic space. For one particular kind of variability, however, a fully satisfactory theory has been developed, explaining adequately both the character and the periodicity of the variation; but here, oddly enough, the theory is based on the paradoxical assumption that no true physical variation is involved at all. The variables referred to are the *eclipsing binaries*. These are simply double stars whose components happen to be moving in a plane passing through or nearly through the earth. Some 300 of these objects are now known; all exhibit variability such as could be produced by the mutual eclipses, total or partial, of a pair of stars, usually different in size and brightness but not variable in themselves, revolving around their common centre of gravity according to ordinary dynamical principles. The correctness of this explanation would seem to be clinched when it is remarked that every eclipsing variable which has been observed spectroscopically has turned out to be a spectroscopic binary whose radial velocity varies in a period equal to that of the light variation. Naturally, not a great proportion of spectroscopic binaries will vary in brightness, because the accident which places their orbits edge-on to the earth will be of infrequent occurrence.

Using this theory of the light variation of these stars, an astonishing amount of information has been derived from a mathematical study of their light curves. By making a few plausible assumptions, we can derive the sizes of the components in terms of the orbital dimensions, the ellipticity of the orbit and its inclination to the line of sight, the luminosity of the components relative to the sun, and in certain cases the tidal distortion of the components and their "darkening at the limb," *i.e.* the amount by which their surface brightness falls off towards the edge of the disc. If spectroscopic observations are also available (they are for all but the faintest eclipsing binaries), the actual dimensions of the orbits can be obtained, and thence the diameters of the components, in miles, and their mean densities.

With such a rich haul from the study of these somewhat rare objects, it behoves us to make very sure that the assumptions underlying our theory of the variation are quite trustworthy. It would be an exaggeration to say that any real doubt has ever been raised about their validity, but the case of ε Aurigæ has always been rather a rift within the lute. The period of the star is so long that the exact nature of the variation remained in doubt until its latest minimum in 1929–30—the fourth since discovery of its variability. No fewer than 13,800 photometric observations made by 47 observers since 1842 are now available, and establish a period of 27·1 years. The periods of most eclipsing variables range from a few hours to a few hundred days ; a value of nearly 10,000 days is unique. This is, however, doubtless merely a matter of the size of the orbit : by Kepler's harmonic law, for systems of equal total mass (all stars are in fact closely similar to one another in mass), the square of the period is proportional to the cube of the orbital diameter. The long period observed for ε Aur., then, simply means a large orbit. The real trouble with the star is that the photometric observations contradict the spectroscopic ones. For about twenty-five years the visual magnitude remains steady at 3·1. It then slowly decreases in brightness for 192 days to a minimum of 4·0 mag., at which it remains for 330 days. Then its brightness returns to normal in a further 192 days, and remains there for another quarter of a century. So far, so good : the flat minimum evidently means that the eclipse is total, the components differing somewhat in size ; whilst the drop of 0·9 magnitudes (*i.e.* to a light output 0·43 times that out of eclipse) suggests that the components are comparable in luminosity. Unfortunately, in this last respect the spectroscopic observations tell quite another tale. The star is a single-spectrum binary : that is to say, the secondary component is so faint that it does not contribute any lines to the spectrum, those lines which are present simply oscillating about their mean position as the primary alternately advances and recedes, instead of doubling, as would be expected with components of comparable luminosity. There is no question here of experimental error : the two sets of data are precise and are in point-blank disagreement. The discrepancy is the more disturbing in that, taken separately, the two arguments are irreproachable : they have, in fact, been used, individually and in conjunction, in hundreds of cases, and on them depends much of our knowledge of stellar duplicity and of stellar diameters and densities.

A complete explanation of this astronomical enigma has come quite recently from Yerkes (*Astrophysical Journal*, **86**, 570, 1937). The model of the system which is postulated must of necessity present some unusual features—no commonplace explanation would have been overlooked for so long—but its character is sufficiently startling for somewhat garbled accounts to have reached the non-scientific press. It consists of a small, massive F2 supergiant [1] of luminosity 60,000 times that of the sun, eclipsed every twenty-seven years by a very much larger translucent component at a temperature of only about 1300° K. and of diameter some 3000 times that of the sun. The spectroscopic and photometric results can now be brought into harmony: the large I (" infra-red ") star will give no detectable spectrum in the photographic region because of its low temperature; whilst during the one effective eclipse per revolution, the light observed is practically that of the *eclipsed* F star, the diminution in brightness being due to absorption in the I atmosphere, and not to simple removal of the light of one component. This drop in brightness is presumably the result of ionisation opacity produced in the outer shell of the I star by ultra-violet radiation from the F component. This fits with the observed flat minimum to the light curve during totality: if the photo-ionisation were produced uniformly throughout the I atmosphere, a curved minimum would result as the light passed through a series of chords of increasing and then decreasing lengths; but since the ultra-violet F radiation will be quickly absorbed in the outer shell of the hemisphere presented to the hot component, it will produce a sort of Heaviside layer, of fairly constant thickness, whose opacity to visible light will cause almost constant absorption during the total phase.

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[1] The terms giant and dwarf in astrophysics refer primarily to luminosity, not to linear dimensions nor mass. Nevertheless, great masses and diameters are often associated with high luminosities.

Direct evidence for the existence of the I component has recently been obtained (*Astrophysical Journal*, **87**, 209, 1938) from photoelectric photometry in the infra-red. At long wave-lengths the composite colour of the system is redder than that of a normal F2 supergiant, the excess energy reaching 0·13 mag. at 9600 A. Finally (*Harvard Bulletin* **908**, 11, 1938), the radiometric magnitude of the system has been measured as 2·65. The corresponding value for the F component, deduced from the visual magnitude of the whole system (the I star is ineffective visually), and the average heat index for F2 stars, is 2·91. Within the accuracy of the observations, the difference corresponds exactly to the contribution expected from the I star if its radius and effective temperature are indeed those determined at Yerkes from the photometric and spectroscopic data.

This confirmation of the postulated model will be welcomed alike by the practical astronomer, who can now draw a breath of relief at the removal of an outstanding paradox in his observational data, and by the theoretical astrophysicist, who has been presented with a direct challenge to investigate these very cool stars, whose existence has indeed long been suspected but whose properties remain practically unknown.

PHYSICS. By F. A. Vick, Ph.D., University College, London.

Transmutations of Heavy Nuclei.—Atomic disintegration experiments are among the more spectacular in modern physics, but are seldom treated as news by the daily press, even if reporters by some means get to know of them. An exception was provided early this year, when we were greeted by headlines announcing a "Gigantic Atomic Cannon Ball." The experiments provoking these headlines may be considered as members of a series, begun in 1934, on the bombardment of uranium and thorium by neutrons. To obtain a proper perspective and to appreciate the developments which have taken place, it is necessary to recall what was known about nuclear reactions involving neutrons. The production and properties of neutrons have been described recently in this journal by Dr. Feather (SCIENCE PROGRESS, **33**, 240, Oct. 1938). Strong electrostatic fields have no effect upon them, so they can penetrate into a nucleus much more easily than can charged particles. From considerable experience with various elements it was found possible to classify as follows reactions resulting from irradiation by neutrons:

Type I.—Capture of the neutron

$$X^{A}_{Z} + n^{1}_{0} \rightarrow Y^{A+1}_{Z} + \gamma$$

where the element X has atomic weight A and atomic number (nuclear charge) Z. The new element, Y, is an isotope of the old.

Type II.—Capture of the neutron and emission of a proton

$$X^{A}_{Z} + n^{1}_{0} \rightarrow Y^{A}_{Z-1} + H^{1}_{1}$$

giving an element one place lower in the periodic table.

Type III.—Capture of the neutron and the emission of an α-particle

$$X^{A}_{Z} + n^{1}_{0} \rightarrow Y^{A-3}_{Z-2} + He^{4}_{2}$$

the product being two places lower in the table. These processes are in addition to elastic scattering.

It was Bohr who pointed out (*Nature*, **137**, 344, 1936) that every nuclear process must be treated as a many-bodied problem. Each nucleus is considered to be made up of neutrons and protons held together by strong but short-range forces. The energy of a captured neutron is therefore rapidly distributed among all the particles in the nucleus. After a comparatively long time, on the average, it may happen that this energy is all concentrated on one particle, or on the group of protons and neutrons constituting an α-particle, which may then leave the nucleus. We thus have, for Types II and III:

Initial nucleus + incident neutron
→ compound nucleus (excited)
→ final nucleus + ejected particle.

If the ejected particle is a neutron, the whole process may be thought of as "scattering." Sometimes two neutrons may be emitted. However, during the comparatively long life of the compound nucleus, there may be ample time for the excess energy to be emitted as radiation. This is a reaction of Type I. The resulting isotope may be stable or radioactive. In the latter case, we have "induced" or "artificial" radioactivity, with decay, usually by the emission of an electron, until a stable nucleus is reached. The "radiative capture" type of reaction was studied extensively by Fermi and his associates. One of their observations was (*Proc. Roy. Soc.*, A, **149**, 522, 1935) that the number of nuclei transmuted was enormously increased when the source of neutrons and the irradiated material were surrounded by a substance, such as paraffin or water, containing a large proportion of hydrogen and no heavy elements. Hydrogen atoms are not disintegrated, but scatter neutrons and share their kinetic energy. The neutrons are therefore slowed down until, in the limit, they have thermal velocities only. Moon and Tillman found, in confirmation of this, that the yield of the radioactive element was influenced by the temperature of the hydrogeneous material slowing down the neutrons (*Proc. Roy. Soc.*, A, **153**, 476, 1936).

Neutrons of thermal energies can give rise to reactions of Type III also, but only with the light elements. For example:

$$\mathrm{Li}_3^6 + n_0^1 \rightarrow \mathrm{H}_1^3 + \mathrm{He}_2^4$$

$$\mathrm{B}_5^{10} + n_0^1 \rightarrow \mathrm{Li}_3^7 + \mathrm{He}_2^4.$$

These reactions owe their importance to their convenience in detecting slow neutrons. Ionisation chambers are lined with Li or B compounds, or filled with BF_3, and the ionisation produced by the products of the reactions with neutrons gives a current which can be amplified. The potential barriers of heavy nuclei are too large for α-particles to surmount unless they have large energy, so such reactions are observable only with fast neutrons. Similar considerations apply to reactions of Type II.

Among the sixty elements subjected to neutron bombardment by Fermi and his collaborators were uranium and thorium (*Proc. Roy. Soc.*, A, **146**, 483, 1934). Besides the natural UX_1 and UX_2, uranium was found to yield a series of at least four radioactive elements, distinguished by four half-value periods of decay (10 sec., 40 sec., 13 min. and 100 min.). They showed, by an investigation of chemical properties, that neither the 13 min. nor the 100 min. period element was isotopic with any element having atomic number between 82 and 92. Perhaps it should be mentioned that in order to investigate the chemical properties of a radioactive element

Y, it is not separated by itself from the target. The target is mixed, before or after bombardment, with the element or elements to which it is supposed that Y may belong. The test consists in finding which of these elements is accompanied by the activity when separated from the target. Now if we accept Types I, II and III as representing all the possible reactions between heavy nuclei and neutrons, it seems clear that the atomic number of the product cannot be far from that of U (92). Since the chemical evidence ruled out elements 82–92, it was suggested that the new radioactive substances are *trans-uranium* elements with atomic numbers greater than 92. Hahn and Meitner (*Naturwiss.*, **24**, 158, 1936) assigned the 13-min. product to Eka-Re (Z = 93), and the 100-min. product to Eka-Os (Z = 94). Later, Meitner, Hahn and Strassmann (*Z. Phys.*, **106**, 249, 1937) demonstrated the existence of at least nine radioactive periods, six of which were assigned to elements beyond uranium.

The allocation of the new atoms to these positions in the periodic table led to several difficulties. In the first place, an uncomfortably large number of nuclear isomers appeared. Two nuclei are isomeric when they have the same atomic number and mass but different periods of decay. Secondly, some of the new periods could be obtained with slow neutrons, some with fast, and some with both fast and slow, which is difficult to fit in with our reaction types. A closer investigation of the chemical properties of these elements was clearly desirable, and very careful work has been carried out in the Kaiser Wilhelm-Institut für Chemie, Berlin, in France, and elsewhere. I. Curie and P. Savitch (*J. Phys. Rad.*, **9**, 355, 1938; *C. R. Paris*, **206**, 906 and 1643, 1938) found a product from uranium which had a period of 3½ hours and the chemical properties of La (a rare earth) *not* actinium. Hahn and Strassmann followed this up, and found that a group of at least three elements formed from U were chemically similar to Ba, and therefore presumably isotopic with Ra (*Naturwiss.*, **26**, 756, 1938). Further investigation (*Naturwiss*, **27**, 11, Jan. 1939) showed, however, that it was impossible to separate these bodies from Ba, though mesothorium, an isotope of Ra, could readily be separated. They were forced to conclude, from this chemical evidence, that isotopes of Ba (Z = 56) are formed by the bombardment of U (Z = 92) with neutrons.

This is a complete break-away from the reaction types I, II and III. But it is not inconsistent with Bohr's theory of the nucleus, according to which the particles within the nucleus behave rather like molecules in a liquid drop. If sufficient energy is added to a heavy nucleus, it is conceivable that it will break up into two parts, just as a liquid drop will on violent shaking. It seems possible that the U nucleus has only a small stability of form, and may, after neutron capture, divide into two approximately equal parts. These would repel each other and gain a total kinetic energy of about 200 million electron volts (MeV), according to calculations of the loss in mass consequent on the difference between the packing fraction of U and a typical element in the middle of the periodic table. As Dr. Moon has described for us (SCIENCE PROGRESS, **33,** 645, April 1939), all known stable light nuclei have approximately equal numbers of neutrons and protons. Application of simple arithmetic shows that one or both products of the fission of U would have an excess of neutrons over protons, and would presumably be radioactive. The excess of neutrons may be removed by β-ray emission (neutron $\rightarrow$ proton) or by the " evaporation " of neutrons. In these cases it would not be necessary to invoke nuclear isomerism, because the different radioactive periods belonging to the same chemical element may then be attributed to different isotopes of that element, since varying proportions of neutrons may be given to the two parts of the U nucleus. Similar arguments would apply to the disintegration of thorium. Much of this reasoning was put forward by Meitner and Frisch in a letter to *Nature* (**143,** 239, Feb. 11, 1939), and by Bohr (*Nature*, **143,** 330, Feb. 25, 1939; *Phys. Rev.*, **55,** 418, Feb. 15, 1939), but before these communications appeared, there was clear experimental evidence, other than chemical, for the new type of nuclear reaction. M. A. Tuve describes in *Science* (**89,** 202, March 3, 1939) how, at a Conference in Washington on January 26, Bohr and Fermi discussed Hahn and Strassmann's results, and also the suggestions of Meitner and Frisch, privately communicated. The new high voltage plant in the Department of Terrestrial Magnetism at Washington was immediately used to look for the disintegration products, and they were found on January 28. It was subsequently learned that they had been detected at Johns Hopkins and Columbia Universities, in Copenhagen and in Paris. There has been abundant confirmation since. Most of the early experiments were with an ionisation chamber, a linear amplifier and a cathode ray oscillograph. (Fowler and Dodson, *Nature*, **143,** 233, Feb. 11, 1939; *Phys. Rev.*, **55,** 417, Feb. 15, 1939. Roberts, Meyer and Hafstad, *Phys. Rev.*, **55,** 416, Feb. 15, 1939. Frisch, *Nature*, **143,** 276, Feb. 18, 1939. Green and Alvarez, *Phys. Rev.*, **55,** 417, Feb. 15, 1939). For example, Roberts, Meyer and Hafstad, at Washington, placed an ionisation chamber, about 5 mm. deep, below a neutron source consisting of a target of Li which could be bombarded by deuterons accelerated by a P.D. of a million volts. A copper disc was coated with the material to be investigated and placed on the collecting electrode of the ionisation chamber, connected to an amplifier feeding a cathode ray oscillograph. The usual α-particle pulses were observed when the disc was coated with uranium oxide. On exposure to the neutron radiation, two additional groups of pulses appeared, (*a*) about four times as intense as the α-particle pulses, corresponding to " neutron-recoils " from the air in the chamber, as previously observed with the same amplifier gain but without the uranium ; (*b*) a group 20–40 times larger still and corresponding to energies of 75–150 MeV per particle released in the chamber. Similar effects were observed with Th, but not with Bi, Pb, Au or any other element tried. Paraffin round the source doubled the yield from U but not from Th.

A delightful method of investigating the products of the fission was used by F. Joliot (*C. R. Paris*, **208**, 341, Jan. 30, 1939 ; *J. Phys. Rad.*, **10**, 159, March, 1939), and suggested independently by Meitner. The products of the fission of a U nucleus are projected (in opposite directions) with high energy and can move right away from the U film. Joliot placed a source of neutrons, consisting of 700 millicuries of Ra mixed with Be powder, in a brass cylinder 2 cm. diam. and 5 cm. high, and coated externally with a thin layer of uranium oxide. Three mm. from this surface was placed a concentric cylinder of bakelite. Preliminary experiments showed that no activity was produced in the bakelite in the absence of the U. With U present, after a given time of irradiation the bakelite was removed, slipped over a Gerger-Müller counter, and showed the presence of radioactive atoms which had been projected across the 3 mm. air gap from the uranium layer. The decay periods of these atoms were investigated. Similar results were observed with Th, including a period of about 3½ hours, which Curie and Savitch suggested (*C. R. Paris*, **208**, 343, Jan. 30, 1939) is due to the same rare earth isotope as the corresponding period in the uranium product. Meitner and Frisch, in Copenhagen, used the same general method, the collecting surface being water, and by chemical means showed that the products collected by the water were identical with the so-called transuranium elements previously examined. Cloud chamber photographs of the tracks of the ejected particles have been obtained (Joliot, *C. R. Paris*, **208**, 647, Feb. 27, 1939 ; Corson and Thornton, *Phys. Rev.*, **55**, 509, March 1, 1939). The maximum range was judged to be about 3 cm. of air.

Work is proceeding actively on the identification of all the products of the fission of U and Th, as may be seen by looking at recent issues of *Nature, Physical Review, Comptes Rendus,* and *Naturwissenschaften.* It is as yet too early to give a classified list of the nuclear reactions. They are consistent with the views already indicated, *i.e* the incident neutron is captured by the U or Th nucleus to form a compound nucleus. Reactions of Types I, II or III may follow, but most compound nuclei break up into products about half the size. One or both of these smaller nuclei contain excess of neutrons and are unstable. They may, possibly after a measurable time, disintegrate by successive β-ray decay or neutron emission until the neutron-proton ratio of a stable nucleus is reached. N. Feather has, in a very interesting way, found an upper limit to the lifetime of the compound U nucleus (*Nature,* **143,** 597, April 8, 1939). A " sandwich " was made of a thick layer of U between gold foils, each 6·74 mgm./cm.^{2}, and the activities of the products collected on equal polythene sheets placed in contact with the gold on either side, were compared after the sandwich had been irradiated with fast neutrons impinging on one side only. The general activity was reduced to $\frac{1}{8}$ of that from uncovered U, and 10 ± 3 per cent. more was obtained on the forwards collector than on the other. When gold films 7·13 mgm./cm.^{2} were used, 19 ± 3 per cent. more activity was obtained forwards than backwards. These observations showed that, after capture of the neutron, fission of the compound nucleus usually occurred before the forward momentum was lost. Using approximate values of range and momentum, an upper limit of 5×10^{-13} sec. was calculated for the life-time of the compound nucleus.

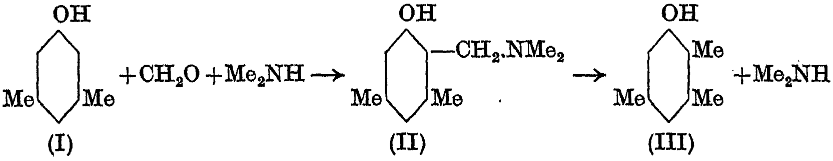
We must resist the temptation to describe the observations on β-ray and neutron emission from the radioactive products, and the evidence for a greater number of emitted neutrons than absorbed, for space will not allow and since many more experiments will have been described by the time this article appears in print. Perhaps, at some future date, we may return to these questions. Sufficient has been said, however, to show that the " transuranium elements " have had a short life in modern physics, and that a fruitful type of nuclear reaction has been added to the list.

GENERAL AND ORGANIC CHEMISTRY. By O. L. Brady, D.Sc., F.I.C., University College, London.

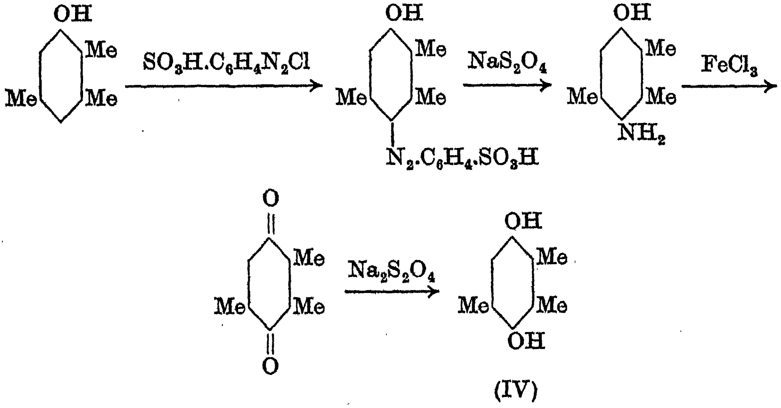
Alkylation of Phenols.—A novel method of introducing a methyl group into phenols has been described by Caldwell and Thompson (*J. Amer. Chem. Soc.,* 1939, **61,** 765). This consists in the reduction of the dimethylamino-methyl derivatives obtained by Décombe

(*Compt. Rend.*, 1933, **196**, 866; **197**, 258) by the condensation of phenols with formaldehyde and dimethylamine.

The method has been used specifically for the synthesis of 2 : 3 : 5 trimethylhydroquinone, a compound of importance in the synthesis of one of the anti-sterility vitamins, α-tocopherol (Karrer, Fritzsche, Ringier and Salomon, *Helv. Chim. Acta.*, 1938, **21**, 820). *Sym*-xylenol (I) was treated with 35 per cent. aqueous dimethylamine and formalin added drop by drop, when 2-dimethylaminomethyl-3 : 5-dimethylphenol (II) was obtained. This was reduced in dioxan solution at 165° C and 177 atmos. with hydrogen in the presence of copper chromite as catalyst to 2 : 3 : 5-trimethylphenol (III).



The conversion of this compound to the trimethylhydroquinone (IV) was brought about by coupling with diazotised sulphanilic acid, reduction of the oxy-azo-compound with sodium hydrosulphite to the amino-phenol, oxidation of this with ferric chloride to the quinone and final reduction with sodium hydrosulphite.



CIS- AND TRANS-AZOBENZENE.—Azobenzene can exist in stereoisomeric forms but it is only recently that the two isomerides of

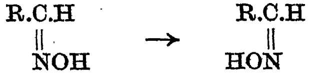
$$\begin{array}{c} Ph.N \\ \| \\ N.Ph \end{array}$$

trans-azobenzene
m.p. 68°; $\mu = 0$

$$\begin{array}{c} Ph.N \\ \| \\ Ph.N \end{array}$$

cis-azobenzene
m.p. 71°; $\mu = 3{\cdot}0$

this common compound have been isolated. (Hartley, *Nature*, 1937, **140**, 281; *J. Chem. Soc.*, 1938, 633). Commercial azobenzene is nearly pure *trans*-azobenzene and the pure *trans*-isomeride (m.p. 68°) can be prepared from it by crystallisation from an alcoholic solution boiled for six hours in the dark. When solutions of this compound are exposed to light an equilibrium between the *cis*- and *trans*- form is established containing from 15–40 per cent. of the *cis*- form according to the solvent used. This change is analogous to the conversion of the stable aromatic α-aldoximes into the labile β-aldoximes by the action of light (Ciamician and Silber, *Ber.*, 1903, **36**, 4268; Brady and McHugh, *J. Chem. Soc.*, 1924, **125**, 547).



$$\begin{array}{ccc} R.C.H & & R.C.H \\ \| & \longrightarrow & \| \\ NOH & & HON \end{array}$$

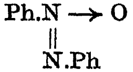
α-aldoxime β-aldoxime

The *cis*-azobenzene is more soluble than the *trans*- form in polar solvents especially in water ($6{\cdot}5 \times 10^{-4}$ mol. per litre at 25° C. compared with 2×10^{-5} mol. per litre) but less soluble in such solvents as light petroleum (about 0·9 per cent. compared with 3·5 per cent. in petroleum b.p. 40–60°). Pure *cis*-azobenzene (m.p. 71·4°) was prepared by exposing a saturated solution of ordinary azobenzene in glacial acetic acid to bright light for three or four hours and fractionally precipitating the compound by the addition of water, when the more soluble and therefore last precipitated portions contain most of the *cis*-compound. Final purification was effected by dissolving the impure material in light petroleum at room temperature and cooling in an ice and salt mixture, when the pure *cis*-azobenzene crystallises out in bright red crystals.

A more rapid method of obtaining small quantities of *cis*-azobenzene has been described by Cook (*J. Chem. Soc.*, 1938, 879) and consists in chromatographic analysis of a solution of azobenzene in light petroleum irridiated for 30 minutes with a quartz mercury vapour lamp.

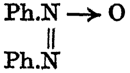
The *cis*- compound is stable in the solid state in the dark at room temperature but is converted completely into the *trans*- form on keeping it above its melting point for a short time; in solution in the dark it changes slowly to the *trans*- compound and this change is hastened by rise in temperature. The photochemical change gives rise to an equilibrium *cis* ⇌ *trans* but the thermal change is in one direction only *cis* → *trans* and at room-temperature is slow compared with the photochemical transformation. The configuration of the two forms was determined by dipole moment measurements, the *cis*- form having a moment of 3·0 D and the *trans*- a moment of zero.

In order to determine the relative amounts of isomerides present in equilibrium mixtures in solution either a photometric method depending upon the greater absorption in the blue region of the spectrum exhibited by the *cis*- form or dipole moment measurements may be employed. (Hartley and Le Fèvre, *J. Chem. Soc.*, 1939, 531). The latter workers have shown that ordinary *p*-azotoluene is the *trans*- form but that when a solution of this compound in benzene is exposed to light an equilibrium is set up containing 7 per cent. of the *cis*-isomeride. *Cis*-*p*-azotoluene has been isolated by Cook (*loc. cit.*) by chromatographic analysis of an irridiated solution as deep red needles, m.p. 105°. Two forms of azoxybenzene have been known for many years (Reissert, *Ber.*, 1909, **42**, 1367) but it was only recently that it was established that these are geometrical isomerides and their configuration determined by dipole moment measurements. (Gehrckens and Müller, *Annalen*, 1933, **50**, 296).



$$\begin{array}{c}\text{Ph.N}\rightarrow\text{O}\\ \| \\ \text{N.Ph}\end{array}$$

stable *trans*-azoxybenzene
m.p. 36° ; μ = 1·7



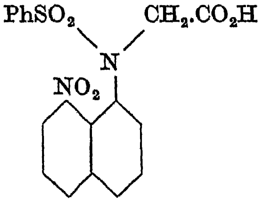
$$\begin{array}{c}\text{Ph.N}\rightarrow\text{O}\\ \| \\ \text{Ph.N}\end{array}$$

labile *cis*-azoxybenzene
m.p. 81° ; μ = 4.7

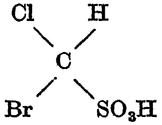
ASYMMETRIC TRANSFORMATION (OPTICAL ACTIVATION) IN SOLUTION. —An important contribution to this subject has been made by Jamison and Turner (*J. Chem. Soc.*, 1938, 1646).

The optical resolution of an optically stable acid A by means of an optically active and optically stable base *l*B is successful if a solvent can be found in which the solubilities of the two diastereoisomerides, *l*B.*d*A and *l*B.*l*A are sufficiently different to permit of what is virtually a fractional crystallisation. This is, of course, the basis of Pasteur's well-known method of resolution. When the acid is optically unstable, that is, it racemises relatively rapidly under the conditions of experiment, the separation of the less soluble salt *l*B.*d*A, for example, is followed by racemisation of the acid portion of the salt left in solution *l*B*l*A $\rightleftharpoons$ *l*B*d*A, with the result that more of the less soluble salt *l*B*d*A separates until finally all the acid separates in this form. There are many instances of this phenomenon on record and Kuhn (*Ber.*, 1932, **65**, 49) has designated them as "asymmetric transformations of the second order."

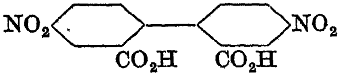
Perhaps the most striking case is that recorded by Mills and Elliott (*J. Chem. Soc.*, 1928, 1291). When brucine and N-benzenesulphonyl-8-nitro-1-naphthylglycine (V) are mixed in moist acetone solution all the salt in solution separates as brucine-*l*-acid ($+ 1H_2O$).



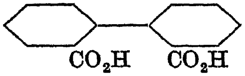
(V)



(VI)



(VII)

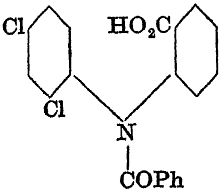


(VIII)

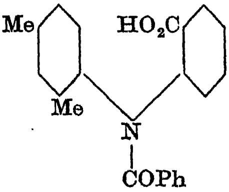
If this salt is dissolved in moist methyl alcohol the solution soon deposits the whole of the dissolved material as brucine-*d*-acid (+ $3H_2O$) since this is the less soluble salt in the new solvent.

In these cases the determining factor deciding which salt will separate is the solubility ratio of *l*B.*d*A and *l*B.*l*A and the relative stabilities of these salts do not affect the result. If, however, neither salt crystallises the solution may contain unequal amounts of *l*B.*d*A and *l*B.*l*A. The first observation of this phenomenon was that of Read and McMath (*J. Chem. Soc.*, 1925, **127**, 1572), who concluded that the composition of the equilibrium mixture in an acetone solution of the *l*-hydroxyhydrindamine salts of chlorobromomethane sulphonic acid (VI) was 19 per cent. of *l*-base-*d*-acid and 81 per cent. of *l*-base-*l*-acid. Kuhn and Albrecht (*Annalen*, 1927, **455**, 272) found that 4 : 4′-dinitrodiphenic acid (VII) gave a strongly *d*-rotatory salt with *l*-quinine and ascribed this result and that of Read and McMath to " asymmetric transformation of the first order." Lesslie and Turner (*J. Chem. Soc.*, 1934, 347) found that diphenic acid (VIII) itself behaved similarly to its dinitro derivative.

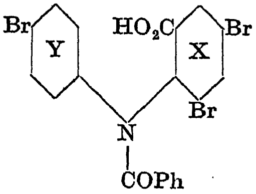
Jamison and Turner (*loc. cit.*) in an investigation of the processes underlying first order asymmetric transformation have shown that the equilibrium *l*B.*d*A $\rightleftharpoons$ *l*B.*l*A, when B is an optically stable active base and A is an optically unstable acid, is markedly altered by adding excess of *dl*-A, even in the case of acids of very low optical stability. For example, a chloroform solution of equivalent quantities of nor-*d*-ψ-ephedrine and benzoyl-2 : 4-dichloro-2′-carboxy-diphenylamine (IX) was *l*-rotatory, but when more of the inactive acid was added to this solution the *l*-rotation gradually decreased with increasing quantities of acid and the solution finally became *d*-rotatory. Similar observations were made with the corresponding dimethyl compound (X). The optical stability of these two acids is so small that mutarotational effects were not



(IX)



(X)



(XI)

observed at room temperatures but when chloroform solutions of an active base and excess of either inactive acids were mixed at −30° rapid mutarotation was observed and it cannot be doubted that this was due to the establishment of an equilibrium in which *d*A, mainly combined as *l*B.*d*A, was present in larger quantities than *l*B.*l*A. It, therefore, appears to follow that if addition of an optically unstable acid *dl*A to a chloroform solution of an optically stable, active base at ordinary temperatures is accompanied by a considerable change in rotation, first order asymmetric transformation is responsible and that if the addition were carried out at a low enough temperature this transformation could be observed by the mutarotation of the solution.

It also follows that with an acid of moderate optical stability, that is, one whose racemisation can be observed in solution at ordinary temperatures, addition of excess of inactive acid to the neutral salt of this acid with an active base should cause no great change in rotation at the moment of mixing but that mutarotation should be observed. This was confirmed by using benzoyl-4-2′-4′-tribromo-6′-carboxydiphenylamine (XI). A kinetic investigation showed that for the *l*-cinchonidine salt of this acid the equilibrium in chloroform for the neutral salt was 49·1 per cent. *l*B.*d*A and 50·9 per cent. *l*B.*l*A. In the presence of excess of the acid, approach to equilibrium was much faster, and it is probable that a certain proportion of the excess of free acid is finally present in the active form. The authors make it clear, however, that they regard their work as but a preliminary investigation of asymmetric transformation and further results will be awaited with interest.

The optical activity of the acids (IX), (X) and (XI) is due to restricted rotation within the molecule. Consideration of a model of (XI) shows that the system Y—N—COPh cannot rotate freely round the axis N—X owing to the presence of the bromine and the carboxyl group; in consequence, this acid has moderate optical stability. In the cases of the acids (IX) and (X) the obstructional effect is much smaller and the optical stability is very slight. When the chlorine atoms in (IX) are absent the alkaloidal salts of the acid still show marked change in rotation on the addition of excess of acid, but even at −30° mutarotation is too rapid to be observable.

The work described promises to increase our knowledge of the nature of salts in solution in organic solvents. The optical activations observed by Jamison and Turner must depend on the presence in solution of non-dissociated salts. In hydroxylic solvents a salt such as *l*B.*d*A is largely dissociated and the relative stabilities of *l*B.*d*A and *l*B.*l*A cannot have their effect.

Loss of Nitrogen in the Chamber Process for the Manufacture of Sulphuric Acid.—In accordance with the reactions usually given for the oxidation of sulphur dioxide in the Chamber Process, the oxides of nitrogen introduced, apart from mechanical losses, should suffice to carry on the process indefinitely

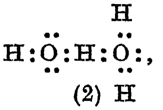
$$2SO_2 + N_2O_3(NO_2 + NO) + O_2 + H_2O \rightarrow 2SO_2(OH).O.NO$$
$$2SO_2(OH).O.NO + H_2O \rightarrow 2H_2SO_4 + N_2O_3.$$

In practice, however, this is not the case and the oxides of nitrogen must be continually renewed. The isolation of sulphur monoxide (Schenk, *Zeit. anorg. Chem.*, 1933, **211**, 150) and the demonstration that this compound is formed to the extent of 1 per cent. when a mixture of sulphur vapour, sulphur dioxide and air is passed through a tube heated to 700° suggests that this compound may be present in appreciable quantities in the gases from pyrites burners and may reduce the higher oxides of nitrogen to nitrous oxide and/or nitrogen and thus remove them from the cycle. Support to this possibility is given by the work of Wilkins and Soper (*J. Chem. Soc.*, 1939, 600), who have shown that sulphur monoxide readily reduces nitrous acid to nitrogen in accordance with the equation

$$3SO + N_2O_3 = 3SO_2 + N_2.$$

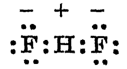
PHYSICAL CHEMISTRY. By H. W. Melville, D.Sc., Ph.D., Colloid Science Laboratory, Cambridge.

The Hydrogen Bond.—In the representation of the structure of chemical compounds it is usual to consider that the hydrogen atom is attached to only one other atom by what is termed a single bond involving the sharing of two electrons. But within recent years a considerable body of evidence has accumulated which points to the fact that the hydrogen atom is rather more versatile in its behaviour. Indeed the hydrogen atom may apparently become perhaps not divalent, but certainly it may possess a bond order greater than unity. This idea is not by any means new for it was suggested many years ago as a means for explaining the association of molecules containing hydrogen. For example Huggins (1919, quoted by Hilbert, Wulf, Hendricks and Liddel, *J. Amer. Chem. Soc.*, **58**, 548, 1936) and Latimer and Rodebush (*ibid.*, **42**, 1419, 1920) suggest that the well-known association of water molecules and the formation of ions such as HF_2^- might be explained in this way. The presumed structure of associated water was supposed to be



$$H:\underset{\cdot\cdot}{\overset{\cdot\cdot}{O}}:H:\underset{\underset{(2)\,H}{\cdot\cdot}}{\overset{\overset{H}{\cdot\cdot}}{O}}:,$$

the hydrogen (2) being equally shared between the two oxygen atoms. Similarly the HF_2^- ion was represented by



$$:\underset{\cdot\cdot}{\overset{\overset{-}{\cdot\cdot}}{F}}:\overset{+}{H}:\underset{\cdot\cdot}{\overset{\overset{-}{\cdot\cdot}}{F}}:$$

since Pauling has pointed out (*Proc. Nat. Acad. Sci.*, **14**, 359, 1928) in virtue of the Pauli Exclusion Principle that the bonding must be ionic in character. Sidgwick (*J. Chem. Soc.*, **127**, 907, 1925) extended the idea to organic compounds. Furthermore, since the beginning of classical physical chemistry of liquids and solutions many examples of associated molecules have been observed especially among carboxylic acids. These latter methods could sometimes give no more than a qualitative indication of the presence of associated molecules, although with the carboxylic acids molecular weight determinations at once gave exactly the degree of association. The problem to be tackled then is not so much the demonstration of the existence of associated molecules but the precise determination of their structure and concentration in order that some ideas might be obtained about the formulation of an explanation of the phenomenon based on the newer concepts of valency.

Two methods may obviously be applied to the problem, namely (*a*) the X-ray and electron diffraction technique [1] and (*b*) the infra-red method of determining molecular structure. The former is more suited for the solid state of matter and the latter for the liquid state. This review will be mainly confined to infra-red investigations of hydrogen bonding, the pioneer work having been done by Wulf and his co-workers and by Errera and his colleagues.

First of all, however, mention may be made of an early paper by Pauling and Brockway (*Proc. Nat. Acad. Sci.*, **20**, 336, 1934) on the electron diffraction pattern of formic acid vapour. By making the assumption that a given proportion of double molecules is present these authors found that the following structure will theoretically give a diffraction pattern similar to that observed.

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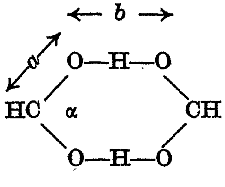
[1] A review of this aspect of the problem by J. M. Robertson has recently appeared in SCIENCE PROGRESS, XXXIII, 663, 1939.



$a = 1{\cdot}29 \pm 0{\cdot}02$A

$b = 2{\cdot}67 \pm 0{\cdot}04$A

$\alpha = 125 \pm 5°$

The actual position of the hydrogens is not of course known, but it is interesting to note that the O—O distance is intermediate between that in ice—2·77 × 10^{-8}cm.—and that in potassium hydrogen phthalate—2·54 A. Further the angle is intermediate between that for a double and a single bond, but the C—O distance is normal compared with that—1·29 × 10^{-8}—found by Pauling and Sherman (*ibid.*, **20**, 340, 1934) for basic beryllium acetate.

The infra-red work started when Hilbert, Wulf, Hendricks and Liddel (*Nature*, **135**, 147, 1935) and Errera and Mollet (*Comp. rend.*, **200**, 814, 1935) independently found that the characteristic absorptions of OH containing molecules at about 0·9, 1·6 and 3·0μ were absent when the liquids or solutions in carbon tetrachloride were examined. Before describing the results in more detail, perhaps some indication of the nature of OH and of NH absorption should be given. The exact position of the peak and also the form of the absorption band of such molecules, whether fundamental or overtone, depend on the structures of the remainder of the molecule. The question then arises as to how some quantitative measure of OH and of NH absorption may be obtained which will not depend on groups attached to the oxygen and the nitrogen atoms respectively. Wulf and Liddel (*J. Amer. Chem. Soc.*, **57**, 1464, 1935) had however found by the investigation of a very large number of such molecules that if the molecular extinction coefficient (measured in dilute solution to avoid association) is plotted against frequency in cm.$^{-1}$ the area under the curve so obtained does not vary by more than a factor of 2–3 for similar compounds. For example with phenolic compounds which absorb around 7100 cm.$^{-1}$ (1·41μ), that is, the second harmonic of the N—H vibration, the molal absorption coefficient area [1] in litres cm.$^{-2}$ mole^{-1} varies from 70 for *o*-methoxy benzyl alcohol to 170 per OH group for catechol.

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[1] This is the area under the absorption curve in cm^2. The ordinates and abscessæ are actually arbitrarily chosen.

Similarly for NH_2 compounds absorbing around 6600 cm.$^{-1}$ (1·51μ) the extreme values are 65 for methyl anthranilate and 200 for *p*-nitro aniline. Likewise NH compounds exhibit the same characteristics, for example, 55 for di-*n*-heptylamine and 125 for 9-fluorenoneimide. In estimating the number of OH, NH, etc., absorbing groups in any given known mixture the accuracy will of course greatly exceed that implied by the above mentioned figures, though it must be remembered that the molecular extinction coefficient, apart from association phenomena, does vary with the concentration of the solution.

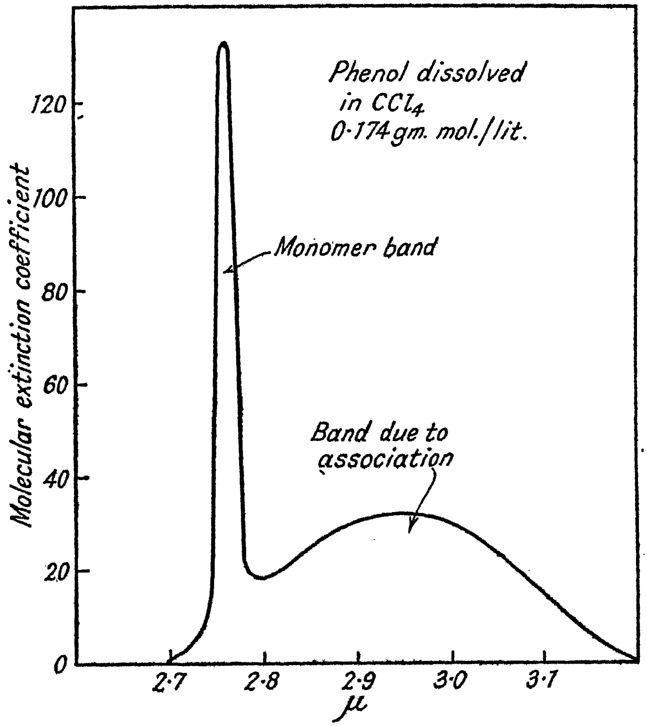
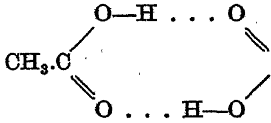


FIG. 1.—Absorption of phenol dissolved in carbon tetrachloride showing position of band due to association of the monomeric molecules.

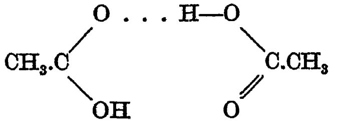
The type of absorption spectra exhibited when association of molecules occurs is clearly shown by Fig. 1 taken from a paper by Fox and Martin (*Proc. Roy. Soc.*, A, **162**, 419, 1937) on the alcohols. As will be observed the monomer and polymer bands are well separated.

It is now of importance to see whether in simple compounds exhibiting association, the infra-red technique can be correlated quantitatively with other physical methods. This will then indicate how much confidence may be placed on infra-red data in those molecules where other methods fail to yield a satisfactory answer to the question of appreciable association.

Carboxylic Acids.—The carboxylic acids provide a good test of the method. Badger and Bauer (*J. Chem. Physics*, **5**, 605, 1937) have found that acetic acid vapour possesses a sharp and well defined OH absorption band at 0·970μ (10,300 cm.$^{-1}$), that is, the second harmonic, the intensity of which depends on concentration and temperature. The percentage of OH groups present was estimated by putting known concentrations in the absorption cell and heating to such a temperature that only monomeric molecules were present. Assuming that temperature had no appreciable effect on the molecular extinction coefficient of the OH group the number of OH groups could be computed when there was association. Although the accuracy of the method was not claimed to be high, the infra-red data agreed well with the vapour density results of MacDougall (*J. Amer. Chem. Soc.*, **58**, 2585, 1936). There are, however, two dimeric formulæ for acetic acid



and



It is improbable that the second formula represents the structure of the dimer, but the infra-red measurements in the vapour only indicate a partial disappearance of the characteristic OH absorption and cannot therefore discriminate between the two possibilities.

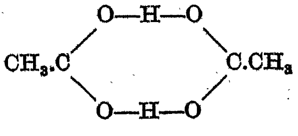
These experiments have been extended by Davies and Sutherland (*J. Chem. Physics*, **6**, 755, 1938) who examined a number of carboxylic acids in carbon tetrachloride solution, not only for OH absorption but also for C=O and for —C—O absorption. These additional observations lead to further conclusions about the structure of the dimer. Acetic acid and benzoic acid both show strong absorption at 2·84μ (3520 cm.$^{-1}$), such absorption increasing with increasing temperature and being due to the OH group. At about 3·3μ (3030 cm.$^{-1}$) where absorption by the dimer might be expected the C—H absorption is so strong that it masks that due to the dimer. In trichloracetic acid C—H absorption is avoided and an association band now appears at 2·9 to 3·3μ (3450–3030 cm.$^{-1}$) as is proved by the decrease in its intensity with increasing temperature. The OH absorption in alcohols at 2·76μ (3620 cm.) thus differs somewhat from that in acids, and therefore allows of a discrimination in a complex molecule between the two types of absorbing centre. In trichloracetic acid the peak of the absorption is at a slightly longer wavelength, due presumably to a weaker bond on account of the greater strength of the acid. The centre of the dimeric band is at 3·25μ (3076 cm.$^{-1}$), this band being much broader than the sharp OH band. The shift in frequency on association is thus 448 cm.$^{-1}$ compared with 230 cm.$^{-1}$ for the alcohols (see below).

The C=O vibration is likewise affected by association. Here there are two peaks at 5·66μ (1768 cm.$^{-1}$) and 5·82μ (1718 cm.$^{-1}$) the latter being the more intense. When the temperature of the solution is raised to 73·5° C. the peaks shift a little to 5·64μ and 5·81μ but are now nearly equal in intensity. The effect of temperature on the absorption peaks due to the —C—O— group also yields evidence about the origin of these bands, of which there are three for trichloracetic acid at 6·98, 7·06 and 7·85μ. The band at 6·98μ disappears as the temperature is raised, whereas the band at 6·82μ changes its position to 7·2μ and becomes very strong. The former band must therefore be due to the dimer and the latter to the monomer. From these observations it is possible to estimate approximately from the frequency shifts what change occurs in internuclear separation when dimerisation takes place. To do this use must be made of an empirical relationship due to Clark (*Phil. Mag.*, **18**, 459, 1934) between internuclear distance and vibration frequency. The following results for acetic acid show that the change in internuclear distance is comparatively small:

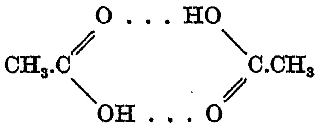
C = O VIBRATION

	Absorption band		$r_{C=O}$(A)	$\Delta r_{C=O}$(A)
	μ	cm.$^{-1}$		
Monomer	5·65	1768	1·195	0·016
Dimer	5·82	1701	1·211	
	—C—O— VIBRATION			
Monomer	7·25	1379	1·298	0·013
Dimer	6·94	1425	1·285	

In view of the small frequency shift in the OH bands it must be concluded that when acetic acid associates it does not do so to a symmetrical structure



but to the unsymmetrical form



in which the hydrogens are only very weakly bound to the ketonic oxygens.

Using the integral absorption of the monomeric band at 2·83μ (3530 cm.$^{-1}$), Davies and Sutherland (*J. Chem. Physics*, **6**, 767, 1938) have estimated how the concentration of the monomer varies with total acid concentration and with temperature for acetic, benzoic and lauric acids in carbon tetrachloride solution. The figures so obtained agree reasonably well with what would be expected from the law of mass action. The temperature coefficient of the equilibrium constant gave a value of 9300 cals. per mole for the heat of association as compared with 15,500 cals. from other data. The result was not so satisfactory for benzoic acid. This is probably due to the fact that the integral molecular absorption of the monomeric OH band is affected both by concentration and by temperature.

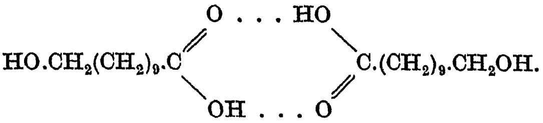
When a hydroxyl group is introduced into the acid molecule matters become a good deal more complicated because of the tendency of the alcoholic OH groups to exhibit association phenomena. There are now a number of different associated structures, but it is possible, as has been shown by Davies (*J. Chem. Physics*, **6**, 770, 1938), in the case of ω-hydroxy undecanoic acid

$$CH_2OH(CH_2)_9COOH$$

to determine approximately what happens at any given concentration, for the absorption of alcoholic and carboxylic OH may be studied simultaneously. The following figures show how the concentration of free OH groups, calculated from the integral absorptions, and apparent molecular weights vary with concentration

conc. gm. mole/lit.	0	0·0035	0·007	0·020	0·037	0·050	0·0625	0·074	0·125	0·162
Mol wt. . . .	202	—	—	—	386	—	—	420	—	490
Free alcoholic OH, % . . .	—	30	64	63	—	—	50	—	56	—
Free carboxylic OH, % . . .	—	18·9	28·5	—	—	15·9	—	—	1·25	—

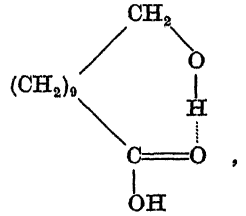
The relatively small percentage of free carboxylic OH groups at concentrations where the percentage of free alcoholic OH groups is considerably higher, *i.e.* above 0·007 gm. mole/lit. indicates that the following type of associated structure must be present



But this structure alone would not account for any reduction at all in alcoholic OH concentration and hence it is probable that there is interaction between two molecules involving the alcoholic OH group according to these formulæ

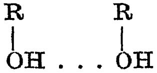
(*a*) $HOOC(CH_2)_9.CH_2OH \ldots HO.CH_2(CH_2)_9.COOH$
(*b*) $HO.CH_2(CH_2)_9.COOH \ldots HO.CH_2(CH_2)_9.COOH.$

It will be observed that a concentration of 0·0035 moles/lit. both OH absorptions are low and that moreover the OH carboxylic absorption at 0·007 moles/lit. is actually greater than that at 0·0035. This anomalous behaviour is attributed to cyclisation



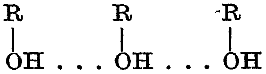
although then the carboxylic OH ought not to be affected.

ALCOHOLIC ASSOCIATION.—It has long been known that hydroxylic compounds exhibit anomalous physical properties which behaviour has been attributed to association of some kind. Unlike carboxylic acids, however, such properties could not directly give a measure of the degree of association. Here therefore the infra-red technique becomes especially valuable (Fox and Martin, *Proc. Roy. Soc.*, A, **162**, 419, 1937). A typical plot of molecular extinction coefficient as a function of wavelength is shown in Fig. 1. The narrow absorption band at 2·77μ (3610 cm.$^{-1}$) is characteristic of all alcohols and the peak of the sharp band does not depend to any large extent on the structure of the alcohol. For all normal aliphatic alcohols it is at 2·75μ, for $C_6H_5CH_2OH$ 2·764, for $(C_6H_5)_2CHOH$ 2·765, and for $(C_6H_5)_3COH$ 2·769μ. On the long wavelength side of this band, there is a wide band which is certainly due to association, for as the concentration of the solution is increased this band becomes more prominent. The question arises as to whether the associated molecule here is a dimer



$$\overset{\overset{R}{|}}{O}H \ldots \overset{\overset{R}{|}}{O}H$$

or a polymer

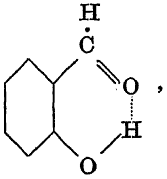


$$\overset{\overset{R}{|}}{O}H \ldots \overset{\overset{R}{|}}{O}H \ldots \overset{\overset{R}{|}}{O}H$$

If association took place to the dimer then the OH absorption should only be decreased to half at the highest concentrations. In point of fact the reduction in extinction coefficient is greater than this, which would favour the polymer structure. Again, however, the effect of concentration on the molecular extinction coefficient of the OH group may account for this result. Similar behaviour is shown by all alcohols, the difference in the position of the two absorption bands being relatively small, thereby indicating that the strength of the OH band is not much weakened as a result of the association of the hydrogen atom with another molecule.

When the alcohol is dissolved in a nonpolar solvent there is the possibility of interaction between solute and solvent (Errera and Sack, *Trans. Faraday Soc.*, **33**, 728, 1938). With ethyl alcohol dissolved in CCl_4 the first and second harmonics of the monomer band appear at 3630 and 7100 cm.$^{-1}$ and the polymer bands at 3300 cm.$^{-1}$ and 6300 cm.$^{-1}$ On solution in dioxane or acetone only a single band at 3500 cm.$^{-1}$ makes its appearance, while the peak is slightly different at 3260 cm.$^{-1}$ in pyridine. Likewise the characteristic phenolic bands disappear, to be replaced by a single band. If to these binary mixtures is added a nonpolar solvent such as carbon tetrachloride all three bands, that is monomer, polymer and alcohol-dioxane, make their appearance. It is a curious fact that although the intensity of the polymer band decreases rapidly with increasing temperature, that due to the complex is not much affected.

INTRA- AND INTER-MOLECULAR BONDING THROUGH HYDROGEN.—Provided certain geometrical criteria are fulfilled intramolecular association can occur as was shown by Errera and Sack (*loc. cit.*) for the hydroxybenzaldehydes. Liquid *o*-hydroxybenzaldehyde does not exhibit the characteristic absorption of phenol, two broad bands with peaks at 3150 and 3080 cm.$^{-1}$ appearing in place of the former band. This absorption is not due to intermolecular association for a dilute solution in carbon tetrachloride absorbs in the same region. But if dioxane is used as a solvent the peak of the band is shifted to 3250 cm.$^{-1}$, that is, towards the monomeric OH frequency, thus pointing to a reduction in intramolecular hydrogen bonding. With *m*-hydroxybenzaldehyde the normal OH frequency at 3300 cm.$^{-1}$ is present, for the hydrogen of the OH group is too far away to interact with the oxygen of the aldehyde group. The structure of the *o*-hydroxy benzaldehyde is probably



a six-membered ring being formed.

The detection of hydrogen bonding between unlike molecules is complicated by the fact that the proton acceptor or electron donor, as it is sometimes called, absorbs strongly in the region of the monomeric and polymeric OH bands. In order to overcome this difficulty W. Gordy (*J. Chem. Physics*, **7**, 93, 1939) has studied the infra-red absorption spectra of CH_3OD. By replacing the hydroxylic hydrogen by deuterium the fundamental absorption is shifted from 3 to 4μ that is, clear of the absorption of other molecules. CH_3OD shows the same association phenomena as CH_3OH. In benzene solution the OD absorption has its peak at 3·75μ while the pure liquid has a peak at about 4μ which is due to association. Similar association peaks are observed when CH_3OD is dissolved in a large number of oxygen-containing solvents. Association is least in evidence in nitrobenzene and *o*-nitrotoluene. In the acetates, aldehydes and ketones it is very marked while diethyl ether and *iso*propyl ether lead to the strongest association if this is measured by the area of the absorption band or by the intensity of the absorption at the peak. Typical absorption curves are shown in Fig. 2. Nitriles and amines also act as proton acceptors, the OD band in some mixtures becoming double, although to a less extent than oxygen-containing solvents. Whether the reverse action occurs, that is, the amine acting as a proton donor is a question yet to be investigated in more detail.

Errera and Mollet were the first to suggest the formation of intramolecular hydrogen bonds in *o*-hydroxybenzaldehyde and *o*-nitrophenol (*Compt. rendus*, **200**, 814, 1935; *J. phys. Radium*, **6**, 281, 1935), owing to the absence of OH absorption in these liquids. There are, however, innumerable organic compounds where similar intramolecular links may exist, but a number of factors must be taken into consideration when deciding whether this type of linking will actually occur, as has been pointed out by Hilbert, Wulf, Hendricks and Liddel (*J. Amer. Chem. Soc.*, **58**, 548, 1936). The first is that of geometrical configuration, that is, whether the oxygen atom is sufficiently close to allow of effective interaction with the hydrogen atom; the second is the electron affinity of the atoms sharing the hydrogen bond and the third the possibility of the formation of structures exhibiting resonance. Geometrical considerations are best elucidated by determining the infra-red absorption of a number of compounds in which hydrogen bonding is expected. From a considerable amount of X-ray investigation (see J. M. Robertson, SCIENCE PROGRESS, XXXIII, 663, 1939) it would appear that the minimum distance between two oxygen atoms with a hydrogen atom in between is about 2·60 A. This is therefore the first condition to be fulfilled. Thus in the enol forms of acetyl acetone and of benzoyl acetone no normal OH absorption can be detected. On the other hand, aldol—β-hydroxy butyraldehyde—shows evidence of hydrogen bonding even though the geometrical configuration is precisely similar to that in β-diketones. These latter compounds form six-membered rings, presumably free from strain. The reluctance of aldol to form hydrogen bands is therefore attributed to small electron affinity. In compounds such as benzoin, *d*-diethyl tartrate where hydrogen bonding would involve the formation of five- or seven-membered rings absorption (though somewhat modified) in the OH region shows that no such bonding occurs.

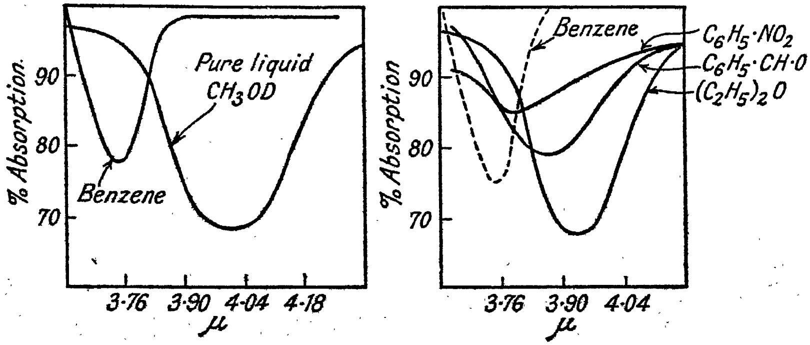
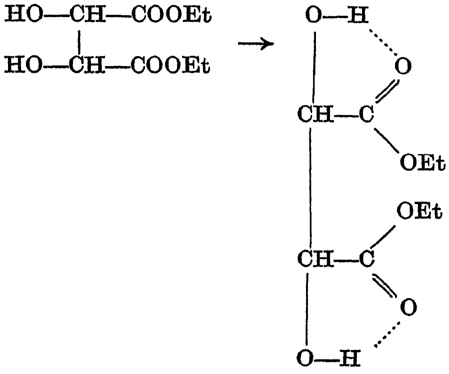
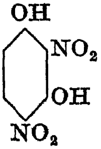


FIG. 2.—Absorption of CH_3OD in various solvents

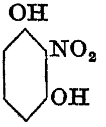


Similarly in ethylene glycol monoacetate where a seven-membered ring might be formed, there is again no OH absorption.

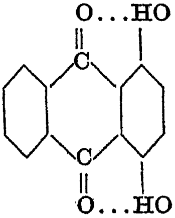
All compounds of the general type X—CHO, X—COOR and X—COR, where X is an ortho hydroxy aromatic nucleus unsubstituted in the three position, exhibit intramolecular bonding. The presence of substituents in the 4, 5 or 6 positions do not prevent bonding. The nitro compounds also show some interesting features. Ortho nitro phenols give no characteristic OH absorption, in *m*-nitro phenol OH absorption is strong. In 3-nitro-catechol only one of the hydroxylic hydrogens is involved in hydrogen bonding, the other exhibits strong absorption with a peak at 6920 cm.$^{-1}$ But the substitution of another NO_2 group as in 2 : 4



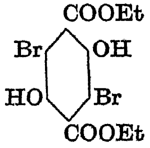
(I)



(II)



(III)



(IV)

dinitro-resorcinol (I) wipes out OH absorption once more. A rather curious example is 2-nitro-resorcinol (II); here no OH absorption can be detected, hence it must be concluded that two hydrogen bonds terminate on one nitro group. Another example of a compound possessing more than one hydrogen bond in the same molecule is afforded by 1 : 4- (and also 1 : 5-) dihydroxy-anthraquinone (III). Purely steric effects seem to be responsible for the persistence of OH absorption in 3 : 6-dibromo-2 : 5-dihydroxy-diethyl-terephthalate (IV) the suggestion being made that the bulky bromine atoms push the carbethoxy group away from the hydroxyls. Removal of the bromine atoms to form 2 : 5-dihydroxy-diethyl-terephthalate once more leads to hydrogen bonding.

Examples can of course be multiplied almost indefinitely. One of the main advances made by the application of the infra-red technique is the definite proof of the existence of hydrogen bonds in compounds where the usual physical methods would have been unequivocal in their decision for or against the structure. The spectroscopic method at once provides a certain and direct indication of the existence of hydrogen bonds about which there can be little argument.

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., D.Sc., F.R.S.E., The University, Glasgow.

Petrography of Igneous Rocks (Great Britain, Europe, Asia).—L. M. Ingold describes " The Geology of the Currywongaun-Doughruagh Area, co. Galway " (*Proc. Roy. Irish Acad.*, XLIII, Sect. B, No. 10, 1937, 135–59). The hills consist of an intrusion of eucrite which metamorphoses the schists and injection-gneisses of the Connemara Series. This rock is particularly rich in rhombic pyroxene, and is probably connected with a small intrusion of serpentine. Both intrusions are thought to be of Pre-Cambrian

One of the most interesting new results of J. G. C. Anderson's exhaustive work on " The Etive Granite Complex " (*Quart. Journ. Geol. Soc.*, XCIII, 1937, 487–533) is that the Bheinn a' Bhuiridh screen on the south-eastern side of the Cruachan Granite consists of Devonian hornblende-andesites which have been metamorphosed on a considerable scale to biotite-schists by contemporaneous contact-action and shearing. The Meall Odhar Granite has been separated from the Cruachan Granite, which it cuts and overlies with a flattish base. This is in accord with the hypothesis of large-scale stoping which has been invoked to explain the structural relations of the complex.

The main part of W. A. Deer's paper on " The Diorites and Associated Rocks of the Glen Tilt Complex, Perthshire " (*Geol. Mag.*, LXXV, 1938, 174–84) is devoted to a discussion of the granite-diorite hybrid rocks which have been developed on the margin of the complex. An interesting point which has emerged from this study is that a low P_2O_5 content appears to be characteristic of granite which has suffered endo-contamination (Nockolds), in contrast to the high P_2O_5 content which is usually found in exo-contaminated granites.

With the aid of several new analyses, the same author again discusses "The Marginal Rocks of the Cairnsmore of Carsphairn Complex" (*ibid.*, LXXIV, 1937, 361–76), and shows that the sedimentary contamination on the margins of this complex was due to incorporation of xenoliths of greywacke and shale in an early gabbro member before the main intrusion of tonalite took place.

The geochemistry of the process of granitisation is considered by Malcolm MacGregor in a paper on "The Evolution of the Criffel-Dalbeattie Quartz-diorite: A Study of Granitization" (*Geol. Mag.*, LXXV, 1938, 481–96) with the aid of additional analytical data to that given in his earlier paper on the same massif (SCIENCE PROGRESS, XXXII, 1938, p. 530). He concludes that the process of "granitisation," whereby greywackes and shales have been converted into quartz-diorite at the western end of the complex, was due to the uprise of a "parent magma" of "alkali-feldspar" composition, involving selective metasomatism of the country rocks by which their composition, whatever its initial make-up, converged towards quartz-diorite.

C. Koomans and P. H. Kuenen discuss the "Differentiation of the Glen More Ring-dike" of Mull (*Geol. Mag.*, LXXV, 1938, 145–60), with the aid of four new analyses. The theory that the upward transition in this intrusion from basic to acid is due to gravity-differentiation has been opposed by A. Holmes and others, and a hybridisation theory of the mixing of two contrasted magmas substituted. Koomans and Kuenen, however, support the original theory with small modifications, and point out the serious difficulties encountered by hybridisation and mixing theories.

In his third paper on "The Petrology of Barnavave," S. R. Nockolds describes "Some Hybrids from East and South-East of Barnavave Mountain" (*Geol. Mag.*, LXXV, 1938, 469–79). The eucrite of Barnavave is altered and impregnated by later quartzo-feldspathic material. Certain small "alkaline pools" in the altered rocks are ascribed to materials carried in by granitic veins rich in alkali-feldspar.

The "Replacement Veins in the Mourne Mountains Granites, North Ireland," described by S. R. Nockolds and J. E. Richey (*Amer. Journ. Sci.*, **237**, 1939, 27–47), are mostly of greisen type. The dominant variety carries abundant siderophyllite, a rare iron-rich green mica. Later fissure veins carry fluorite, apophyllite and daphnite, an iron-rich chlorite. Iron, fluorine and water, with minor amounts of aluminium and lithium, have been added to the veins from outside, and the transporting medium has likewise removed nearly all the sodium, and some of the potassium, calcium and silica.

In his memoir on "The Anorthosite and Kenningite of the Nordingrå-Rödö Region: A Contribution to the Problem of the Anorthosites," H. von Eckermann (*Geol. För. Förh. Stockholm*, **60**, 1938, 243–84) claims that the initial magma was of gabbroid composition, and that anorthosite was formed by the concentration of plagioclase crystals towards the roof of the magma-chamber. Concentration of volatiles beneath the roof of frozen but still hot anorthosite led to the formation of a zone of gabbro-pegmatite. Some highly porphyritic dike-rocks, called *kenningite*, are of similar chemical composition to the anorthosite, and are believed to represent volcanic magma produced by a sudden short-lived re-fusion of a part of the anorthosite roof.

In his paper on "L'Activité volcanique dans le Barrandien," F. Slavik (*Bull. Soc. géol. France* (5), VII, 1937, 527–40) summarises the volcanicity of the geological periods from Algonkian to Devonian, still comprised under the term "Barrandian." It appears that the majority of the igneous rocks belong to the spilitic suite.

In his paper "La pétrologie du pluton de la Bohême centrale," A. Orlov (*ibid.*, VII, 1937, 497–512) discusses the evolution of an enormous complex batholith, 4000 sq. km. in area, which consists of gabbros, diorites, syenites and granodiorites of many different types. The order of intrusion is, as usual, from basic to acid. During its very slow and prolonged consolidation the different intrusions comprising the batholith came under the influence of intermittent tectonic pressures which caused a kind of filtration-differentiation within the separate intrusive bodies which, nevertheless, are all genetically connected.

G. Wilson discusses "The Evolution of the Granodiorite Rocks of the South-eastern End of the Kopaonik Batholith, Yugo-slavia" (*Geol. Mag.*, LXXV, 1938, 193–218). This composite batholith forms one of the more southerly members of the peri-Adriatic tonalite massifs. It is intrusive into sedimentary rocks ranging from dominantly calcareous to argillaceous types. The marginal rocks of the batholith are considerably modified by assimilation of the sediments, which have been metamorphosed into crystalline limestones, skarns and calc-silicate hornfels, grading downward into mica-, amphibole- and epidote-schists, and locally cordierite-bearing rocks. The differentiation of the batholith, and the processes of contamination and metamorphism, have been the subjects of close study, and are illustrated by many detailed chemical analyses.

L. Jugovics and A. Marchet describe "Der Sägberg in Ungarn und seine Ergussgesteine" (*Min. Petr. Mitt.*, **49**, 1937, 369–414) as a mass of basaltic tuffs surmounted by two flows of basalt. A final eruption of dolerite filled fissures in the upper basalt. The rocks belong to the Atlantic Series, and are chemically similar to the most melanocratic, nepheline-free basalts of the Plattensee.

In their memoir "Zur Kenntnis des oberkretazischen Vulkanismus im östlichen Bulgarien," K. H. Scheumann and A. Schüller (*Min. Petr. Mitt.*, **50**, 1938, 29–80) describe the extrusive rocks of this region as varying from lime-rich pyroxene-andesites, through andesitic dacites, to trachytes and quartz-latites. The lavas are thoroughly propylitised. They belong to the sub-Balkan zone of igneous activity and are connected with the exposed plutonic massifs of eastern Bulgaria.

Two papers on Roumanian igneous petrography by A. Codarçea have been received. The first is a "Note préliminaire sur certaines roches éruptives alcalines et sur quelques lamprophyres de la région Ogradina (Dept. de Severin)" (*Bull. Sect. Sci. Acad. Roumanie*, XVIII, 1936, 1–7 (reprint)), which deals with a series of nepheline-syenites, porphyries and tinguaites, in which cancrinite is a prominent mineral. The second is a "Note sur la structure géologique et pétrographique de la région Ogradina-Svinita (Dept. de Severin)" (*C.R. Inst. Géol. Roumanie*, XXI, 1937, 179–98). The Ogradina region is notable petrographically for the occurrence of an extensive zone of "ophiolitic" rocks, including serpentine, gabbro and epi-gabbro. The Ogradina Granite is believed to be a hybrid rock of palingenetic origin.

An important memoir by F. Kossmat deals with "Der ophiolithische Magmagürtel in den Kettengebirgen des mediterranen Systems" (*Sitz.-Ber. Preuss. Akad. Wiss. Phys.-Math. Kl.*, **24**, 1937, 308–25). The term *ophiolitic*, including serpentine, gabbro, dolerite, green-stones, with diabase-porphyrites, spilites, keratophyres, etc., is the Continental equivalent of our "spilitic series." An enormous cicatrix (*Narbe*), says Kossmat, filled with Sima rising from abyssal magma-zones, unites the tectonically-connected northern continents (North America, Eurasia) with the southern continental complex known as Gondwanaland. The course of the cicatrix, the arcuate segments of which follow all the windings of the young folding-zones and are plastically moulded on them, clearly proves that its infilling antedated the main folding—a view further supported by the prevailingly Late-Mesozoic age of the ophiolitic eruptions.

In a paper "On the Imperial Porphyry," G. Andrew (*Bull. Inst. Égypte*, XX, 1938, 63–81) describes the geology and petrography of the beautiful rose-red ornamental stone (*porfido rosso antico*) which was quarried by the Romans in a remote part of the eastern desert of Egypt. It is an andesitic tuff pigmented by a fine powder of hæmatite, but the rock also contains the pink manganiferous epidote, withamite.

In a paper on "Spilitic Rocks from Chitaldrug, Mysore State" C. S. Pichamuthu (*Current Science*, VII, 1938, 55–7) shows that chemical analyses, the occurrence of pillow-structure, and the association of the albite-rich basalts with keratophyres and soda-granites, confirm the view that this ancient suite of igneous rocks belongs to the spilitic series.

In his study "On the Origin of the Pacific Magma Types in the Volcanic Inner-Arc of the Soenda Mountain System," R. W. van Bemmelen ("*De Ingenieur in Nederlandsch-Indie,*" *IV. Mijnb. en Geol.*, "*De Mijn-ingenieur,*" Jaarg. IV, No. 1, 1938, 1–15) distinguishes three methods for the generation of magma-types: (1) differentiation of primary magma; (2) magmatisation of pre-existing crustal rocks by ascending emanations from the sub-stratum (secondary or palingenetic magmas); (3) contamination of magmas derived from processes 1 and 2 by assimilation of foreign material (syntexis). He concludes that the Pacific magma-types of Java, and of the world in general, owe their origin to processes 2 and 3, and especially to the ascent of magmatic emanations rich in silica, alumina and alkalis (the "migmatite-front" of Wegmann).

According to R. W. van Bemmelen, in his memoir on "De Ringgit-Beser: een geplooide alkali-vulkaan in Oost-Java" (*Nat. Tiddschr. v. Nederlandsch.-Indie*, XCVIII, 1938, 171–94), this volcano was initiated during the Plio-Pleistocene in a shallow sea. During the later Pleistocene it was subjected to folding movements, and an asymmetrical anticline developed at its southern foot. A collapse-cauldron was simultaneously produced at the summit of the rigid main cone. The lavas are highly potassic, ranging from leucite-melilite-basalt to leucite-tephrite and phonolite, of which sixteen new analyses are published, as well as four from the adjacent Loeroes complex.

In his paper "On a Rock of Nordmarkitic Composition from Hakutô Volcano, Korea," J. Suzuki (*Journ. Fac. Sci. Hokkaidô Imper. Univ.* Ser. IV, Vol. 4, 1938, 173–81) shows that the nordmarkite occurs as blocks over the whole surface of the volcano, which is mainly composed of flows of alkali-trachyte. The nordmarkite probably represents the deepseated portion of the magmatic body which gave rise to the volcano.

The same author describes "The Occurrence of Aegirine-Augite in Natrolite Veins in the Dolerite from Nemuro, Hokkaidô" (*ibid.*, 183–9). The igneous rocks occurs as innumerable sills and lavas within a Senonian formation, and consist mainly of analcite-olivine-dolerite. Ægirine-augite is developed where augite phenocrysts project into natrolite veins.

According to H. Kuno, in his "Note on the Daruma Volcano, Izu, Japan" (*Bull. Earthquake Research Inst., Tokyo Imper. Univ.*, XVI, 1938, 763–71), this cone may be classed as a shield volcano of Hawaiian type, with breached crater, gentle slopes, and hardly any violently explosive action. The lavas are hypersthene-augite-andesites which became somewhat richer in olivine in later stages of activity.

"The Erh Ko Shan Volcano, Lung Chiang Province, Manchuria," described by T. Ogura and M. Matsumoto (*Survey Reports of Volcanoes in Manchuria*, No. 2, 1938, 1–18 (Japanese), 19–21 (English summary)), consists of three isolated cones with craters still intact or but slightly breached. The lavas are identified as leucite-basanite.

PEDOLOGY. By Professor N. M. Comber, D.Sc., A.R.C.S., F.I.C., The University, Leeds.

A notable addition to pedological literature is the English edition of Sigmond's *Principles of Soil Science* translated from the Hungarian by Yolland (see review, p. 188).

In the *Revue générale des Sciences* December 1938 and January 1939, Professor Agafonoff writes a "Revue de Pédologie." This is a general account of the subject designed to interest the French scientists, with a broad account of the soils of France as they are classified in the Soil Map of Europe. The Soils of Palestine are described by Reifenberg in a book which gives a valuable summary of the theories of soil formation in the Mediterranean climate (see review, p. 189).

Soil structure—the size, shape, consistency and degree of permanence of aggregates—has always been an important feature of soil profile descriptions. Various systems of classifying structional units have been suggested—see Clarke, *The Study of the Soil in the Field*, second edition. Recently E. W. Russell (*Imperial Bureau of Soil Science Technical Communication No. 37*, 1938) has reviewed the investigations of soil structure—its specification, classification and relation to cultivation.

The experimental investigation of soil structure is much more difficult than might be supposed. Investigations based on the determination of permeability and of the pore space are complicated by the fact that water applied to a dry soil does more than merely fill the pore space; some is strongly absorbed by the surface of the soil particles. Moreover, the soil aggregates on absorbing water, swell, and to some extent disintegrate. Some of the difficulty is overcome by using a non-polar liquid.

Several workers have investigated the possibilities of aggregate analysis but there is no final and ultimate definition of an aggregate. Some investigators are concerned to determine the size of aggregates as they exist in the field, although these are admittedly not permanent. At the other extreme there is the conception of what are virtually permanent aggregates, and intermediately the water stable aggregates may be considered. It is not many years ago that the problem of making mechanical analyses of soil, that is of determining the sizes of the prime particles, was beset with the difficulty of getting a complete disintegration of the aggregates and the independence of the prime particles. Attention is now largely focussed on the aggregates themselves instead of only on the separate particles composing them.

Various attempts have been made to find some concise expression of the structural character of a soil and state of aggregation of its particles. Some definitions of structure are expressed in terms of the proportion of prime particles below a specified diameter that are bound in aggregates above that same or some other specified diameter.

After an air-dried soil has been mixed with water and allowed to dry out the state of aggregation of the particles will vary according to the amount of water used. Where only small quantities of water are used there is very little aggregation. When considerable excess of water is used, the tendency is for large and hard clods to be formed. Intermediately there is an optimum amount of water which gives rise to aggregates which have a maximum of mechanical strength and water stability.

The important phenomenon of base exchange has been mostly studied in connection with the mineral colloids, but it has been quite well known for some time now that the organic colloids weight for weight have even greater base exchange capacity than the clay. There has also accumulated some evidence that the organic and mineral colloids of the soil are probably chemically combined. Further understanding of this subject comes with a paper by Tyulin (*Soil Science*, **45**, 1938). Tyulin separates from the soil by peptisation in the form of the sodium complex one group of the soil colloids. After this there still remained in the soil, colloids which can be examined by optical and other colloidal methods but which can only be separated by alkali or acid treatment. The first group of colloids are designated "electronegative gels," and their sodium complex has an electrokinetic potential of 40–50 millivolts. The second group is designated "isoelectric gels," and the electrokinetic potential of their sodium complex is only of the order of 4 millivolts. Examination of the two groups of colloids from three different soils suggests that the absorption capacity of a soil is largely dependent upon the preponderance of the first group rather than upon the total colloids, thus a red soil had a low absorption capacity in spite of a content of about 80 per cent. colloidal matter : the colloidal matter in the first group, however, was only about one-third of the whole. A Tschernozem with a much higher absorption capacity had a slightly less total colloid content, but a great preponderance of the colloidal matter belonged to the electronegative group. The extraction of easily removable humic colloid from these two groups goes to show that organic colloids make up the outer layer of particles in the first or electronegative group, while another part of the organic matter in this group is more firmly held in association with the mineral colloid. In the second group of gels organic matter does not seem to occur on the surface.

The organic constituents of certain Scottish soils have been examined by Shewan (*Journal of Agricultural Science*, April 1938). The proximate analysis of the organic matter in the various horizons of a number of profiles confirms the recently developed views on the lignin-protein constitution of humus. The organic matter in the profiles examined fell into two groups : the raw humus type in which the amount of hemicellulose becomes rapidly less below the surface layer and a mull type in which the hemicellulose persists throughout the whole profile. Shewan also draws attention to the fact that his data consistently with that of other recent workers invalidates the still persistent statement that the $\frac{C}{N}$ ratio in soils does not vary much from ten. In one of the author's tables the ratio varies from 8 to 40.

The possible excretion of nitrogen into soils by leguminous plants has been investigated from time to time with rather inconsistent results. A paper on the subject by P. W. Wilson and J. C. Burton (*Journal of Agricultural Science*, April 1938) gives a résumé of previous work on the subject and adds to the experimental enquiry on the matter. There seems little doubt that both positive and negative results have been correctly obtained. The writers make an interesting suggestion in explanation of the discrepancies although it is not claimed to be necessarily a complete explanation. When weather conditions are not propitious or any circumstance makes the growth of the leguminous plant poor the fixation of nitrogen from the air will be small and no excretion will take place. At the other extreme, if conditions are highly conducive to an abundant growth of a legume, that abundant growth may need all the nitrogen that the plant fixes and again none will be excreted. It is thought that in some intermediate set of conditions excretion of nitrogen, that is expressed in the greater growth of crops alongside the legumes, may take place.

BOTANY. By PROFESSOR E. J. SALISBURY, C.B.E., D.Sc., F.R.S., University College, London.

IN 1937 A. D. Shamel (*Jour. Hered.*, **28**, 350) recorded the production of a tetraploid type of pear with larger fruits derived as a sport from Williams' Bon Chretien (Bartlett). This tetraploid had a haploid chromosome number of 34 instead of 17. Crane and Thomas now record (*Jour. Genetics*, XXXVII, 287, 1939) a parallel large-fruited type derived from the pear " Fertility." This is also a tetraploid with $2n = 68$. The mean weight of the fruit of the tetraploid was 197 grams as compared with 125 grams for the fruit of the diploid. Whereas the diploid " Fertility " is almost self-incompatible, yielding when self-pollinated an average of 0·8 per cent. fruits with two good seeds per fruit, the tetraploid, which the authors term " Improved Fertility," yielded when self-pollinated an average of 37·5 per cent. of fruits with a mean number of 5·7 seeds per fruit. This tetraploid which arose as a bud mutation from " Fertility " is thus self-fertile in an appreciable degree. A further point of interest is the frequent development in the tetraploid of branched pollen tubes.

A study of the cytology of the Liliaceous genus Calochortus, native to Western North America, has been carried out by J. M. Beal (*Bot. Gaz.*, **100**, 528, 1939), who has investigated twenty-eight of the known species, which number about fifty. The basic chromosome number, it is suggested, is probably seven for the Section Mariposa and perhaps for the genus as a whole. The somatic numbers observed were 12, 14, 16, 18, 20, 21, 28, 32, and 40. On the basis of the form of the chromosomes it is not unlikely that *C. superbus* with $2n = 12$ may have been derived from *C. luteus* by the fusion of two chromosomes.

A wide range of chromosome numbers in the Leguminosæ is recorded by H. A. Senn (*Bibliog. Genetica*, p. 175, 1938). The haploid numbers range from 5 to 16, 18 to 26, 30, 32, 40, 48 and 65. The basic number in the Papilionatæ is held to be 8, giving rise to the Vicieæ with 7 and the Phaseoleæ with 11 chromosomes. Some of the woody types are held to be derived.

D. Cairns (*N.Z. Journal of Science*, XX, 173*a*, 1938) records the frequent regeneration of *Senecio Jacobœa* from rootstocks. The vegetative shoots arise endogenously in the pericyclic phellogen of the roots. The root of the Ragwort is peculiar in that there is no definite endodermis except during a brief period in the early development of the root in the region of the root-hairs; subsequently it becomes broken by the growth and division of the pericyclic cells. The adventitious shoots arise on injured roots and this presents an obstacle to eradication if roots or portions thereof are left in the soil. These shoots may also develop on plants whose main stem has died down normally.

When the foliage of trees is destroyed the leaves which subsequently develop are often different in form from the normal leaves. Such atypical leaves have been studied by E. S. Oliver (*Bot. Gaz.*, **100**, 563, 1939) in the following species: *Populus tremuloides, Corylus americana, Tilia americana, Ulmus americana,* and *Platanus occidentalis.* It is claimed that the atypical foliage corresponds in form to that of fossil types and is thus to be regarded as atavistic.

Amongst the known British representatives of the Chrysophyceæ the genus Uroglena has so far only been represented by *U. volvox.* Miss E. M. Lind now records (*Jour. Bot.*, LXXVII, 106, 1939) the occurrence of *Uroglena soniaca* from a pond near Sheffield, a species recently described by Dr. Conrad from the forest of Soignes. In the same journal (p. 116) an interesting new genus of Cruciferæ from Arabia is recorded by Dr. Turrill, under the Name Horwoodia. The fruits are one-seeded, by abortion of the ovule in the second loculus which is well-nigh occluded, and bear two wings. It appears to be most nearly related to the genus Isatis, but presents an interesting combination of characters that have been regarded as of taxonomic importance in the family.

The Phytoplankton of the River Shannon and Lough Derg is the subject of a paper by the late R. Southern and A. C. Gardiner in which quantitative data are furnished for a period of 59 consecutive weeks. The authors list fourteen species which appear to be perennial. Eight of these are diatoms of which *Melosira italica f. crenulata, Fragilaria crotonensis* and *Tabellaria flocculosa* exhibited a pronounced maximum in March, *Gyrosigma attenuata* a maximum in April, *Asterionella gracillima* and *Synedra acus* a maximum in May and *Tabellaria fenestrata* a maximum in June. The perennial Cyanophyceæ are *Gomphosphæria lacustris* (autumn), *Aphanizomenon Flos-aquæ* (Nov.) and *Cœlosphærium Nœgelianum* (Oct.). The perennial Chlorophyceæ were *Scenedesmus quadricauda* (May), *Ankistrodesmus falcatus* (June–July) and *Pediastrum Boryanum* (July) (*Proc. Roy. Irish Acad.*, XLV, B, 89, 1938).

From experiments carried out by H. Jenny and R. Overstreet (*Soil Science*, **257**, 47, 1939) these authors conclude that there is evidence for the existence of contact exchange by which cations may transfer from root to clay or vice versa. They found that, whilst barley roots with a low salt content retain their potassium against distilled water and against salt solutions of moderate concentrations, yet dilute clay suspensions carrying adsorbed sodium, ammonia or hydrogen ions remove potassium from the barley roots. They compare their results with those of Albrecht, who found that clay systems of varying degrees of saturation could remove cations from soybean plants grown in them (*Amer. Jour. Bot.*, 403, 1938). If this conception be valid, the intimate contact between root hairs and soil, as well as the large surface involved, may have a significance hitherto unrecognised.

In the same journal F. Heyward reports data respecting the moisture relations of burnt and unburned areas in Longleaf Pine Forests. In general, as might be expected, the unburnt areas showed a higher moisture content, but little difference with respect to the wilting coefficients. The differences are attributed to destruction of organic mulch.

Some interesting data concerning the effects of the sea floods in Norfolk in 1938 are furnished in the *Trans. Norfolk and Norwich Nat. Soc.*, XIV, Pt. 4, 334–90, 1939). The meres which before the flooding had a salinity of between 1·5 per cent. and 2 per cent. were flooded in February and even by the following November the salinity was still between 6 per cent. and 12 per cent. Amongst the plants which survived may be mentioned *Sonchus palustris, Calystegia sepium, Lychnis flos-cuculi, Potentilla anserina, Ceratophyllum demersum, Caltha palustris, Eupatorium cannabinum, Cladium mariscus* and various sedges. Amongst those which succumbed were *Ranunculus acris, R. bulbosus, Cardamine pratensis, Trifolium repens, Ulmaria palustris, Epilobium hirsutum, Sambucus nigra, Prunella vulgaris, Iris pseudacorus* and several grasses. Many aquatics perished but both *Lemna trisulca* and *Lemna minor* survived 16·2 p.p. thousand of chloride and *Ranunculus heterophyllus* survived 14 p.p. thousand.

In the same journal (pp. 398–438) F. W. Oliver describes the seasonal aspects of the vegetation at Mareotis in Egypt, and not only furnishes a useful list of the characteristic species but also a striking illustration of the importance of the seasonal distribution of rainfall as well as its amount. In 1939, owing to the fact that the total rainfall was nearly double that which normally falls and distributed throughout the period from October to March, with furthermore continuously cool weather rendering the precipitation more effective, the vegetation was estimated by the author as about ten times the amount that is normally developed at this period.

The *Limosella* plants of Glamorgan are the subject of a paper by Miss E. Vachell and Dr. K. Blackburn (*Jour. Bot.*, LXXVII, 65–71, 1939) from which it appears that there are two species and the hybrid between them. The North American *Limosella subulata* occurs at Kenfig Pool, the Glaslyn River, Crumlin Bog and Morfa Swamp. It is distinguished by its subulate leaves, which are light green, its larger (4 mm.) and strongly scented flowers, with a long style and a somatic chromosome number of 20. In *L. aquatica* the leaves are elliptical at maturity, of a dark green colour; the flowers are smaller (2·5 mm.) and not scented, whilst the style is short. The somatic number of chromosomes is 40. The hybrid is a larger, more vigorous plant, with flowers similar to those of *L. subulata* and scented. The style is of intermediate length with a large stigma. The hybrid, which has a somatic chromosome complement of 30, is sterile.

ENTOMOLOGY. By H. F. Barnes, M.A., Ph.D., Rothamsted Experimental Station, Harpenden.

General Entomology.—In 1934 about 2000 fossil insects were collected in the Creede formation, Colorado, in the Rocky Mountain region (*Psyche*, **45**, 1938, 105–20). F. M. Carpenter has given an introductory account of the environment, while the Neuroptera, Isoptera and Diptera (Tipulidæ, Bibionidæ and Syrphidæ) have been dealt with respectively by F. M. Carpenter, T. E. Snyder, C. P. Alexander, M. T. James and F. M. Hull. The great duplication of a few species suggests that the fauna is an exceedingly small one. This is typical of present-day faunas at high altitudes.

The interrelationships and lines of descent of living insects have been discussed by G. C. Crampton (*Psyche*, **45**, 1938, 165–81), who gives a phylogenetic tree which it is desired should be interpreted as a three dimensional one. This investigator's views differ radically from those commonly accepted, and one object of the present contribution is to indicate that the latter are not the only possible or necessarily the correct ones.

Two important papers on musculature in insects have appeared recently. One by G. S. Graham-Smith (*Parasitology*, **30**, 1938, 441–76) deals with the generative organs of both sexes of the blow-fly *Calliphora erythrocephala*, the other by A. D. Imms (*Q.J.M.S.*, **81**, 1939, 273–320) is on antennal musculature in insects and arthropods.

There are apparently two types of first antennæ in arthropods, *viz.* the segmented and the annulated. The former consists of a variable number of segments each with intrinsic musculature. This type obtains in the Chilopoda, Diplopoda, Pauropoda, Symphyla, Collembola and Thysanura Entognatha, as well as in many Copepoda and Ostracoda. The annulated type consists of a peduncle or protopodite of one or more segments, each with intrinsic musculature, and an annulated flagellum devoid of intrinsic muscles. The movements of this flagellum are effected by muscles originating in the peduncle. This second type of antenna is found in the Crustacea Malacostraca, the Thysanura Ectognatha and all the Insecta Pterygota. The fact that the Thysanura Entognatha have segmented antennæ is additional evidence in support of the Symphylan theory of the ancestry of insects (see SCIENCE PROGRESS, No. 125, 1937, 120–1).

A biometrical analysis of growth in locusts of different phases bred under experimental conditions has been made by A. J. Duarte (*Bull. Ent. Res.*, **29**, 1938, 425–56). Among the conclusions are the following: Dyar's rule holds good for growth in the length of the middle femur and the width of the head. Secondly, Przibram's principle as modified by Bodenheimer is true for growth in length of the different parts and latent cell-divisions occur, one in the width of the head and pronotum and four in the length of the pronotum. [Przibram and Megusar (1912) found that at each moult in *Sphodromantis* the exuviæ and the bodies doubled their weight at the previous moult and that the increase in length of the pronotum was constant and equal to the cube root of 2. This connection between increase in weight and in length was considered to be due to the doubling in mass through the division of all the cells. Counts made by Sztern (1914) of the number of cells in the hypodermis between two successive moults in *Sphodomantis* supported this hypothesis. Frequently, however, a great range of variation for the growth-coefficients is found. Bodenheimer suggested that, where the rate of increase exceeds the standard coefficient, cell divisions (" latent divisions ") may have occurred more than once in the same stadium.] Another important paper on growth progression factors for successive growth stages is by F. H. Harries and C. F. Henderson (*Ann. Ent. Soc. Amer.*, **31**, 1938, 557–72). These authors, after a short historical account of Dyar's law and Przibram's principle, give the result of analysing certain measurements of several species of Cicadellidæ together with related data from other sources. Their findings support Dyar's law in general, but exceptions to the rule are fairly common. However, they claim that, as data for linear growth do not support the theory that growth in insects proceeds by simultaneous cell divisions, the significance of Przibram's progression principle is doubtful.

The distribution of insects by aerial currents has been studied by A. C. Hardy and P. S. Milne by means of sending up kites supporting nets which were opened at the desired height (150–2000 feet) and then closed again before being hauled down (*J. Anim. Ecol.*, **7**, 1938, 199–229). In this paper the equipment and methods of working are fully described. Eighty-two samples taken during a total flying time of 124½ hours yielded a total of 839 insects, including certain aphids, thrips and flies of economic importance. Most of the insects in this aerial plankton are small or light bodied with weak powers of flight but with relatively large wing surface. High temperature and low humidity were found to be the most favourable conditions for this aerial insects' drift. Another stratum of aerial insect populations has been investigated by H. E. McClure (*Ann. Ent. Soc. Amer.*, **31**, 1938, 504–13). Flying insects were collected in Illinois and Kentucky daily over a known distance by means of a net attached to the front fender (about 3 feet above ground level) of a motor-car. An aggregate of 2100 miles was covered and over 74,000 insects were captured ; of these 78 per cent. were Diptera, 11 per cent. Coleoptera, 2·4 per cent. Hymenoptera and 2·7 per cent. Aphididæ. The Diptera comprised Cecidomyids, Chironomids, Mycetophilids and miscellaneous Muscoidæ, while the majority of the beetles were Staphylinids. Insects were caught each month except January, and partly cloudy weather seemed most favourable for insect flight.

The insect enemies of chermes with particular reference to *Pineus pini* and *P. strobi* have been studied in England by F. Wilson (*Bull. Ent. Res.*, **29**, 1938, 373–89). No parasites were found. The predators fell into three groups, those removing the wool from the chermes before feeding, those which enter the wool mass and attack the chermes from within and those predators which do not enter the wool mass but attack the chermes from the outside. The five major predators are a *Leucopis* and a *Lestodiplosis* among the Diptera, a *Hemerobius* and a *Wesmaelius* among the Hemerobiidæ and a *Exochomus* among the Coccinellidæ.

J. T. Creighton (*J. Econ. Ent.*, **31**, 1938, 735–9) has described some experiments which showed that foliage deficient in such elements as zinc and copper affects the rate of caterpillar development. Thus there would be fewer generations during the year and this would ultimately affect the insect population. He suggests that increase in insect populations following fungicidal applications may be due more to a physiological change in the cell sap than to destruction of beneficial fungi.

Soil conservation versus insect control is the subject of an interesting paper by K. L. Harris (*Proc. Ent. Soc. Washington*, **41**, 1939, 20–6). It is pointed out that many of the entomologists' recommendations for the cultural control of pests are diametrically opposed to well-established principles of soil conservation. One example will suffice here; whereas in pest control, ploughing and leaving the ground bare and exposed, using quick-growing clean harvest crops is recommended, the soil conservationist demands a soil covered by a growing crop or residue from a crop.

J. A. Hyslop (*J. Econ. Ent.*, **31**, 1938, 557–9) makes a plea for the uniform and distinct use of the terms "brood" and "generation" commonly employed quite indiscriminately.

A hygrometer and an evaporimeter capable of investigating atmospheric conditions over small areas close to the surface of a leaf have been described by J. A. Ramsay, C. G. Butler and J. H. Sang (*J. Exp. Biol.*, **15**, 1938, 255–65). From rapidly transpiring leaves in a slow air stream a humidity gradient has been detected up to a height of 1·2 cm.

ORTHOPTERA.—Some interesting facts have been brought to light by G. E. Gould and H. O. Deay (*Ann. Ent. Soc. Amer.*, **31**, 1938, 489–98) in a detailed study of the biology of *Periplaneta americana*. The length of life of adult females varied from 102 to 588 days with an average of 450. There is a pre-ovipositional period of 11 or 12 days and then the female produces an egg capsule at intervals of about 3 days in June, July and August and of 12 days in October. The maximum number of capsules produced by one female was 90, and had all been perfect the offspring would have been 1440 whereas actually the records showed 970. The total life span for 3 individual females was 783, 793 and 913 days. Tests also indicated that only one mating was necessary in order to produce fertile capsules throughout the cockroach's life.

COLEOPTERA.—In 1937 a new research station for wireworm investigations was established at Warburton, Cheshire, with H. W. Miles as Director. Its first annual report (31 pp.) for 1937 contains various papers on the biology, ecology and control of wireworms.

The first of a series of studies on the morphology and bionomics of immature stages of Japanese chafer beetles by H. Yuasa and T. Endo, with an English summary (*J. Imp. Agric. Exp. Sta. Tokyo*, **3**, 1938, 151–82) deals with *Anomala cuprea*.

LEPIDOPTERA.—Among the recent papers on natural and biological control of insects the following can be mentioned. E. Cameron (*Bull. Ent. Res.*, **29**, 1938, 277–313) has studied the pea moth *Cydia nigricana* while J. E. Hardy (*loc. cit.*, 343–72) has dealt with the diamond-back moth *Plutella maculipennis*. Both investigations were carried out in England. The latter was undertaken because this pest, which was introduced into New Zealand fifty to sixty years ago, apparently has no parasites present in that country and consequently maintains itself at a very high uniform level. In other countries this moth is largely destroyed by parasites.

HEMIPTERA.—Recently C. S. Spooner (*Univ. Illinois. Bull.*, **35**, 1938, 102 pp.) and J. W. Evans (1938) (see SCIENCE PROGRESS, XXXIII, 131, 1939, 553–4), in studies on the head of Hemiptera, have independently concluded that the lora are secondarily detached parts of the postclypeus. R. E. Snodgras (*Proc. Ent. Soc. Washington*, **40**, 1938, 228–36) comes to the conclusion that the hemipterous lora are the expanded lateral parts of the basal region of the hypopharynx which would be exposed if the mandibles were reduced in size or removed.

J. W. Evans' most recent contribution to the study of the leafhoppers of Australia (*Pap. and Proc. Roy. Soc. Tasmania 1938*, 1938, 19–71) is in the nature of a summing up. In it he discusses the present systems of classification, the comparative value of different morphological characters and proposes a new classification which embraces twenty-one families. He also gives a phylogenetic tree indicating the possible inter-relationships of the various jassoid families.

The ecology of *Aleurodes brassicæ* has been studied by C. G. Butler (*Trans. R. Ent. Soc. Lond.*, **87**, 1938, 291–311). Parthenogenetic eggs gave rise to males whereas fertilised eggs produced approximately equal numbers of both sexes. Host preference experiments indicated that this whitefly is attracted to certain varieties of *Brassicæ oleracea* more than to others. A well-marked olfactory response to the odour of its host plant was observed. The adults tend to move from a region of low relative humidity to one of high humidity. The conclusion is reached that the damage caused to the plant is by the removal of certain nutritive substances, notably starch and sugars, and also by the wax and honey-dew hindering assimilation and transpiration. The honey-dew also encourages the growth of certain moulds. A later paper by the same author (*Proc. R. Ent. Soc. Lond.*, **13**, 1938, 161–72) deals with the relationship of the whitefly and the physical factors of its environment. The coldhardiness of the pupæ and adults is largely lost in the spring and early summer. Three causes of adult migration were found, *viz.* wilting of the plant, overcrowding of the adults and, most important, temperature. The extent to which the biotic and physical factors of the environment affect *A. brassicæ* is discussed and it is claimed that a fairly good theoretical estimate of its numbers in the field at a given time may be obtained from a previous study of the biotic and physical factors of the environment. This paper also deals with two Chalcid parasites, probably of Italian origin, which unfortunately cannot withstand the winter in England.

HYMENOPTERA.—An interesting set of olfactometer experiments has been made by W. H. Thorpe and H. B. Caudle (*Parasitology*, **30**, 1938, 523–8) on the olfactory responses of *Pimpla ruficollis*, a parasite of the pine shoot moth, to the food plant of its host. It was found that the adult females are not attracted by the oil of *Pinus sylvestris* until the third or fourth week of adult life and by this time their ovaries are relatively large. Previous to this period the oil is repellant and the ovaries are very small. From biological studies it would appear that the parasites emerge some time before the next generation of moth larvæ are ready for oviposition by the parasites and in this period the latter forsake the pines and visit Umbelliferous flowers.

D. C. Lloyd (*Phil. Trans. R. Soc. Lond.*, B, **229**, 1938, 275–322) has investigated in the laboratory some of the factors governing the choice of hosts and the distribution of progeny by the Chalcid *Oœncyrtus kuvanœ*. The distribution of the eggs by the parasite is not at random but has reference to the nature of the hosts available. Usually unparasitised hosts are selected, but if superparasitisation is forced those hosts containing the youngest parasite stage are chosen. If only parasitised hosts are accessible the female tends to retain her eggs. It is concluded that the ovipositional response is to a perceptual complex of stimuli which is such that the female tends to choose the hosts in which its progeny are able to develop.

Ecological notes on the *Sirex* wood wasps and their parasites in England have been written up by H. S. Hanson (*Bull. Ent. Res.*, **30**, 1939, 27–76).

DIPTERA.—L. D. Cleare (*Bull Ent. Res.*, **30**, 1939, 85–102) has given an account of the introduction and establishment of the Tachinid fly *Metagonistylum minense* into British Guiana. This fly, generally known as the Amazon fly, is an important parasite of *Diatrœa saccharalis*.

A full report on work carried out in Denmark on the housefly and the stable fly has been supplied by M. Thomsen (*Beretn. Vet.-og. Landbohojsk. Forsogslab.*, No. 176, 1938, 352 pp.). This includes accounts of their biology, ecology and control. Besides dealing with these two flies, groups of flies associated with cattle or with cow-dung in fields, with dwellings, stables and manure also receive attention.

OTHER ORDERS.—The Thysanoptera of Finland are being studied by Y. Hukkinen and V. Syrjänen. Already five papers have appeared in the *Ann. Ent. Fenn.*, **3–4**, 1937–8.

ARCHÆOLOGY. By E. N. Fallaize, B.A.

Early Man.—In the recent trend of archæological thought to re-examine fundamentals Mr. J. Reid Moir's Huxley Memorial Lecture for 1939, delivered on May 4 before the Imperial College of Science and Technology (Macmillan, 1*s*.), will rank high as a document of no little importance. It is a lucid and comprehensive exposition of the evidence now available bearing on the antiquity of man, according to the interpretation of one who, consistently throughout his career as an archæologist, has led the van in the more advanced school of thought.

Mr. Reid Moir sets out to answer T. H. Huxley's question, posed in his book *Man's Place in Nature*, "Where, then, must we look for primæval man? Was the oldest *Homo sapiens* Pliocene or Miocene, or yet more ancient?" Pointing out that the human origin of palæolithic axes had once been denied, the lecturer referred to the strenuous opposition with which had been met the claim for the human origin of the flaked flints found beneath the Pliocene Red Crag of East Anglia. The arguments for and against the artificial flaking of these flints were then reviewed in detail, and summed up in Mr. Reid Moir's view as in favour of the conclusion that the pre-Crag specimens were made by man in the Pliocene epoch. It was argued further that the Bone Bed, in which these flints were found, being of detrital origin, it was not an anomalous conclusion which saw in them, and the rare bone and other artefacts associated with them, evidence of no less than five distinct periods, of which the earliest must, in all probability, be relegated to Lower Pliocene times. The discovery of these pre-Crag artefacts, therefore, permitted the first part of Huxley's question, whether man was living in the Pliocene, to be answered in the affirmative.

When Mr. Reid Moir went on to examine the evidence for the existence of man in the Miocene, he entered upon a question upon which his opinion is awaited with no little interest by archæologists. For, as is well known, he has recently been engaged in a detailed examination of the evidence from Aurillac in the Cantal, France, where flaked pieces of flint of implemental forms occur in the gravel of the Upper Miocene. Specimens of these were brought to England by the late Mr. Ernest Westlake, and were examined by the late Prof. Sollas, who with some apparent hesitation was inclined to regard some of them, on the balance of probability, as the work of an intelligent being. Though precluded from dealing with this material in detail, pending publication, Mr. Reid Moir regards certain of the specimens as comparable with the Harrisonian eoliths. With these are other forms, such as points, scrapers, push planes, etc., together with rare examples of the rostro-carinates. It is evident, he concludes, that the Aurillac specimens are very simple types, and may possibly represent the earliest examples of intentionally flaked stone implements. If this should be established, he goes on to say, it will be possible to give an affirmative answer to Huxley's question as to whether man was living in the Miocene period.

In support of this conclusion, which goes far beyond what archæologists have hitherto been prepared to consider, Mr. Reid Moir points to the present trend of discovery in palæolithic deposits, which is in the direction of recognition that man was considerably more advanced in palæolithic times than has hitherto been supposed. In support he refers to the remarkable discoveries, pointing to a high level of culture, in Czecho-Slovakia at Predmost, and in European Russia, which, he says, constitute an archæological revolution.

The question was also discussed as to how far back it was possible to trace *Homo sapiens*, which was properly the subject of Huxley's enquiry. In this relation the importance of the recently discovered Swanscombe skull in the 100-ft. terrace of the Thames was adequately stressed, together with the evidence that at the beginning of the Pleistocene early man in the form of the Piltdown race, the Heidelberg men and Peking Man, showed very considerable variability. So that in very early times, at the beginning of the Pleistocene period, not only had man of the modern type come into existence, but also human beings were even more diversified than they are now. In view of this evidence it would seem impossible that man originated in the Pleistocene and "improbable that this momentous event took place even in the Pliocene epoch." On geological and other evidence, notwithstanding difficulties of computation, it must have been somewhere in the neighbourhood of two million years since the first flint implements were made.

THE PREHISTORIC SOCIETY AND ARCHÆOLOGICAL STUDIES.—Since the reorganisation of the work of the Prehistoric Society, which has enlarged its status from that of a regional to that of a national association, its publications, always of considerable interest and importance for the archæological student, have rapidly become of enhanced value. The Proceedings have now assumed a position in the foremost rank of archæological publications for their strictly scientific analysis of evidence bearing on some of the more obscure problems of British prehistory. It is, however, permissible to express a hope that the members of the Society will give serious consideration to the suggestion thrown out by Mr. O. G. S. Crawford in the course of his Presidential address for 1938, that the scope of the work of the Society might be so far extended as to include up to the Norman Conquest, instead of stopping, as it now does, at the Roman invasion. Recent advances in prehistoric studies have been of such a character as to suggest that if a like systematic attack were directed to the more obscure periods of the later history of Britain, in which at present little provision is made for the examination of strictly archæological evidence, progress in knowledge of cultural development in the Dark Ages, at present sadly to seek, would be no less marked than it has been in certain departments of prehistoric culture.

As an example of an advance in one direction in the prehistoric field, which is directly due to an organised attack made possible by the initiative of the Prehistoric Society, reference may be made to the excavations at Woodbury, Wiltshire, in 1938, through which a revealing light has been thrown on the social life and organisation of an Iron Age community in Britain of a type already known from partial excavation on other sites. This investigation was the first of what it may be hoped is a series of field investigations to be undertaken by the Prehistoric Society with a variety of aims, of which not the least important perhaps is to secure a greater degree of active co-operation among members of the society, and also at the same time to afford an opportunity for training in excavation of the younger archæologists.

The existence of the site, or rather of two sites on the south-west side of Salisbury was first demonstrated from an air photograph by Mr. O. G. S. Crawford. The smaller was selected for examination by the Society. Its size made the work financially feasible, and good results were promised by its clear markings on the photographs. A test excavation in March 1938 revealed evidence of a ditch of V-shaped section, and Iron Age A pottery. The excavations were under the direction of Mr. Gerhard Bersu, and lasted from June 12 to September 18, when part of the site had been cleared down to solid chalk. About one-fifth of the total area of 15,000 sq. yd. was examined. It can now be stated with certainty that the settlement belonged to Early Iron Age A, and probably to a late stage. The occupation was evidently of long duration; but the settlement seems throughout its career to have been immune from catastrophes by fire, and does not seem to have come to an end by violence. Except in a few places, the ditch apparently was never completed. Evidently it had been planned at some time during the existence of the settlement; and from the fact that analogous sites near Salisbury show ditches with similar features, it would appear that during Iron Age A times the people of the district were threatened, though by what is unknown, and thereupon took to defending their open settlements by banks and ditches, while at about the same time high places began to be protected by strongly planned defences. Hence it seems that we are dealing with an event of far-reaching effect. Smaller ditches parallel with the main ditch are further evidence of long occupation.

One of the most interesting features of the settlement is the large number of pits within the enclosure. They originate from different phases of the settlement and certainly are not contemporaneous. Of these more than ninety were examined. All had been artificially filled in, the filling varying greatly. Besides chalk, many contained ashes mixed with hundreds of burnt flints. In many pits there was more or less burnt " cob " in moderate amounts, in others fragments of burnt clay in forms with wide openings at the top and small fire holes in the sides. There was also much refuse consisting of animal bones, food refuse, potsherds, etc. The pits were evidently neither cooking-places, nor used for habitation. It is most probable that they were storage places for grain. Wheat could hardly be preserved in such pits without special preparation. This must have entailed roasting, or partial roasting. This would explain the quantities of ash, while the burnt fragments of clay and cob represent the ovens, in which the wheat was roasted. The fact that each of such pits could be used for a limited time only accounts for their number.

The fact that there cannot have been many dwellings on the site throws some light on the social structure of the settlement. Two large houses have been identified. One, or both, must have been inhabited by the proprietor of what was not a village, but rather a farm, a forerunner of the later Roman villa. Near the farmhouse, and protected in common with it, were the stock and products of agriculture. Hollows in the surface appear to have served as work-places, at which the people of the district must have assembled, as at a manor house, for the extra work following on the harvest. There is no space for cattle, and if, as is probable, cattle formed part of the economy of the settlement, they must have been housed elsewhere. Indications of such arrangements close to the main enclosure have been noted. This conclusion is supported by the fact that no water-supply, indispensable for the cattle, is near. Evidence of industrial activity is still lacking, nor does the relatively small number of spindle whorls point to any great production of textiles. When excavation of the site is complete, it is possible that light will be thrown upon this, with a number of other interesting problems which have emerged. These, as is pointed out by the excavators, affect not only Woodbury, but also conditions over a wide area in the early Iron Age of Britain.

South-Western Britain and the Continent in the Later Iron Age.—It is one of the fascinations of archæological investigation that the solution of one problem invariably brings in its train a number of other problems, which beckon the investigator along further tracks of which the end ever recedes. It may be remembered that in the excavations carried out by Dr. R. E. Mortimer Wheeler at Maiden Castle, near Dorchester, a civilization makes its appearance with some abruptness in the century immediately preceding our era, which is evidently of foreign origin, and is characterised by a new method of defence on a fortified site—multiple lines of outworks, instead of the single line of ditch and rampart hitherto found on the Wessex Downs, and manifestly designed to meet a new weapon of attack, the sling, for the use of which a multitude of sling stones now bears witness. Similar evidence has recently come to light on other sites in the south-west.

In seeking a source for the origin of this new culture, it was natural to look to Northern France, more especially to Normandy and Brittany, and in 1938 a series of exploratory investigations was undertaken by Dr. Wheeler, with the co-operation of the French authorities, on behalf of the Society of Antiquaries, and with the assistance of the Leverhulme Trust and the University of London.

In Northern Brittany and North-West Normandy Dr. Wheeler found two camps only which were at all comparable with Maiden Castle. Of these one was at Huelgoat, near Morlaix, and the other was situated near Avranches on the Bay of Mont St. Michel. These dated approximately from the middle of the first century B.C. The method of construction and the character of the surrounding country, which is infertile, were however such as to lead Dr. Wheeler to the conclusion that the settlements were of a military, rather than an economic significance. They were, in his view, rallying posts for the tribesmen from the surrounding country at the time of the Roman conquest. This view found support in the fact that the fortifications of one of the camps, that on the Bay of Mont St. Michel, had never been completed, but had been destroyed by military action at a date indicated by coins of local type to coincide with Cæsar's conquest. These offered no analogy to the Maiden Castle civilization of which he was in search.

The attention of Dr. Wheeler's expedition was then directed to the remains of Southern Brittany; and here Dr. Wheeler found the true analogy of the civilisation he had discovered at Maiden Castle. Here along the coast on the jagged cliffs, the bays and wooded estuaries, extending from Douarnenez to the mouth of the Loire, was the home of the Veneti, the tribes in whom Cæsar found his most strenuous opponents, both by land and by sea. Here Dr. Wheeler found, wherever digging has taken place, multiple ditches and quantities of sling stones—the "true forerunners of those tumultuous defences of South-Western Britain." A trial excavation was made on a commanding hill-top to the west of Quimper. The triple ramparts to-day, says Dr. Wheeler, are mere grass-grown mounds, but beneath the turf the innermost rampart was found to have been provided with a continuous range of stone steps, whereby it could be readily manned by the slingers, whose pebble ammunition lay thickly scattered in the vicinity. Pottery provided further links with certain sites on or near the southern coast of Britain.

In seeking an explanation of the relations between Southern Brittany and the Wessex Downs, Dr. Wheeler points out that the Veneti as traders were already familiar with the coasts of Cornwall. It may be that when Cæsar in 56 B.C. set out to crush the Veneti, some of the vanquished chieftains may have collected their scattered tribesmen and, following in the tracks of the Venetic traders, crossed the channel to introduce a new civilisation to dominate Wessex from Maiden Castle.

An interesting sequel to this expedition to Brittany, which will be resumed in the coming summer, is afforded by a small archæological excavation of a headland fortified post on Gurnard's Head near St. Ives, which was carried out under the direction of Dr. Wheeler in April of this year. Here a number of hut-circles are enclosed within a triple line of ditches running across the peninsula. Of these the innermost ditch is faced with stone and is stepped in the manner in which the inner rampart of the fort at Quimper was found to be stepped, while among a number of sea-rolled pebbles were a number of sling stones. The site appears to have been used as a place of refuge in times of stress rather than as an occupation site. It is interesting to note, further, that among the pottery is a type which is similar to pottery found on another site some 4 miles away, for which a relation with northern Spain, arising out of the ancient industry of metal working, has been suggested. Be this as it may, the present investigation clearly demonstrates a relation between the Cornish headland forts and those of southern Brittany; while in the similarity of the names of Kercaradec of the fort at Quimper and Cær Caradoc in Britain a further point in support of that relation is suggested.

NOTES

Molecular Spectra and Celestial Objects (A. Harvey)

At a symposium held at the Yerkes Observatory in June 1938,[1] some eleven papers were read dealing with recent progress in the interpretation of molecular spectra and with the study of molecular spectra in celestial spectra. Some of the papers presented were of interest only to the spectroscopist, but others dealt with topics familiar to the spectroscopist and then applied these considerations to problems of interest to the astronomer and astrophysicist.

As illustrating this may be taken the article by Herzberg on "Forbidden Transitions in Diatomic Molecules." In this are discussed the possible reasons for the appearance of forbidden transitions; (*a*) selection rule derivation employing an approximation, (*b*) quadrupole or magnetic dipole radiation, (*c*) presence of electric field either applied externally or due to the fields of surrounding molecules (enforced dipole radiation). Examples of these classes in celestial spectra are quoted, *i.e.* for class (*a*) there are singlet-triplet transitions illustrated by the Vegard-Kaplan bands of N_2 found in the aurora and also in the night sky luminescence, whilst for class (*b*) the best examples of magnetic dipole radiation are the red atmospheric absorption bands of oxygen.

Comets come in for a considerable amount of attention. Herzberg, in the paper mentioned above, discusses the difference in the spectrum of the head and of the tail of a comet, whilst Wurm pursues the matter considerably further. Wurm is interested primarily in cometary spectra and shapes (the latter being closely related to the physical state of the atmospheres). Some of the facts known at present are that the "head" of the comet contracts strongly as the sun is approached, and that the atmosphere contains the following molecules: CN, C_2, CH, CO^+ and N_2^+. It is taken for granted that the atmosphere is produced by heating of the meteorites which give off their occluded gases. Since the gravitational effect of the nucleus is negligibly small, the shape of the comet should be deducible if the manner in which the solar radiation acts on the freed gases is known. It would be expected, then, that all comets would be made up in similar fashion. In spite of the considerable variation in tail structure this is borne out by a study of the various emitting molecules over the different parts of the comet (see figure), it will be seen that the tail of the comet exhibits only the CO^+ and N_2^+ spectra. There seems no doubt that these ions are formed near the nucleus and are then driven off by light pressure. The CO^+ system (known as the "comet-tail" band system) is generally stronger than the N_2^+. Whether this is due to greater abundance of CO^+ or to a higher transition probability is not yet known.

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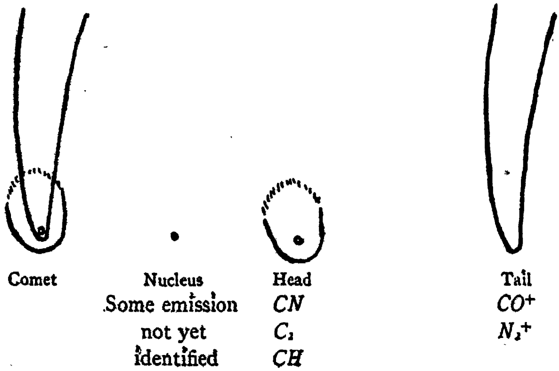
[1] *Astrophys. J.*, **89**, 283–319, March 1939.



Schematic representation of the distribution of the emitting molecules in comets.

The explanation of the luminosity of comets as a fluorescence phenomenon—first suggested by Schwarzschild—is supported by Wurm. Undoubtedly the density of a comet is extremely low and collisions can be of no importance. Taking into account the direct influence of solar radiation, and ionisation and dissociation by light absorption, Wurm accounts for, firstly, the appearance of C_2 molecules—by photo-dissociation of a "parent-molecule" which might be dicyanogen or acetylene. Secondly, for the different extensions of the various molecules—because of different velocities originating in the process of photo-dissociation. Thirdly, for the contraction of the head with approach to the sun—here it seems that the C_2 and CN molecules must be either dissociated or ionised by absorption of light. Thus the life of these molecules will be shortened on approach to the sun (with the accompanying increased intensity of radiation) and with shortening of the life there will also be a decrease in the distance travelled from the nucleus before dissociation occurs. Such an explanation also accounts quite naturally for the non-appearance of CN and C_2 molecules in the tail.

It is interesting to note, as pointed out by Bobrovnikoff, that so far there is no evidence for the existence of any polyatomic molecules in the stars. As regards diatomic molecules to date, twenty-seven of these have been definitely identified in stellar spectra, and most of these occur in the spectrum of the sun. It must be remembered, however, that conditions, in general, are not favourable for work on molecular spectra in the stars, in most cases the dispersion usable being such that only band edges can be dealt with. In contrast with cometary spectra ionised molecules are conspicuous by their absence—the bands observed being resonance bands of neutral diatomic molecules, a consequence of the thermal excitation. Work of this type leads naturally to the question of why certain molecules have strong bands in stellar spectra whilst others have faint bands or none at all. The problem has been tackled by Russell, assuming that stars in general are of the same composition as the sun, but it is obvious that both laboratory and observational data will have to be very greatly improved before much headway is possible.

A new scheme for obtaining an indication of stellar temperatures is put forward by Morgan. He has found that in the case of the brighter K and M dwarfs there is a definite connection between the appearance of the various TiO bands and the temperature or luminosity. He concludes that from the spectral type alone, determined from the intensity of the strong TiO bands in the visual region, the following data concerning these dwarfs is obtainable —absolute magnitude, effective temperature and stellar diameter.

As with most symposiums for every question that might be regarded as settled several new ones were raised, and one of the most useful aspects of the work of the Yerkes symposium must be the attention which has been drawn to the many problems in astrophysics, the solutions of which depend upon an improved understanding of the parts played by compounds in stellar atmospheres.

Salmon Fishery Research (W. L. Calderwood)

The two sea trout papers by Mr. Nall are of interest since the one deals with the fish of an east coast river with a long estuary, and the other with a typical west coast river. With the presence of the estuary, we have the "false checks" on the scales which are often so puzzling. In illustrating the Conon scales (1), we have two figures from the same fish at different stages which would puzzle most scale readers if the marking particulars had not been available. In the west coast fish (4), the heavy weights instanced are 14 lb. from the Carron, 16½ from the Ailort and from the Ewe there is a fish which has survived 19 summers.

The paper by Mr. Hartley (5) deals with the data collected from scales of the 1937 Fishery Board marking at Loch Inchard, 12 miles south of Cape Wrath on the west coast. The movements indicated by the marking and recapture of the fish is the subject of Mr. Menzies' paper (2). A fair proportion of the fish went to the east coast, but in this, as in the other paper by Mr. Menzies (8) the particulars of marking and recapture are not given, which we think unfortunate, particularly with reference to the results obtained from the Ardnamurchan operations, where ready reference to the data respecting lines of movement would have been advantageous. Also the, otherwise excellent, map in the first paper (2) does not explain itself for want of some caption. The more important paper on Movements is that respecting the Soay and Ardnamurchan marking. These came as a surprise to all interested in the subject. The Soay operations at the south of the Isle of Skye, were confined to the months of April and May, a time of year when west coast rivers are commonly without salmon. The Fascadale or Ardnamurchan marking was carried on in June and July. It was perhaps natural to suppose that, south of Skye, marked fish would have been moving south. Instead of this, 18 of the 22 recaptures from Soay went away north, the recaptures being made along the north coast and in rivers of the north coast, in the Moray Firth, and south as far as the North Esk district and the Tay (400 miles). Of the two fish which remained on the west coast, one was caught at Ardnamurchan, only 25 miles away, and the other in Loch Killisport where no spring fish could be expected, but where it was caught in ? September, or four and a half months after marking, the date of which is not given.

The Fascadale marking was very largely of grilse, and the fish behaved quite differently from the spring fish at Soay. Five were recaptured at the place of marking, and other five at Soay, seven were on the shores of the Isle of Mull, and four in the rivers of Loch Linnhe. Raasay, Snizort in Skye, and Easdale had each one. Three went to the rivers of North Ireland, and one was taken in a tributary of the Conway in North Wales. These were all interesting and significant recaptures of a new order. Added to them, however, there were recaptures on the east coast, in the Moray Firth, and in the River Tay. It is noticeable that no indication appeared as to fish moving to the Solway. The maps of this paper are clear and instructive, and in that showing the Soay returns, the two winters fish are distinguished from the 2 + fish. There is also a third map in which the results of the marking at two places in the north west coast are compared with the Fascadale or Ardnamurchan results. It is to be hoped that further operations will be possible in order to elucidate lines of movement with greater certainty.

Mr. Macfarlane contributes a paper on scales from fish taken in the Upper Solway in 1934 (3). The collection consisted of 1890 scale samples taken at Newbie from February to September. By correlating the data with percentage figures of the total catch, the actual representative value of the collection was ascertained. The lowest percentage of previously spawned fish was here found, 1·1 per cent. A certain comparison with Nith and Dee scales previously examined has been possible, and the calculated smolt lengths are large as in those rivers. This appears to indicate better early feeding than in east coast conditions.

The combined paper on the Bacteriophage (6) shows extended studies of river waters in which the phage has been found, but gives a negative result in connecting it with Furunculosis.

The paper on the absorption of bones in the skull of the salmon at or towards breeding time (7) is a short paper with rather inconclusive result. There is growth of other skull parts for which calcium is required, and there is also growth of " breeding teeth." The question of absorption has received a good deal of attention but is not yet fully understood.

1. " Sea Trout of the River Conon." By G. Herbert Nall.
2. " The Movements of Salmon marked in the Sea. II: The West Coast of Sutherland in 1937." By W. J. M. Menzies.
3. " Salmon of the Upper Solway District, 1934." By P. R. C. Macfarlane.
4. " Sea Trout of the River Carron and Loch Doule, Western Ross-shire. By G. Herbert Nall.
5. " Salmon Caught in the Sea—West Sutherland, 1937." By G. W. Hartley.
6. " Bacillus salmonicida Bacteriophage: with particular reference to its occurrence in water and the question of its application in controlling *B. salmonicida* infection." By M. H. Christison, I. Mackenzie, and T. J. Mackie.
7. " The Absorption of Bones in the Skull of Salmon during their Migration to Rivers." By Dr. V. Tchernavin.
8. " The Movements of Salmon marked in the Sea. III: The Island of Soay and Ardnamurchan in 1938." By W. J. M. Menzies.

Paramagnetic Absorption and Dispersion (L. F. Bates)

It is well known that when we measure the dielectric constants of certain liquids the values obtained are found to depend upon the frequency of the applied field, and in recent years Debye has given a very satisfactory picture of many aspects of dielectric absorption and dispersion on the basis of the dipole theory. On this theory it is easy to understand how the dielectric constant of a liquid containing polar molecules may rise to a sharp maximum as the frequency of an applied field is increased from zero, and why a dielectric exposed to an alternating field should become heated.

Now it has recently been found that analogous phenomena of absorption and dispersion exist in the case of the magnetic permeability of iron ammonium alum, but, whereas electrical dispersion may in general be observed at room temperatures, it is necessary to cool the paramagnetic substance. Thus, C. J. Gorter (*Zeit. für tech. Phys.*, **19**, 501, 1938) placed the paramagnetic salt within a coil which formed part of an oscillatory circuit and observed a frequency change, which was measured by the heterodyne heat method, corresponding to the increase in the self-inductance of the system. Due allowance was, of course, made for capacity changes brought about by the introduction of the salt, obtained by replacing it with a diamagnetic of similar chemical composition. Frequencies between 0·3 and 5 megacycles per sec. were generally used, and the salt was maintained at the temperatures of liquid nitrogen and of liquid hydrogen. Gorter found that with iron and chrome alums the quantity χ/χ_0 decreased by about 75 per cent. as a steady magnetic field, applied parallel to the axis of the coil, was increased up to 5000 oersteds; χ is the value of the incremental susceptibility measured by the high frequency field while χ_0 is the value in a static field. W. J. de Haas and F. K. du Pré (*Physica*, **5**, 501, 1938) found that similar phenomena occurred with frequencies as low as 15 to 40 cycles per sec. at liquid helium temperatures.

The existence of the phenomena described above means that when an alternating magnetic field is applied to a paramagnetic, there must be a component of the induced magnetic moment which is 90 degrees out of phase with the field, and, consequently, heat must be generated in the material. F. Brons and C. J. Gorter (*Physica*, **5**, 60, 1938) measured the heat W generated in a specimen by means of a special calorimeter, when a steady field H_c was applied to it and the material was simultaneously exposed to a small alternating field of frequency ν. They obtained curves showing the relations between W/ν and H_c with ν constant, and between W/ν and ν with H_c constant, which are briefly discussed below.

A very neat theory of the phenomena has been given by P. Debye (*Phys. Zeit.*, **39**, 616, 1939), based upon the idea, due to H. B. G. Casimir and F. K. du Pré (*Physica*, **5**, 507, 1938), that if electron spins are responsible for the paramagnetism and do not completely follow the alternating field, then there must exist at any instant a temperature difference θ between the absolute temperature T of the crystal lattice and that of the electron spins, so that the heat imparted to the lattice in a time *dt* is given by $\alpha \,.\, \theta \,.\, dt$, where α is a constant. Debye shows that the ratio χ/χ_0 is equal to

$$\frac{1 + \frac{j2\pi\nu}{\alpha}C_\sigma}{1 + \frac{j2\pi\nu}{\alpha}C_H},$$

where C_σ and C_H are the specific heats of the material at constant magnetisation and at constant field respectively. For the special case of a ferromagnetic which obeys Curie's law rigorously, we have $\chi = C/T$, so that $C_\sigma = o$, and the real part of χ/χ_0 may be written equal to $1/1+\nu^2\rho^2$, where

$$\rho = \frac{2\pi}{\alpha}C_H.$$

The last expression for χ/χ_0 was deduced by Casimir and du Pré, and the quantity ρ is termed the *relaxation constant.* However, even at very low temperatures, and where the Curie law does not obtain, as long as the permeability in a steady field is a positive quantity, C_H must be greater than C_σ, so that the expression for χ/χ_0 does not vanish but merely becomes less than unity as ν tends towards infinity. In general, however, the decrease of χ/χ_0 with increase in frequency is not as rapid as the expression suggests. Moreover, we deduce from the complex expression given above that the value of W per gm. in a small alternating field of amplitude h_0 should be equal to

$$\frac{\pi h^2{}_0\rho\nu^2}{1 + \nu^2\rho^2}\left(\frac{C_H - C_\sigma}{C_H}\right).$$

We may say that the experimental results of Brons and Gorter with iron and chrome alums suggest that the heat generated in the paramagnetic is the sum of two terms, one constant and the other of the form given above, but it would appear that the last expression has not yet been thoroughly tested in the light of existing data, for the phenomena are but newly discovered. *A priori* it would be expected that cæsium-titanium alum would show the phenomena to a marked extent, yet experiment shows them to be practically non-existent at liquid nitrogen and liquid hydrogen temperatures. R. de L. Kronig (*Physica*, **6**, 33, 1939) has explained this on the basis of a spin orbital coupling, but it may be that the explanation is equally well provided by Debye's expression for W.

International Co-operation in Astronomy (A. Hunter)

In a recent note to *Science* (**87**, 364, 1938), Dr. O. Struve analyses present and former mailing lists of the *Astrophysical Journal* and finds a decline of 25 per cent. in foreign subscriptions during the last decade. Arguing that international collaboration in science depends largely on the exchange of publications, he deplores this fall as evidence of a trend away from co-operation. In support of this view, he points out there is a marked correlation with the appearance of nationalistic movements, the largest declines having occurred in subscriptions from China (80 per cent.), Russia (70 per cent.), Japan (50 per cent.), Italy (40 per cent.), and Germany (30 per cent.). Smaller decreases have occurred in English, French, and Dominion subscriptions, but the U.S. numbers are almost stationary.

Astronomy is perhaps more dependent on international co-operation than is any other science. The problems it is called upon to solve are, by their very nature and magnitude, insoluble except by co-operative effort from all parts of the world. Nationalism will therefore be most apparent, and will have the most disastrous results, in this science, and prompt measures should be taken if any symptoms can be clearly shown to exist. But an editorial note in the English astronomical magazine *The Observatory* (**61**, 223, 1938) puts a less sinister interpretation on the situation. This journal sells only about a quarter of its subscriptions in England, and its foreign subscriptions are actually on the upgrade, new readers coming from all parts of the world. The somewhat more popular treatment of astronomical matters in *The Observatory* may, however, be a factor here. As modern astrophysics and astrometry grow less and less comprehensible to the amateur, he is likely to withdraw his support more and more from the technical journals whose primary appeal is to the specialist. In any case, the causes suggested by Dr. Struve for the decline in *Astrophysical Journal* subscriptions—financial difficulties connected with militarism, and legislative restrictions on foreign purchases—should presumably operate unfavourably with the English journal whatever the nature of its subject-matter, whereas no such tendency is detectable.

In view of this divergence of opinion, it becomes of interest to examine other aspects of the same subject—society membership, for example. In the last ten years, the overseas membership of the

Royal Astronomical Society has shown a steady increase, though the total membership has declined somewhat. The proportion of foreign members in the Society has therefore increased markedly. As a result of this, overseas circulation of the Society's *Monthly Notices*, a publication of much the same standing as the *Astrophysical Journal*, has gone up. A more direct indication of the state of affairs is afforded by the present position of the International Astronomical Union. Triennial general assemblies of the Union are held, in one or other of the various adhering countries, for the purpose of taking unified action on draft reports which have been drawn up by international correspondence between the meetings. The amount of work done at these assemblies has been steadily increasing since the birth of the Union, whether it is measured by the number of decisions taken or by the scope of those decisions. The last meeting, held at Stockholm in August 1938, showed a record attendance of 282 delegates from 29 countries. There seems little evidence here for any decline in international co-operation.

The fact remains, of course, that the *Astrophysical Journal* is not as widely distributed in foreign countries as it was formerly. The United States, possessing as it does the biggest telescopes in the world, has contributions of great significance to make to our knowledge of the universe, but their value depends largely on the extent to which they are disseminated. The matter is serious, then. Nor is it one of interest only to astronomers : it is vital that we should maintain channels for the exchange of scientific information of all kinds between America and other countries. American isolation has in the past been disastrous politically and economically. At a moment when these barriers are at last being broken down, it would be doubly disastrous if they should be replaced by scientific ones.

Miscellanea

The list of those elected Fellows of the Royal Society at the meeting held on March 16 is as follows : G. S. Adair, physiologist, Cambridge ; C. H. Andrewes, pathologist, National Institute for Medical Research ; M. Born, mathematical physicist, Edinburgh ; A. J. Bradley, physicist, Cavendish Laboratory ; D. Brunt, meteorologist, Imperial College of Science ; F. A. E. Crew, geneticist, Edinburgh ; F. W. Edwards, entomologist, British Museum ; B. M. Jones, aeronautical engineer, Cambridge ; G. W. C. Kaye, physicist, National Physical Laboratory ; E. G. T. Liddell, physiologist, Oxford ; E. J. Maskell, plant physiologist, Cambridge ; I. Masson, chemist, Sheffield ; C. E. K. Mees of the Eastman Kodak Co., New York ; M. H. A. Newman, mathematician, Cambridge ;

H. H. Read, geologist, Imperial College of Science; Sir R. G. Stapledon, botanist, Aberystwyth; H. M. Turnbull, anatomist, London Hospital; E. E. Turner, organic chemist, Bedford College, London; V. B. Wigglesworth, entomologist, London School of Hygiene and Tropical Medicine; E. J. Williams, physicist, Aberystwyth.

H.M. the King has approved the award of the Founder's medal of the Royal Geographical Society to Mr. A. M. Champion for his surveys of Turkana Province and the volcanoes south of Lake Rudolf, and of the Patron's medal to Prof. H. Ahlmann, Stockholm, for his work in the Arctic.

Other medals and prizes awarded during the past quarter included the James Scott prize of the Royal Society of Edinburgh to Prof. P. A. M. Dirac; the James Alfred Ewing medal of the Institution of Civil Engineers to Prof. A. H. Gibson of Manchester; the gold medal of the Linnean Society of London to Prof. E. D. Merrill, professor of botany in Harvard University, and the Perkin medal of the Society of Dyers and Colourists to Mr. J. Baddiley of Imperial Chemical Industries, Ltd.

Prof. F. G. Donnan has succeeded Sir F. Gowland Hopkins as president of the Association of Scientific Workers and has been elected an honorary member of the Société Philomathique of Paris.

Prof. H. C. Plummer has been elected president of the Royal Astronomical Society for the coming session, Sir Robert Robinson, president of the Chemical Society, Mr. W. A. S. Calder of the Institute of Chemistry, Prof. H. H. Swinnerton of the Geological Society of London, Dr. Allan Ferguson of the Physical Society, Sir Sidney F. Harmer of the Ray Society, Prof. W. H. Roberts of the Society of Public Analysts and Other Analytical Chemists and Mr. W. E. Watson-Baker of the Quekett Microscopical Club.

Dr. J. M. Stagg has been appointed superintendent of the Kew Observatory, Richmond, in succession to Dr. F. J. W. Whipple who has retired under the age limit.

We have noted with great regret the announcements of the death of the following well-known scientific workers during the past quarter: Prof. H. Balfour, F.R.S., Oxford University, anthropologist; Mr. E. R. Bolton, chemist; Sir William Butler, brewer; Dr. L. H. Dudley Buxton, Oxford University, anthropologist; Mr. Howard Carter, egyptologist; Prof. F. J. Chesire, F.R.S., physicist; Prof. A. P. Coleman, F.R.S., University of Toronto, geologist; Dr. J. E. R. Constable, National Physical Laboratory, physicist; Prof. W. Cramp, University of Birmingham, electrical engineer; Prof. H. A. Cummins, University College, Cork, botanist; Prof. H. M. Dawson, F.R.S., University of Leeds, physical chemist; Prof. J. W. Edington, University of Sheffield, bacteriologist; Dr. L. Fabry, astronomer; Mr. H. J. George, University of Oxford, chemist; Dr. W. E. Grimshaw, deputy-director of ballistics research, Woolwich; Dr. E. H. Hankin, bacteriologist; Prof. G. Hellman, of Berlin, meteorologist; Prof. A. M. Hocart, University of Cairo, social anthropologist; Prof. C. M. Jessop, University of Durham, mathematician; Prof. C. C. Okell, University College Hospital Medical School, bacteriologist; Prof. Joji Sakurai, Imperial University, Tokyo, chemist; Prof. K. Schröter, Federal College of Technology, Zürich, botanist; Prof. A. Smithells, F.R.S., University of Leeds, chemist; Prof. S. P. L. Sørenson, director of the Chemical Dept., Carlsberg Laboratory, Copenhagen; Prof. G. Tamman, Göttingen, physical chemist; Eng.-Commander J. J. Walker, entomologist; Prof. E. B. Wilson, F.R.S., Columbia University, zoologist.

The Stanton Ironworks Co. of Nottingham has been awarded the contract for the supply of the 52 miles of high-pressure gas mains required for the main underground artery of the first stage of the West Yorkshire Gas Grid. It is intended to manufacture coal gas at the pitheads and to distribute it to consumers through the usual supply companies. The order comprises some 20,000 large-diameter pipes weighing up to 16 cwt. each and tested to withstand an air pressure of 60 lb. wt. per sq. in.

The Illumination Section of the Science Museum at South Kensington has been increased by an additional 3000 sq. ft. of exhibition space, partly so that the illumination collections may be made thoroughly representative of modern practice and partly to exhibit in a worthy fashion the "Bryant and May" collection of fire-making appliances which has been lent for permanent exhibition in the Museum.

The Bryant and May collection is unquestionably the most complete in existence and is of world-wide interest. Practically every known method of making fire, from primitive wood friction devices to the modern safety match is represented. Among the more unusual exhibits are an alarm-clock, over two hundred years old, which fired a trigger that exploded a charge of gunpowder which lit a candle that sprang up and lit the owner out of bed; a gold tinder holder from China, studded with opals; fire pistons from the

South Seas that worked on the same principle as the bicycle pump which gets hot as it is pumped; the original record of sale and some original friction matches made by John Walker of Stockton in 1827.

The Illumination Section begins with an historical display tracing the story of the candle, oil lamp, acetylene burner, gas and electric light, all arranged in well-lit cases showing stages in development. Among the many notable exhibits are such interesting objects as dried birds used as oil lamps in the Shetland Islands; a lighthouse lamp with parabolic reflector dating back to 1763; part of an illuminated sign made to celebrate the battle of Waterloo in 1815 (it contained over 1300 oil lamps); and one of the arc lamps used in the pioneer scheme outside the Royal Exchange in 1881.

Following this section is a number of automatic demonstrations of the principles which must be understood if good lighting is to be obtained. Most of these demonstrations, including rooms illustrating decorative lighting and the various types of lighting from fully direct to fully indirect, were prepared for the Electric Illumination Exhibition held at the Science Museum some eighteen months ago.

The *Report of the National Physical Laboratory* for 1938 (H.M. Stationery Office, 2*s*. 6*d*. net) contains the usual summary of the more important investigations either completed or brought to a definite stage in their progress during that year. In the Heat division the freezing point of rhodium has been determined and the result obtained, 1965 ± 3° C., is indistinguishable from that previously obtained at the National Bureau of Standards. A method for the determination of the temperature of liquid steel in the furnace (*c*. 1650° C.) has been developed. A platinum thermocouple is used in conjunction with a "quick-immersion" technique. Preliminary experiments showed that the thermocouple, lightly sheathed in silica, withstood immersion for 20 sec. at temperatures up to 1690° C. without fracture of the sheath or apparent depreciation of the couple. The sheath tended to collapse but could be used for 12 immersions without renewal. Ten to 15 sec. sufficed for the couple to reach temperature equilibrium and the results obtained were in good agreement with those given by an optical pyrometer working under black-body conditions.

An investigation of a rather different kind originated in a request from the Receiver for the Metropolitan Police District for suggestions for improving temperature conditions in police telephone boxes. Considerable improvement resulted from the use of a false ceiling of ¾-in. wall board covered on both faces with aluminium foil.

The testing departments dealt with more than half a million clinical thermometers, over 22 gm. of radium and more than eight times as many instruments required for electricity supply measurements as in 1936. Fewer taximeters were tested and only one meter was submitted for a Class I test.

The reason for the poor acoustical properties of the Assembly Hall of the League of Nations at Geneva was investigated and it was discovered that the plan finally adopted was not among those submitted to the Architectural Acoustics Committee at various times during the eight years in which the design was under consideration !

Experiments in the Electricity Department showed that the risks of explosion due to generation of static electricity in dry-cleaning processes can be reduced by using soaps which increase the conductivity of the cleaning solvents, and a portable instrument showing whether the conductivity is adequate was devised. The Metrology Department completed the work on the value of g at Teddington, the final value being $981{\cdot}181_5 \pm 0{\cdot}0015$ cm./sec.^{2} The Metallurgy Department investigated the Seger cones used for temperature measurement in the fire-brick industry and found that, with proper use, temperatures over the range 1560°–1760° C. could be measured with an accuracy but little short of that obtainable with a good optical pyrometer. Investigation of the inorganic constituent of tooth enamel showed that it is mainly hydroxyapatite in which a small proportion of the calcium atoms is replaced by magnesium.

Work in the William Froude laboratory proved that by suitable design the fuel consumption of small 70–80 ft. motor driven drifters can be reduced by some 40 per cent. Thirty-seven designs only were tested in this laboratory during the year as against 68 in 1937 and 88 in 1936—the peak year.

The Department of Scientific and Industrial Research has issued three further leaflets in the series describing standard methods for the detection of poisonous gases produced in industrial processes. They deal with sulphur dioxide, benzene and nitrous fumes. A concentration by volume of 1 part of sulphur dioxide in 2000 is dangerous for even short exposures and 1 part in 100,000 is the maximum concentration allowable for several hours' exposure. Concentrations down to 1 part in 250,000 can be estimated by using test-paper treated with starch and potassium iodate mixed with potassium iodide.

Benzene acts as a narcotic in high concentrations, while prolonged exposure to low concentrations affects the blood and blood-forming organs of the body. Individual susceptibility is well recognised, women and young persons being particularly susceptible. Concentrations of from 1 to 200 to 1 to 500 have been found in factories where poisoning has occurred. Concentrations down to 1 in 10,000 can be detected by absorbing the contaminated air in concentrated sulphuric acid containing a trace of formaldehyde.

Nitrous fumes are extremely dangerous on account of their insidious character. There may be, and generally are, no immediate effects and therefore it is impossible to foretell the serious consequences that may result from the inhalation of these fumes. A workman, unaware that he has inhaled the fumes, continues at work, often remaining well until after he has returned home. Some hours later he becomes restless with a dry cough and shortness of breath. These symptoms increase, accompanied by a frothy sputum tinged with blood. If appropriate treatment is not applied, death follows from œdema (waterlogging) of the lungs.

Concentrations stronger than 1 in 10,000 are frequently fatal if breathed for more than a few minutes. It is therefore most important to note that a concentration which is dangerous to inhale for even a short time may be hardly noticeable, because no disagreeable symptoms may be produced. For this reason any atmosphere in which nitrous fumes are noticeable either by smell, irritation, or colour, should be regarded as dangerous. The method of test which has been developed will easily detect a concentration of 1 part in 100,000. It is based on the Griess-Ilosvay reaction and is carried out by drawing the atmosphere under test by means of a hand pump through a tube containing the reagent (a mixed solution of α-naphthylamine and sulphanilic acid in acetic acid) until a rose-pink colour of standard depth is reached. From the number of strokes of the pump required to produce the standard colour, the concentration of nitrous fumes present can be obtained by reference to the table given in the leaflet which, with the others in the series, can be obtained from H.M. Stationery Office.

Food Investigation Special Report No. 47 (H.M. Stationery Office, 2*s*. 6*d*. net), entitled *Microbiology in the Preservation of the Hen's Egg*, contains a most interesting account of the reasons for the deterioration of eggs and its prevention. " Mustiness," " fishiness " and " cabbage-water " in the white are micro-biological in origin. The cause of the " strong " flavour in the yolk is not yet certain but that which appears after about seven months' storage is probably due to the absorption of odours from the packing, e.g. the straw board fillers. The " water-glass " method of storage is perhaps the best, eggs having been kept in good condition for several years in this way, but the method is not very convenient for use on a commercial scale.

The March number of the *Journal of Research* of the National Bureau of Standards (Vol. 22, No. 3) contains an interesting paper by Hoge and Brickwedde on the establishment of a temperature scale for the calibration of thermometers between 14° and 83° K. They compared platinum and platinum + 10 per cent. rhodium resistance thermometers with a helium gas thermometer and reduced the results to the thermodynamic scale by the Leiden method of extrapolation to zero pressure using Henning's results for the B coefficient.

The January issue of the *Bell System Technical Journal* contains another paper by Karl K. Darrow—this time dealing with the nature of cosmic rays and the mesotron.

ESSAY REVIEW

KARL PEARSON. By E. C. RHODES, D.Sc. Being a Review of **Karl Pearson : An Appreciation of Some Aspects of His Life and Work,** by E. S. PEARSON. [Pp. viii + 170, with 9 plates.] (Cambridge : at the University Press, 1938. 10*s.* 6*d.* net.)

No one person can write adequately on the subject of another. I first met K. P. in 1919, when he was 62 years old. At this age, present-day professors are looking forward to retirement or they have already retired. I went to see him at University College about the prospect of working under him as an assistant in his department. He was housed at that time in the older part of the College, and told me of the new buildings which were about to be taken over by his staff. He was at the beginning of a new life ! I realised that he was my ideal of the scientist, someone with whom one would serve an apprenticeship, someone who would initiate the pupil into the mysteries of his craft and who would expect at the same time skilled work and service from the apprentice. In the next few years, I learned what it was to keep going through a maze of algebra, where the answers did not always emerge in the simple form reserved for the text-book or the examination-room, and I learned that one may spend some days computing before an arithmetical result was achieved. But what a thrill when one did arrive at the answer, feeling confident that, by constant checks during the process of calculation, the result was accurate !

When one has contacts with such a man as Karl Pearson was, one naturally is interested in his scientific history. Why and how did he start doing statistics ? How did Galton become interested in such a subject ? I used to get some interesting tit-bits of early history in lunch-time conversations. I remember once discovering that for years he had taught engineers to use graphical methods, in the days when the engineering student would not be expected to be acquainted with the differential calculus.

To me, the book by his son, Egon Pearson, giving an account of the life and work of his father is especially interesting. It tells me much about K. P. which I did not previously know, and it traces his scientific life through its various phases.

I got the impression, later confirmed by Karl Pearson's speech at a dinner held in his honour at University College in 1934, that his appointment as Professor of Applied Mathematics at University College was to lead to a life which he had not altogether planned. He gave me the idea that there were two young men, himself and another, working at the law and other things, that one of them, K. P., devoted himself to mathematics but should have stuck to law, and the other kept to the law but should have remained a mathematician. What contradictions appear to be present in a situation where a philosopher, and K. P. seemed to be more interested in philosophy in 1884 than in other things, undertook the teaching of Applied Mathematics and Mechanics to Engineering Students. How often would such an appointment be made to-day ? Moreover, and here perhaps one's recollections are not altogether accurate, the old system of salary payments differed from that of the present time. In the old days, a professor had a comparatively small fixed stipend, but he took a share of his student's fees, so that his success as a lecturer was to affect his income. Judging by extracts from letters written on Pearson and his work at that time, which are quoted in the book, his students in the 1880's appreciated his work for them.

It is interesting to read of the origin of the *Grammar of Science*, a book which ranged far beyond its country of origin and which to-day is available in the " Everyman " series. Pearson became " Gresham Lecturer in Geometry " in 1891, and the subject matter of the lectures became the foundation of the book. E. S. Pearson reproduces in an appendix to his account of K. P.'s work a full syllabus of these lectures, and one feels strongly that such lectures should be available for all university students of the present day. In a way, one regrets that such courses are available in book form (instead of lectures), because students with so much to read, neglect an essential part of their education when they fail to place in the general scheme of scientific thought the little part of knowledge on which they concentrate. The Victorian student certainly appears to have had a broader training than the student of to-day.

At this time in his life Pearson was in contact with both Weldon and Galton. Measurements of variation amongst individuals in a group, measurements of correlation, of regression and the way in which these variations were distributed were subjects of deep interest. We get the beginnings of the Pearson system of Frequency Curves and the product moment method of correlation applied over and over again to all kinds of material. We get the subsequent development of other methods of computing the correlation between pairs of variables, the methods being modified to suit the form in which the material of investigation appeared. All the time, the given mathematical problem was determined by the scientific problem. It is well that this should be realised, because the methods invented and used in the past have since been used when the scientific problem to be solved does not properly lend itself to solution by these methods. Perhaps the correlation coefficient has been the most abused. The computation of this measure is a purely mechanical one. Given the correlation process, any number of pairs of values of x and y may be fed to the machine, and when this has done its work, out will come the correlation coefficient. But in many cases it may not mean anything.

It is somewhat astonishing that Pearson never delved further in the ground which lay alongside the correlation field. He wrote on lines and planes of closest fit, where x and y were two variables assumed to be connected by a functional relationship $f(x, y) = 0$, but where observations of pairs of values of x and y were both subject to error. This early work never seems to have been followed up by him. One gathered that he thought that all relationships were of the nature of correlation rather than causation, and, if this is so, perhaps therein lies the explanation.

One often reads in books on elementary statistical methods when an author is dealing with various methods of describing frequency distributions, that the arithmetic mean and the standard deviation are preferred to the median and the quartile deviation, amongst other reasons, because they lend themselves more easily to further algebraical manipulation. It is pure speculation, of course, and perhaps impertinent, but one often wonders what leads a scientist faced with a choice between two hypotheses to choose one rather than the other. Is it that one hypothesis enables practical results to be deduced, and the other is sterile? Is the first then explored further in order to find reasons for its superiority over the second?

One of the most elegant pieces of algebra for which Pearson was responsible was the development of the χ^2-test, and this gave him a powerful weapon with which to test his results in curve-fitting. The manner in which his system of curves would fit all kinds of biometric distributions was astonishing. There certainly seemed to be some divinity at work, and yet, the basis on which the fundamental differential equation was built was really arbitrary. There was a connection with a chance distribution, but it was a peculiar one. To the practical man, here was a convenient method of smoothing out irregularities in a statistical series. The philosopher, perhaps, got no nearer the reasons why a particular phenomenon was distributed in a certain way, but he might be able to assert with confidence that there was some lack of homogeneity in his data. Perhaps, to Pearson, one of the most cogent reasons why he developed his system of curves was his awareness of the weakness of the normal law of errors which had reigned supreme for so long and which was assumed over and over again to be in operation, when observations showed the contrary.

Mention must be made of a *modus operandi* which he recommended to his pupils—to work at a problem *de novo*. When the pupil, equipped with the tools of his trade, had applied his powers to the solution of a problem, then was the time to read what others had done. The pupil, perhaps, found that the masters had done all that he had done, and more, in which case he learnt in the best manner the work of the masters. Maybe, the pupil tackled the problem in a different way from that of his predecessors. The pupil's method may have been the better method. In any case, one was not guided the whole time into the grooves made by others. Perhaps, this method of working, which presumably was Pearson's, explains why his work on frequency curves was independent of the work of the Scandinavian school which concerned itself with modifications of the normal law of error, and that of Edgeworth, which was also concerned with similar modifications.

Further, mention should also be made of Pearson's writings on the interpretation of statistical data. K. P.'s name will go down to posterity mainly on account of his influence on the developments of statistical methods, but he devoted a considerable amount of time to writing and lecturing on the implications of statistical data. His training led him to object vehemently to a thesis founded upon an imperfect basis of fact, or a wrong interpretation of facts, especially facts which had been made susceptible to the counting process. His training, moreover, gave him a deep acquaintance with many technicalities outside mathematics, and he was a doughty opponent in argument. In general, a discussion of a social problem is conditioned by a knowledge of certain facts, which may only refer to aspects of the problem. Some of the information which is available may be of the nature of statistics, which may be peculiar in the sense that the thing measured may carry an artificial definition or the things counted may be special stigmata of the problem. The point is that the statistical data may require very careful handling before the problem is supposed solved. One has only to think of historical series of birth-rates and their relation to various Factory Acts, or the state of civilization in a country being measured by the percentage of illiterates to envisage the difficulties involved in the interpretation of statistics. Pearson's incursions into various controversial fields are dealt with objectively by his son in the present book.

E. S. Pearson is to be congratulated on his appreciation of his father's work. A difficult task has been well done. There is a modern tendency for autobiographies to be written by young men. In a sense, K. P.'s autobiography is in his writings, from his early interest in Philosophy to his later hobby of skulls. We miss, however, in his writings, the human touch which is everywhere to be seen in the pages of the present work. Statistics to the layman may appear to be dry, to the statistician they are living things, willing to speak their secrets to the person who can handle them in the right way. Such a person is interested in how the earlier statisticians lived and why they were impelled to consider certain problems. As far as Karl Pearson is concerned, most of the answers are in the present book, which takes its rightful place in the history of statistics, since the history of a subject is the history of those who developed the subject.

I started out by saying that I met Karl Pearson in 1919 when he was 62. He died in 1936 when he was 79. He retired from his Chair at University College in 1933, but he still retained a little harness. I had gradually learnt something about his versatility and his output, but full realization only came with reading E. S. Pearson's book. K. P. stands out as a giant amongst pygmies.

REVIEWS

MATHEMATICS

Solid Mensuration with Proofs. By WILLIS F. KERN and JAMES R. BLAND. Second edition. [Pp. viii + 172, with frontispiece and figures.] (New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1938. 10*s*. net.)

IN producing a second edition of their book, the authors have entirely rewritten the text. The elementary and most familiar solids are taken first and classed in three groups according to the type of formula for the volume. Then come the sphere, volumes and surfaces of revolution, polyhedra and prismatoids. Proofs are based on the lemma : if, in two solids of equal altitude, the sections made by planes parallel to and at the same distance from their respective bases are always equal, the volumes of the solids are equal.

At the end of the book are four figure tables of logarithms and of trigonometric functions. There are many exercises, and answers are provided. The book is well produced ; diagrams are plentiful and clearly help the reader to see the space-structure of objects. Perhaps this accounts for a rather high price.

J. W. A.

Co-ordinate Solid Geometry. By ROBERT J. T. BELL, M.A., D.Sc., LL.D. [Pp. xiii + 175 + xliii, with 46 figures.] (London : Macmillan & Co., Ltd., 1938. 7*s*. 6*d*.)

THIS book is a reproduction of Chapters I to IX of *An Elementary Treatise on Co-ordinate Geometry of Three Dimensions* by the same author. The larger work is a standard text-book, so the present book needs little introduction to teachers who require a treatment of the subject adapted to a Euclidean space. The topics dealt with are : properties of a plane, the straight line, the central quadrics (referred to their principal axes as axes of co-ordinates and going as far as their plane sections and their systems of generators). For a large body of students, this is all that is needed.

The appropriate miscellaneous exercises have been selected out of the larger book and an extra set has been added. There is also an appendix offering simplified proofs in place of some carried over with the original text.

J. W. A.

Advanced Analytic Geometry. By ALAN D. CAMPBELL. [Pp. x + 310, with figures.] (New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1938. 20*s*. net.)

THIS book is a useful complement to the usual English text-book and stresses the idea of a geometry associated with the transformations belonging to a group. The first part is concerned with affine transformations and the second with the general analytical aspect of projective geometry. The discussion ranges over cross-ratio, collineations, conics, systems of conics, and elementary theory of "higher plane curves."

The exposition is detailed and somewhat informal. Almost every section has its set of exercises, these having a numerical bias.

The logical steps involved in the modification of a Euclidean plane with only real points by the addition of complex points and then further by the addition of a line at infinity are not satisfactorily brought out. In keeping with the projective point of view, Part II opens with a demonstration that a co-ordinate system can be attached to a triangle without needing the concept of distance. Since we never get away from a modified Euclidean plane, wherein distance is ready-made, it may be thought that the reader would find things easier if this discussion came later in the book.

In spite of these points, we hope that students will give this book their attention.

J. W. A.

Calculus. By E. S. Smith, M.E., Ph.D., M. Salkover, Ph.D., and H. K. Justice, C.E., Ph.D. [Pp. xii + 558, with 286 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 16*s*. net.)

The technical student with an elementary knowledge of calculus will find this book congenial. It covers the usual ground for differential and integral calculus, with applications to areas, volumes, and mechanical concepts such as vectors and their derivatives, moments of inertia, mean centre and centre of pressure. The physical applications are not just glanced at, but are treated thoroughly for their own sake, and they provide material for the many examples worked in the text. The explanations, for example, in dealing with integration by substitution, are sometimes too sketchy for a student to follow without a teacher, but this is partially redeemed by the detailed examples which are admirably illustrated and very clearly set out. There are valuable sections on curve tracing and asymptotes and on approximate integration. Oddly enough, Pappus' theorems are not given all the limelight they deserve.

Though the text is not thorough or extensive enough for it to be recommended to the mathematical specialist, it is reasonably rigorous and has the modern approach to the elements of analysis. The explanation given that chord and arc are infinitesimals of the same order does not amount to a proof, and the same may be said of the argument whereby the vanishing of $f'(x)$ in an interval is shown to imply the constancy of $f(x)$. But the authors are generally cautious and do not obscure genuine mathematical difficulties. They frequently use a theorem of Duhamel when expressing the limit of a sum as an integral, but since the theorem itself is not proved, it may well be that Darboux's theorem would have been a simpler and a safer rule to give to the reader for which the book is intended.

H. K.

A Course in General Mathematics. By Harold T. Davis. [Pp. xii + 316, with 43 figures.] (Bloomington, Indiana: The Principia Press, 1935; London: Williams & Norgate, Ltd., 1938. 10*s*. 6*d*. net.)

The author states in his preface that this book is a course "for students preparing to become teachers of arithmetic." The algebra discussed includes the solution of linear equations by determinants, arithmetic and geometric series and a very slight account of the number e. The trigonometry covers the ground required for the solution of triangles. Several other topics are included and the chapter on the elements of statistics is of interest. Throughout the book the author has paid attention to the historical aspects of the subject.

One would not expect vigorous proof in a book of this style but it is unfortunate that the author has occasionally been guilty of mis-statement and over-simplification. In particular it is not true that three equations

$$a_v x + b_v y + c_v z + d_v = 0 \quad (v = 1,\ 2,\ 3)$$

are necessarily inconsistent if the determinant $|a_1,\ b_2,\ c_3| = 0$.

C. M. R.

ASTRONOMY

A Hundred Years of Astronomy. By Reginald L. Waterfield. [Pp. 526.] (London: Gerald Duckworth & Co., Ltd., 1938. 21*s*. net.)

The author of this fascinating book takes the reader over the paths trodden by the pioneers of astronomy of the nineteenth century. There are many recent books dealing with the astonishing conclusions of modern astronomy, but we know of none other which explains with such clarity the methods, both practical and theoretical, by which these conclusions have been reached.

The earlier part of the book traces the historical development of the science, the reader being quickly provided with a general view of its present position. Later chapters fill in the details. This arrangement gives the headings of the chapters an appearance of chaos (*e.g.* " Wireless Time Signals, Stellar Evolution, and Relativity ") which is belied by their contents. The varied contributions made to the evolution of the subject by the invention of the photographic plate, the development of the spectroscope, and the co-operation of opticians and engineers in building the giant telescopes of to-day, are each given their due weight. Nor does the author neglect to give the gist of the theoretical advances of the past century, and to point out how international co-operation has made available a store of information on which much of our statistical knowledge is based.

As might be expected in a book of this sort, much the greater portion is devoted to sidereal astronomy. The reader is given connected accounts of how such stellar attributes as magnitude, diameter, colour, temperature, and surface gravity are measured and interpreted; and of how our knowledge of the structure of the universe has been gradually built up from observations of variable stars, nebulæ, and the absorption of light in space. Coming nearer home, the author describes recent developments, no less spectacular, in a more restricted field—the solar system. His position as Director of the Mars Section of the British Astronomical Association is sufficient guarantee of the accuracy of this latter part.

The treatment throughout is non-mathematical and, as far as possible non-technical; but Dr. Waterfield has nevertheless managed to present a balanced view of the whole subject by making clear how great is the debt which astronomy owes to mathematics and physics, without going into details. It is a pleasure to welcome an accurate work of reference which is at the same time a story which can be read with pleasure as well as with profit by both the professional astronomer and the interested layman.

A. H.

Starcraft. By William H. Barton, Jr., and Joseph Maron Joseph. [Pp. viii + 228, with coloured frontispiece and 139 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 10*s* 6*d*. net.)

This book is written, in a somewhat loose style, with a few minor inaccuracies, for American children (a typical chapter heading: "Squinting through the Magic Eye of the Telescope"). It differs from most other similar works in giving detailed instructions on building simple astronomical instruments, in addition to supplying the star charts and guides to the constellations conventional in books of this type.

The interested reader with a mechanical bent can start by making a cross-staff and quadrant, go on to orreries and sundials, and finally, when his enthusiasm has been thoroughly aroused, embark on his own reflecting telescope. For this latter purpose the authors give complete directions for spherically grinding, polishing and testing a 6-inch mirror (the Ronchi test is described), and for mounting it equatorially. The adult amateur will find this section an admirable introduction to such a book as Ingalls' *Amateur Telescope Making.*

The remainder of the book describes what the assiduous observer may expect to see with the naked eye and with a small telescope. Several irreverent but vivid biographical sketches of bygone astronomers and their cosmological theories conclude what may truthfully be called a most unusual work.

A. H.

PHYSICS

The Elements of Physics. By Alpheus W. Smith, Ph.D. Fourth edition. [Pp. xix + 790, with 4 coloured plates, 5 black and white plates and 789 diagrams.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 21*s*. net.)

The fact that this is the fourth edition of a work first published in 1923 shows that the book enjoys the considerable popularity which it deserves and that the author has, in large measure, achieved his aim of writing a text-book dealing with the basic principles of Physics which should "lay a basis for correct scientific thinking rather than to stress the accumulation of facts." There is much to be said for this attitude of mind. There is no doubt that very many students come to regard the subject as a collection of formulæ and rules to be committed to memory, and for them the wood is largely obscured by the trees. The author chooses his illustrations of the fundamental principles from the realms of engineering, agriculture, biology, and topics closely touching our daily life and the result is a vivid exposition which should (and does) stimulate the interest and imagination of the reader.

The book ranges over a very wide field and the treatment is necessarily elementary in character; a slightly more mathematical treatment of certain topics is given however in the appendices. The fact that it has not been written to suit any particular syllabus is both an advantage and a disadvantage. An advantage in that the author has been able to deal with topics which are obviously beyond the scope of an elementary syllabus, but which are of very present interest and importance. A disadvantage in that it is not easy to say for what particular courses in English Schools and Colleges the book is especially suitable. On the whole it is better not to advocate it from that point of view but rather to emphasise the unity of purpose which pervades it and the skilful selection of material from so vast a domain. Mechanics is dealt with in 16 chapters occupying 182 pages; Wave-motion and Sound—5 chapters, 47 pages; Heat—10 chapters, 108 pages; Magnetism and Electricity—14 chapters, 166 pages; Physical and Geometrical Optics—9 chapters, 111 pages; Electronics, Radiation and Atomic Structure—10 chapters, 131 pages. The book is profusely illustrated and is well printed. It can be thoroughly recommended as a general introduction to the subject of Physics.

Sydney Marsh.

A Course of Physics. By Henry A. Perkins, Sc.D. [Pp. x + 820, with 1 coloured plate and 621 figures.] (London and Glasgow: Blackie & Son, Ltd., 1938. 15*s*. net.)

"The chief aim in writing this book has been to explain the difficult portions of physics fully and clearly, to introduce modern physical ideas wherever they can be discussed with some degree of rigour, and to retain those aspects of classical physics which are still valuable as the basis of its fundamental principles." The book certainly does explain clearly the principles of physics, often by reference to well-known phenomena in everyday life. The scope is about the same as that of an Intermediate Course in an English University, though some topics mentioned are not usually met in such a course—for instance the Michelson interferometer and the Zeeman effect. But the treatment is not as full or as rigorous. There are very few accounts of the classical experiments, such as Regnault's in heat. There are no detailed descriptions of apparatus or methods of measurement. For example, magnetometers are not mentioned. For a student working for an Intermediate examination the book must be supplemented by illustrated lectures and (of course) practical work. The text is non-mathematical, but the language of mathematics is used.

Not all the "modern physics" is confined to the chapters at the end of the book. Some takes its place in the text without any flourish of trumpets. A paragraph on the quantum theory follows the kinetic theory of specific heats, the equivalence of mass and energy is mentioned at the end of the chapter on radiation, the Raman effect in connection with fluorescence, and so on. However, it must not be supposed from this that a large proportion of the book is "modern physics."

Nineteen per cent. of the space is devoted to mechanics and properties of matter, 15 per cent. to heat, 11 per cent. to sound, including an excellent chapter on wave-motion in general, 22 per cent. to light, 23 per cent. to electricity and magnetism, and the rest to "corpuscular physics." Suggestions for further reading and problems (with answers) are at the end of each chapter. There is a good index, and the book is attractively produced at a reasonable price.

If an Intermediate student can afford only one text-book, this is not the one to buy if he is working alone. But every student would enjoy reading the book and profit by it, particularly during the transition stage between Matriculation and Intermediate work. It can also be recommended to those who are not going on with the physical sciences but who wish to have a good idea of the basic principles of the subject.

F. A. V.

Grimsehl's Lehrbuch der Physik. Neubearbeitet von PROFESSOR DR. R. TOMASCHEK. Band II, Teil I: **Elektromagnetisches Feld—Optik.** Eighth edition. [Pp. x + 866, with frontispiece, 1 coloured plate and 1,209 figures.] (Leipzig and Berlin: B. G. Teubner, 1938. Price abroad RM. 19.50.)

THE English translation of this excellent text-book with its amazing number of highly interesting figures and diagrams is so well known that the main interest in a review of a new German edition lies in the answer to the question as to whether the work has been brought up to date. It can be stated without reserve that every effort has clearly been made to make the work complete. For example, a description of the van den Graaf machine is included and a very helpful diagram of the mode of action of a cyclotron is given. The modern aspects of the domain theory of ferromagnetism, the Barkhausen effect and Bitter figures are well described. Proper attention is devoted to the electron microscope and experiments with "polaroid" find a place. The reviewer particularly liked the treatment of the theory of dielectrics and the very illuminating diagrams of the production of electromagnetic waves with Lecher wire systems. He still feels, however, a spasm of horror when he reads statements such as "1 gauss = 10^{-8} Voltsekunden/cm.^{2}," but his individual taste should not stand in the way of his thorough recommendation of an excellently printed and sound work.

L. F. BATES.

Statistical Physics. By L. LANDAU and E. LIFSHITZ. Translated from the Russian by D. SHOENBERG. [Pp. viii + 234, with 54 figures.] (Oxford: at the Clarendon Press; London: Humphrey Milford, 1938. 20s. net.)

ACCORDING to the preface the aim of the authors is to give a "unified presentation of thermodynamics and classical statistics, based on the point of view associated with the name of Gibbs." Since almost all books on statistical mechanics follow more or less the line of Boltzmann a modernised treatment of the beautiful ideas of Gibbs is to be cordially welcomed. In their task the authors have succeeded perfectly. The book is confined to the classical theory and Planck's constant does not occur.

After a general chapter on Liouville's theorem and the law of increasing entropy the Gibbs and Maxwell distribution laws are deduced. The difficulties involved in applying the second law of thermodynamics to cosmology are discussed and the result summarised in the following categorical—but undoubtedly true—form: "It is only a closed system obeying statistical laws which has to be in a state of statistical equilibrium. In fact that part of the universe which obeys statistics is not a closed system (laboratory experiments) while the universe as a whole is a closed system but does not obey the laws of statistics." A short discussion of the fluctuation phenomena is followed by applications of the general theory to the chemical equilibrium, the equilibrium of phases, surface phenomena and to anisotropic bodies.

The exposition throughout is very original and clear, often deviating largely from the line usually adopted. The mathematical and physical deductions are kept free from too high a degree of exactness, a fact which makes the book very pleasant to read. (There is, for instance, no lengthy discussion of the hypothesis of continuity of path.)

The research worker will find the book full of interesting suggestions, though it is hardly the type of book which the reviewer would recommend to undergraduate students.

W. H.

Elementary Thermodynamics. By Virgil Moring Faires, M.E., M.S. [Pp. xiv + 225, with frontispiece, 102 figures and 2 folding plates.] (New York and London: Macmillan & Co., Ltd., 1938. 12*s*. net.)

Applied Thermodynamics. By Virgil Moring Faires, M.E., M.S. [Pp. xviii + 374, with frontispiece and 200 figures.] (New York and London: Macmillan & Co., Ltd., 1938. 17*s*. net.)

Probably no branch of technical science is developing at a greater rate than that of thermodynamics. It might be added that there is probably no branch in which it is more difficult to grasp the fundamental principles.

The books now under review are included in Messrs. Macmillan's "Engineering Science Series" and may be described at once as worthy of their place. Although they are not written with any particular examination in view, they may be said to specialise in making clear the fundamentals of heat engines. Few students could fail to appreciate the clear way in which these texts deal with their subject.

The one point which appears abnormal is the subdivision of the subject matter between the two volumes. The titles of the books would lead one to believe that the smaller text was of the nature of an introduction to the second. This, however, is not the case, for, as remarked in the preface to the elementary volume, the author presents "for short courses . . . textual material that is complete for the topics offered." The result is that practically every paragraph of the smaller book is repeated verbatim in the second, except for the omission from the larger book of a valuable psychrometric chart and a collection of problems for solution. Both these items might well be added to the larger text.

With the exception of these two items, both texts cover the usual sections of thermodynamics which have application in mechanical engineering. These include the characteristic equations and properties of perfect gases and vapours together with the standard air, refrigerator and steam cycles, air compression, flow in nozzles and the properties of mixtures. The work is quite up to date, dealing as it does with the regenerative steam cycle, while the larger book incorporates articles on the binary cycle and the flow of steam in turbines. Here are found also discussions on the effect of variable specific heats in relation to the internal combustion engine, problems on humidity, and the thermal processes in cooling towers. These are matters of considerable importance, but are often neglected in texts of this type.

The author is to be congratulated not only on the clarity of his style but also on the judgment he has shown in the selection of his material.

B. L.-E.

A Treatise on Light. By R. A. Houstoun, M.A., D.Sc., F.Inst.P. Seventh edition. [Pp. xii + 528, with 2 coloured plates, 8 black and white plates and 345 figures.] (London, New York, Toronto: Longmans, Green & Co., 1938. 14*s*. net.)

"Houstoun's Light" is a title familiar to all students of Physics and well it may be, for "printed ten times since 1915" is no mean record. In fact, so well known are the previous editions, that little need be said in general description of the treatise. The book is intended for students who wish to proceed further, after having completed a first year's course and this intention it admirably fulfils.

Advantage has been taken of the opportunity afforded by the production of this new edition, the seventh, to make very extensive changes. The whole volume has been completely reset, old matter eliminated and the book brought up to date by the addition of new material. One chapter is now devoted to the quantum theory and half of another to the photoelectric effect. Other additions include sections dealing with three-dimensional gratings, recent determinations of the velocity of light and the production of optical diffraction by making use of the passage of ultra-sonic waves through liquids, such as was demonstrated by Debye and Sears.

There are eight pages of new half-tone illustrations and another feature which, doubtless, will be appreciated by all students, is the addition of answers to the problems. One may add that experience shows, that a conscientious labelling of the axes of graphs is always really worth while and also, that in the one or two cases, where no other methods of differentiation are employed, arrows attached to light rays would avoid the very small chance of confusion which might arise.

The publishers are to be congratulated upon being able to produce so useful a volume at such a favourable price.

R. E. G.

Light. By F. Bray, M.A. Second edition. [Pp. xii + 369, with 8 plates and 245 figures.] (London: Edward Arnold & Co., 1938. 7*s.* 6*d.*)

After an interval of eleven years, the author has now presented a completely revised second edition of his *Light*. Following an introductory historical chapter, are those dealing with geometrical optics. This part, which has been rewritten to permit the change to one of the sign conventions recommended by the Physical Society, carries the reader on to lens combinations, thick lenses and even to an introduction to aberrations. It is curious that the practice of showing the virtual rays as broken lines should be dropped in just one or two of the lens diagrams.

After a clear description of additive and subtractive colours in the chapter on the compositon of light, there follows a consideration of the eye and optical instruments in general. The chapter dealing with the velocity of light has been brought more up to date by the reference to quite recent measurements. Owing to the advances in recent years in illumination engineering, the next chapter, on photometry, has been completely rewritten; it is much improved and now forms a very useful account. The chapters on interference and diffraction which then follow, mention some applications of interest to more advanced students. The account of polarisation would not suffer by a renewed consideration on another occasion. The text concludes with a chapter on spectra and modern theory which, once again, would appeal only to more advanced students. The questions and numerical answers are very useful but one felt that, in just the few cases where general descriptive questions were included, a gain would result from their replacement by more definite and suggestive queries.

R. E. G.

The Perception of Light. By W. D. WRIGHT, D.Sc., A.R.C.S. [Pp. viii + 100, with 49 figures.] (London and Glasgow: Blackie & Son, Ltd., 1938. 6*s.* net.)

THIS little book gives an account of the phenomena of vision which, to quote from the Preface, " seem of most importance to those concerned with lighting problems and with the technical manipulation of visual sensations." The perception of movement, contrast and detail, dark adaptation, light adaptation, apparent brightness and glare form the main topics. These are discussed in an interesting manner with many references to recent researches. Wherever possible the attempt is made to relate the facts of visual perception with the underlying mechanisms in the retina and nervous system. The practical questions dealt with include the lighting of streets, factories and cinemas, the reduction of glare from motor-car headlights and correct reproduction in photography.

Dr. Wright has an easy style of writing and he succeeds in giving coherence to a mass of facts and theories of vision and their applications. Not all the views expressed will meet with general agreement. The claims made for the use of apparent brightness in dealing with practical problems have yet to be substantiated. It seems unlikely that by the measurement of apparent brightness by binocular matching we can evade the main difficulty which is the assessment of visual performance under practical conditions. The value of the method in probing the mysteries of visual perception has been amply demonstrated by Dr. Wright's own work and is not in question.

Löhle's work (1929) on the relation between the visual threshold and the area of the test spot might well have been quoted instead of the much earlier results of Piper (1903). On the effect of time of exposure of the test spot, the Blondel-Rey law could have been given in place of the less general Charpentier relation. It is questionable whether in a fog at night signal lights are first seen as diffuse light patches, as stated on p. 33. However, these are minor criticisms of a book which provides profitable reading for everyone interested in vision.

W. S. S.

Spectroscopy in Science and Industry. Proceedings of the Fifth Summer Conference on Spectroscopy and its Applications, held at the Massachusetts Institute of Technology, 1937. Edited by GEORGE R. HARRISON. [Pp. viii + 134, with 60 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 15*s.* net.)

IN 1932 a summer conference on spectroscopy and its applications was held at the Massachusetts Institute of Technology. It was so successful that a similar meeting has been held annually ever since, and the present volume contains a slightly abbreviated account of 29 of the 36 papers presented at the conference of 1937. Though confined to the practical applications of spectroscopy, the range covered is extremely wide, including such diverse subjects as metallurgy, pathology, organic and inorganic chemistry, physiology, botany, as well as discussions of instrument design and use, wave-length tables, the special significance of the infra-red region, and the use of the spectroscope for determining boiling points. It is difficult, in fact, to think of a possible field of application of the spectroscope which is not touched upon, except the one in which the instrument is probably used more extensively than in all the others taken together—namely, astronomy.

The history of spectroscopy is a fascinating one. The subject began as a branch of chemical analysis, but the apparent capriciousness of the behaviour of spectra soon led chemists to distrust the method and it was retained almost exclusively by the astronomer, for whom no other means of studying astrochemistry was available. With the advent of a tenable theory of spectrum emission in 1913, the spectroscope began to dominate physics, and for a decade or so it provided by far the most vital data for the investigation of atomic structure. The knowledge thus gained showed how the pitfalls into which earlier spectroscopists had fallen could be avoided, and the greater confidence thereby acquired, coupled with improvements in instrument technique called forth by the physical investigations, virtually created a new and most powerful method of research applicable to almost every science. The volume under review gives a good indication of the rapidity with which this method has been taken up.

As would be expected with such a compilation, the value and interest of the contributions are non-uniform, but the publication of the papers is much to be commended, and it is to be hoped that the proceedings at other conferences will also be made generally available. If so, it would be worth considering whether a little more detail might not be given of the various techniques, and it would be particularly desirable that the illustrations, of which there are many, should be made clearer. It is the exception rather than the rule for a reproduced photograph to show anything of interest, and this seriously detracts from the value of a work which might be studied with great profit by workers in almost every branch of experimental science.

H. D.

A Textbook of Electricity. By H. G. Mitchell, M.A., B.Sc. [Pp. xvi + 525, with frontispiece and 357 figures.] (London: Methuen & Co., Ltd., 1938. 10*s*.)

The book has an attractive form and, as the Author states, covers the ground for the Higher School, Intermediate and University Scholarship examinations. It is quite up to date, in fact neutrons, positrons, cosmic rays and induced radioactivity are all briefly described.

It is a common practice nowadays to begin a book with electrons and after a short account of them to pass to the ordinary classical treatment. This sometimes leads to inconsistencies, and a young student must be rather confused to read on p. 6 that "We must regard metals . . . as consisting of a rigid framework of positively charged atoms through which the free negatively charged electrons are distributed." And on p. 14 "the positive charge redistributes itself, . . ."

The expression "negatively charged electron" occurs frequently and seems to imply that the electron and the charge are two different things. On p. 461 the dual meaning of the term "electron" is explicitly stated, while on p. 504 it is called the elementary particle of electricity.

Copious examples are added to each chapter so that the student has ample practice. The general theoretical treatment and the description of experimental methods are well above the ordinary Intermediate standard.

S. G. S.

A Manual of Radioactivity. By George Hevesy and F. A. Paneth. Translated by Robert W. Lawson. Second edition. [Pp. xvi + 306, with 54 figures, including 5 plates.] (London: Oxford University Press, 1938. 17*s*. 6*d*. net.)

By means of an increase of more than 20 per cent. in the number of pages and of about 10 per cent. in the amount of letterpress on each page, the authors of *A Manual of Radioactivity* have been able to bring the present English edition of their well-known text-book again reasonably abreast of the times. Naturally, only the broad outlines of new developments have been given, but such is their treatment of the older portions of the subject, also. At no point does the treatment descend to great detail. All this is as it should be—and, as a general review of the whole science, "Hevesy and Paneth" will certainly retain a wide circle of readers.

In spite of all this, however, it seems equally clear to the present reviewer that the latest revision would have been even more satisfactory than it is, if it had been effected in a single stage rather than in two instalments. The first English edition was published in 1926, based on a German text of 1923. Then a new German edition was prepared in 1931—and the present English version followed after seven years. It is very unfortunate that the last German edition appeared just on the eve of a period of most rapid advance in the domain of the physics of the nucleus. These seven years have seen such widespread changes in experimental methods and in the theoretical background of the nuclear physicist that writings of an earlier period frequently stand in great need of complete recasting if they are to retain their value. In the book under review, whilst much has been added, there is evidence that a more drastic revision in parts was called for. Too often does one find a generalisation in one of the older passages negatived in a particular instance quoted in the most recent additions (or rendered out-of-date by other results not quoted)—and the additions in question, although they include a satisfactory review of the types of experimental data now being obtained, do not supplement the earlier descriptions of experimental methods sufficiently. One is left almost with the suspicion that the electroscope and the scintillation screen are still the most frequently used tools in radioactive research.

A few examples of unhappy translation remain over from the first edition (such as "eventual" for "possible" in a number of places), and one "purple passage" in particular makes its appearance in the new matter (p. 108): "Charged particles, rich in energy, are able to break through this barrier and effect a transmutation of the atomic nucleus, but the harvest is in general a very modest one." Whether this is a case of conscious rhetoric or of an all too literal rendering of the original, the present reviewer is unable to ascertain.

A high degree of subdivision leads to repetitiveness in some of the chapters and to a surfeit of cross-references, not all of which are very helpful, but the tabulation of data is in general admirably carried out. The sections on radiochemistry and on the geological implications of the subject are especially deserving of praise.

N. F.

Engineering Electronics. By D. G. Fink. [Pp. xiv + 358, with 217 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 21*s*. net.)

The rapid advance in the application of electronic devices to engineering problems has opened up a wide field requiring special consideration, and in this book an attempt is made to assist those who, without much knowledge of the subject, are called upon to deal with such problems in industrial work. With this object in view the author rightly confines attention to essential principles, a selection of illustrative applications, their capabilities and limitations. At the end of each chapter numerical examples are employed to familiarise the reader with the sizes of the quantities involved. The treatment is at once simple and effective as an introduction to a more thorough study of the subject. It is inevitable in a book of this kind that the more earnest student will find many gaps and a certain looseness in some of the statements made. This is particularly noticeable in Parts I and II, dealing with electron tubes and the physical principles on which their operation depends. As a companion volume to Part III, where applications are discussed, the reader may usefully refer to *Industrial Electronics* by Gulliksen & Vedder (John Wiley & Sons, New York; Chapman & Hall, Ltd., London).

H. M. Barlow.

Radio-Frequency Electrical Measurements: A Guide for Radio Engineering Laboratory Instruction. By Hugh A. Brown, M.S., E.E. Second edition. [Pp. xvi + 384, with 177 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 24*s*. net.)

The general plan and size of this book are much the same as the 1931 edition, with the addition of new material and the excision of old. Precise instructions are given for the carrying out of many important radio-frequency measurements. Just sufficient theory is included to enable the student who has some knowledge of alternating current theory to understand the principles underlying each method. There are references to books containing detailed theory and to original papers. In some cases the reader is referred to the first edition of this book for material omitted from the second.

Besides the usual measurements of circuit constants, E.M.F., current, power and frequency, instructions are given for experiments on aerials, the measurement of field strength, valve characteristics, wave-form, radio receivers, and piezo-electric crystals. The instructions are lucid and full and probable errors are considered. The inclusion of typical results from actual measurements would help in some cases. However, the book is already an excellent guide for the laboratory, and very useful for reference.

F. A. V.

Costs and Tariffs in Electricity Supply. By D. J. Bolton, M.Sc., M.I.E.E. [Pp. xii + 214, with frontispiece and 49 figures.] (London: Chapman & Hall, Ltd., 1938. 12*s*. 6*d*. net.)

This is an excellent and timely book. There is among the 600 Electricity Supply Undertakers in Great Britain a vast medley of different tariffs. A single supply authority may quote several different ones for the same service. There are flat-rate and block-rate, two-part and all-in tariffs with innumerable sub-variants. The measure of one part of the charge may be maximum demand, number of rooms in a house, rateable value, floor-space, number of wiring points. Maximum demand again may be defined in a variety of ways. Penalty and bonus clauses, adjustment clauses, special charges are scattered among all the varieties and sub-varieties of tariff in great profusion. So many different ways of charging for one commodity cannot all be sound, and most are, indeed, thoroughly unsound. But supply engineers can hardly be expected to do better. They have looked for guidance in vain. Most of the books and numerous papers published on tariff-making in the past have been more misleading than helpful. Some of these adopt the point of view of the salesman, others that of the accountant, others, again, that of the pure theorist. Each purveys that little knowledge which is a dangerous thing so that the supply engineer has done better in the past to trust his instinct than the written word. Mr. Bolton's great merit is that he appreciates all points of view and has thus provided the first and only sound guide we know for those faced with the practical task of creating order out of chaos.

The first two chapters show the relation between elasticity of demand and the best selling price, and it is proved that reductions in heating rate are commercially sounder and more in the public interest than reductions in lighting rate. Good reasons are given why kW charges should be lower and unit charges higher than superficial theory would imply. The mathematical treatment of the connection between price and profits as a function of elasticity of demand is valuable if only because it helps to clarify thought, and it is probably new to most electrical engineers. It is, however, unfortunate that the mathematical treatment in the first two chapters is liable to land the student in difficulties who attempts to go deeper into the subject. And this is, we suspect, due to a lack of appreciation of the properties of the hyperbolæ of which elasticity curves are constructed. Some of the author's statements and formulæ hold only if the demand curve is tangential to the Y-axis, others only if this curve cuts the Y-axis. It would, moreover, never be guessed from reading the book that the incremental value curve and the demand curve must always meet on the Y-axis, nor is the point clearly defined at which it pays to increase rather than to reduce the price.

A thorough analysis of production costs is admirably presented, though the author has failed to appreciate the assumption on which the Electricity Commissioner's allocation of coal and other works costs between a fixed and a running component is based. This assumption is that when, for instance, coal is plotted against load-factor, the Willans line for any station cuts the X-axis at a negative load-factor of 7·8 per cent. The author's analysis is followed by some rather revolutionary views on diversity, which are, however, backed by sound argument. The author has some interesting things to say on tariff-making as it is and as it should be, and goes thoroughly into the cost of low power-factor and ways of charging for it. We should have liked him, however, to have said more about the important effect of power-factor on the regulation of transmission systems.

The writing is throughout clear and readable, even entertaining, and the diagrams are well selected, so as to maintain in the reader a proper sense of proportion.

R. O. K.

Problems in Electrical Engineering (with answers). By S. Parker Smith, D.Sc., M.I.E.E., A.M.Inst.C.E. Third edition. [Pp. xxiv + 267.] (London: Constable & Co., Ltd., 1938. 6*s.* net.)

This is a third and enlarged edition of a book of numerical problems which has already proved of great value alike to teachers and students of electrical engineering. In the new volume, just published, a considerable extension of scope has been made and a number of questions dealing with telecommunications have been included. The problems have been classified afresh under main headings and detailed sub-headings with a view to simplifying reference to particular subjects. Whether the author has in fact achieved a better system of reference is open to doubt. The new arrangement has involved scattering problems of a similar kind throughout the book. Thus problems No. 41 and No. 802 are practically identical, yet they appear one at the beginning and the other towards the end of the collection. In the reviewer's opinion it would have been preferable to retain the original system of classification. Errors still persist in some of the answers given, as for example in problem No. 41 referred to above. Such deficiencies are comparatively rare and do not substantially detract from the general merit of this exceedingly useful book.

H. M. Barlow.

A Text Book of Applied Hydraulics. By H. Addison, M.Sc., A.M.Inst.C.E. Second edition. [Pp. xii + 435, with 337 figures, including 25 plates.] (London: Chapman & Hall, Ltd., 1938. 21*s*. net.)

The science of hydraulics dates from very early times, one of the first works on record being that of Frontinus, dealing with the water supply of Rome. For many generations the progress was small, until the seventeenth century, when Galileo published a book on hydrostatics. From that date hydraulics has attracted the attention of an ever-increasing number of scientists.

It is natural that this development should take place along a number of lines, and the title hydraulics has now become limited to signify the study of the flow of water from the point of view of the engineer. Even the development of this one branch has been most marked during the last three decades. Before the Great War, many texts could be regarded as little but a collection of empirical formulæ. More recently there has been a tendency to treat the subject from a mathematical standpoint.

The text under review attacks the problem from another angle. The author evidently realises that in writing a book for students, mathematical formulæ are of little value if the student does not understand where these formulæ can be applied. The author is to be congratulated on the way in which he has dealt with this matter. The reader feels at once that he visualises the problem in hand. That a second edition should be required within four years of the first is clear proof of the value of the text, which is up to date and presented in a lucid and simple style.

The book is not intended to cover the syllabus for any particular examination, and it is natural that, written by a Professor in Egypt, it should have a bias towards the subjects of irrigation, water supply and the use rather than the design of turbines and pumps. Within this scope, the book can be recommended not only to students, but also to specialists, as an introduction to more detailed works.

B. L.-E.

Airplane Structures. Vols. I and II. By Alfred S. Niles, A.B., S.B., and Joseph S. Newell, S.B. Second edition. [Vol. I: pp. xvi + 451, with 184 figures; Vol. II: pp. viii + 177, with 50 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. Vol. I, 25*s*. net; Vol. II, 13*s*. 6*d*. net.)

The last few years have seen what may fairly be called a revolution in aeroplane design; they have seen a transition in the majority of designs from the framed structure of a type familiar in other branches of engineering to the "stressed skin" structure.

Familiar methods of stress analysis have undergone rapid extension to cover the new type of structure with its relatively thin skin, and accounts of these new extensions of theory are to be found in the great numbers of technical reports of government departments and articles in the technical journals. The importance to the English aeronautical engineer of this new edition of Niles & Newell's *Airplane Structures* lies in the fact that it is the only textbook in the English language where much of this recent work can be found.

The book is laid out primarily for the student, and many examples are given, but the clarity of exposition will be welcomed by student and practising "stress man" alike.

The general field is well covered. It is interesting to note the importance the authors attach to the use of influence lines, a method of attack little used in aeronautical stress work in this country.

When dealing with continuous beams the theorem of three moments often tends to become unmanageable, and an excellent account is given of the Cross method of moment distribution.

The Wagner tension field theory is well set out, and adequate comment is made upon some of its present limitations, such as its pessimism in the estimation of stresses set up in the booms by the lateral loading due to the transverse component of the web tension.

It is doubtful whether there will be in this country much agreement with the authors' view that stresses in laterally loaded struts, or "beam-columns" as they aptly style them, are better dealt with analytically than with the aid of the Howard polar diagram. They describe Lundquist's method of dealing with the "beam-column" problem based on the Cross method, and the whole field of statically indeterminate structures is presented in an easily understandable form.

Although a considerable portion of the book has a direct application to American conditions of stressing, it can be thoroughly recommended to all in this country who are concerned with the strength of aeroplanes.

G. T. R. H.

Motion Picture Sound Engineering. The Research Council of the Academy of Motion Picture Arts and Sciences, Hollywood, California. [Pp. xx + 547, with 372 figures and 35 charts.] (New York : D. van Nostrand Co., Inc. ; London : Chapman & Hall, Ltd., 1938. 30*s*. net.)

THIS book, which is sponsored by the Research Council of the American Academy of Motion Picture Arts and Sciences, is based on a series of lectures given by specialists in the field of sound engineering and includes some of the most recent developments in the technique of sound recording and reproduction. The presentation is commendable for its simple, direct style, unencumbered by abstruse mathematical arguments. Practical applications of scientific researches undertaken by members of the Council and others are given special prominence and the results obtainable under operating conditions are of particular interest. The volume is divided into two parts, the first of which contains the real substance of the publication. The reader is introduced in turn and made conversant with film recording, noise reduction, re-recording, microphones and receivers, film drive, processing and reproducing systems, sound circuits, phase distortion, general network theory with applications to attenuation equalisers, wave-filters and impedance matching, thermionic valves, amplifier circuits, rectifiers and volume indicators. Part 2, while quite admirable in itself, is merely another elementary treatise on applied electricity and magnetism. It is difficult to understand why this has been included, as the reader must surely have sufficient knowledge of such primary considerations. The main part of the work is a noteworthy achievement of which the authors may be justly proud.

H. M. Barlow.

Engineering Science. Vol. II: Heat and Heat Engines and Electrotechnics. By H. B. Brown, B.Sc. (Eng.), Wh. Ex., A.M.I.Mech.E., and A. J. Bryant, B.Sc. (Eng.), A.M.I.Mech.E. [Pp. x + 450, with 198 figures]. (London: Macmillan & Co., Ltd., 1938. 6*s.*)

This is the second of two volumes intended to cover the first one or two years of Engineering Science at a Technical College. About 100 pages are devoted to heat, 180 to heat engines, 100 to electricity and magnetism, and the rest to general examples, etc. The weakest part of the book is that on simple measurements in heat, where the precautions taken in experiments are very inadequately treated. In Fig. 7 we meet our old enemy the "steam trap." The section on electricity is moderately good, though it is disconcerting to find on p. 373 that in a cathode ray oscillograph electrons are emitted from a heated anode and pass through a small hole in a cathode maintained at a high negative potential!

The authors seem much more at home in the section on heat engines, and have provided a clear and up-to-date introduction to the subject. An excellent feature, which will be welcomed by teacher and student alike, is the large number of worked and unworked examples (with answers).

F. A. V.

CHEMISTRY

Modern Aspects of Inorganic Chemistry. By H. J. Emeléus, D.Sc., A.R.C.S., and J. S. Anderson, Ph.D., A.R.C.S. [Pp. xii + 536, with 53 figures.] (London: George Routledge & Sons, Ltd., 1938. 25*s.* net.)

Some years ago an eminent chemist on an important occasion raised the question—"What has become of inorganic chemistry?" The reply was that inorganic chemistry was a progressive and rapidly growing section of our science which offered unlimited opportunities for the application of new experimental methods and the exploration of little-known fields. The justice of this reply and its truth, especially to-day, is amply confirmed by a perusal of this book by Emeléus and Anderson, which reviews critically the present position of inorganic chemistry in the light of progress made during the last twenty years. Here are dealt with the remarkable advances which have been made in our knowledge of the structure of matter, their bearing on the subject of valency and their relation to mesomerism or resonance. The uses of X-rays and electron diffraction, dipole moments and magnetic susceptibilities in solving difficult problems in inorganic chemistry are lucidly explained, and topics of major interest such as the polyacids, the silicates, peracids, carbonyls and nitrosyls receive full discussion. The co-ordination theory, Werner's great edifice which has become a unifying principle throughout inorganic chemistry, is clearly stated and applied in a masterly chapter which also contains an account of the recent important advances in inorganic stereochemistry. The epoch-making discovery and isolation of deuterium and its compounds is unfolded in a section which includes a discussion on the hydrides of the metals and non-metals. In two very interesting chapters are detailed some of the important extensions which have been made to the fundamental chemistry of metals and non-metals by the utilisation of improved appliances and new experimental technique. Other chapters deal with major topics of absorbing interest. It will be seen that the authors have placed at the disposal of chemists an impressive survey of modern aspects of inorganic chemistry in which they have not shirked the difficulties which the physico-chemical considerations present. For this they deserve the warmest thanks of chemists. The book is admirably produced and should make a wide appeal.

W. W.

Chemical Reactions Involving Solids. A General Discussion held by The Faraday Society, April 1938. [Pp. 265, with 3 plates and numerous figures.] (London : Gurney & Jackson, for the Society, 1938. 12*s*. 6*d*. net.)

AGAIN the Faraday Society has held a discussion at an opportune occasion. Reactions in solids are not as yet a very attractive field for investigators. The data and their manifold interpretations are widely scattered in the literature and it is difficult for anyone to discover what is the present position of the subject. This volume remedies the matter. As is usual the first part of the discussion deals with theory. Papers by Mott and by Slater give a general account of the relevant aspects of the modern theory of solids. These are followed by a number of papers dealing with the information given by the measurement of electrical conductivity of solids (excluding metals). Related to this section are papers on elementary photo processes in solids. A number of papers on the theory of the photographic plate are also included here. Of more purely chemical interest are the papers on the decomposition of detonating substances, dehydration of hydrates and the like. The third section on the reactions of gases and liquids with solid carbon stands apart from the remainder of the discussion and is less co-ordinated as a whole.

The bulk of the discussion is somewhat smaller than is usual owing to the Society's discrimination in selecting its authors more carefully than in the past. This makes for a more relevant, but not necessarily more interesting, discussion.

H. W. MELVILLE.

General Chemistry. By EUGENE P. SCHOCH and WILLIAM A. FELSING. International Chemical Series. [Pp. x + 524, with 74 figures.] (London : McGraw-Hill Publishing Co., Ltd., 1938. 18*s*. net.)

THIS book, which fully justifies its title, is an elaboration of the text used for many years past by elementary students in the University of Texas, and covers a very wide field, chapters on theoretical, descriptive, physical, analytical, organic and electro-chemistry and practical exercises all being included. The authors have not written the book from an encyclopædic point of view and recognise that some teachers may complain of the scarcity of dictionary-like information. It is certainly not a book to turn to if one wishes to "look something up." The authors' plan is to divide reactions into (1) those involving no valency change and (2) oxidation-reduction reactions. Upon the first are built the ideas of solubility, ionisation and activity of electrolytes, upon the second, the relations between the E.M.F. of half-cells, etc. Although many chapters are excellent, the book suffers from the disadvantage of being based on a specific curriculum and the facilities of a particular laboratory, *e.g.* arsenic, antimony, tin and cobalt have been omitted from an otherwise quite normal section on qualitative analysis, and there is no mention of either Rutherford or Bohr in the chapter on Atomic Structure. The presentation of the subject matter is clear and concise and the text shows no sign of the book being a first edition.

J. N. S.

An Introduction to Chemistry. By JOHN ARREND TIMM. Third edition. [Pp. xx + 568, with 162 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 21*s*. net.)

ELEMENTARY text-books of American origin generally make stimulating reading, and this volume in the " International Chemical Series " is no exception. It has a friendly air of informality, is up to date, and is moulded on a sound plan which " emphasises the philosophy of the science and bases the order of presentation of its subject matter on a logical development of the fundamental theories of chemistry." The author regards descriptive and analytical emphasis as unsuitable in a cultural course, and believes that courses based on the wonders of chemistry have " much the same reaction on the student as a five-pound box of candy consumed not wisely but all too well." History and anecdote are frequently used to provoke interest, and the arrangement—while placing wave-mechanics before the composition of water, widely separating nitrogen, nitrogen compounds, and nitrogen fixation, and in general preventing the use of the book either for cramming or for reference—is admirably suited to the needs of those who wish to familiarise themselves with the spirit and methods of modern chemistry. One does not turn over many pages without coming across a statement or an illustration that arrests attention. A few split infinitives and mis-spellings such as *spargyrists, hydragyrum,* and *systine* are perhaps excusable (although less so in a third edition than in a first), and one still likes a verb to agree with its subject, but a little more care in punctuation would have avoided such a statement as " there exist electromagnetic waves longer than the red light of greatest wave-length, visible to our eyes, and shorter than the shortest-waved, violet light." However, one must not allow petty annoyances to mar one's appreciation of a book into which many real students will dive as readily and as deeply as Prof. Compton's brother " dove " into the lake on p. 322.

A. A. E.

A Higher School Certificate Inorganic Chemistry. By G. H. J. ADLAM, O.B.E., M.A., B.Sc., and LESLIE SLATER PRICE, M.A., B.Sc., A.I.C. [Pp. viii + 624, with 87 figures.] (London: John Murray, 1938. 8*s*. 6*d*.)

THIS book is intended for candidates for the Higher School Certificate Examinations, State Scholarships and the External Intermediate Examination. Some previous knowledge of the subject (School Certificate standard) is assumed, *i.e.* the book does not begin at the very beginning, and an " upper limit " has also been imposed, the rarer elements (even gold and platinum) being omitted. Within these limits the authors have produced a really excellent work.

It is divided into three parts, (i) Chemical Theory (pp. 157), (ii) Descriptive Chemistry (pp. 425) and (iii) Atomic Structure and Valency (pp. 31). In Part 1 Chemical Theory and some elementary Physical Chemistry are presented in a clear and logical fashion. Only two minor points suggested themselves on reading this section. It is not immediately obvious from the statement of Henry's Law (p. 9) that the volume of dissolved gas is independent of the pressure, and the further reference to this under the Phase Rule (p. 94) comes much later in the course ; and secondly, the discussion of the common ion effect (pp. 132–3) and its application to qualitative analysis is somewhat condensed for a first reading.

In Part 2 the properties of the common elements are described. Hydrogen comes first, then the inert gases and afterwards the groups in the Periodic Classification are dealt with as a whole and in order, and no distinction is made between metals and non-metals. This method, though unusual, has much in its favour and the importance of the Periodic System as a basis of teaching is enhanced. Part 3, although short, is adequate and contains all that is necessary for a student at this stage.

J. N. S.

The Elements of Ferrous Metallurgy. By J. L. Rosenholtz, Ph.D., and J. F. Oesterle, Ph.D. Second edition. [Pp. viii + 258, with 140 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 15*s*. net.)

This is the second edition of a work originally prepared by Dr. Rosenholtz in 1930. Its object is to present an elementary treatment of the subject unencumbered by minute detail, primarily for the use of engineering students, but at the same time forming an introduction to the subject for metallurgical students. In these objects the book should be a success, for it deals with the subject in a suitable manner to give to engineering students a correct idea of the importance of the ferrous metals and alloys on which they depend so much for the success and endurance of their plant and structures.

The subject is dealt with from the occurrence of iron ores in the earth's crust, through the smelting of these in blast furnaces to the conversion of the iron produced into steel by the Bessemer, the open-hearth and the electric furnace methods together with the manufacture of wrought iron by the puddling and Aston processes, the cementation process and the production of crucible steel.

Approximately one-half of the book is devoted to these methods of producing iron and steel, the other half being devoted to a description and explanation of the iron-carbon system, the mechanical and heat treatment of steel and the composition, properties and uses of alloy steels.

The book is well illustrated with figures, diagrams, and photomicrographs and where necessary the chemical reactions taking place are explained simply and clearly. In the portion dealing with the smelting and manufacture, a very good account is given of the large-scale practice for the production of the large quantities of ferrous metals required annually and the portion dealing with the mechanical and heat treatment of the products gives a clear account of the principles involved and both portions should prove most useful to all engineering students.

C. O. B.

A Manual of Foundry Practice. By J. LAING, A.M.I.Brit.F., and R. T. ROLFE, F.I.C., M.I. & S.Inst., M.Inst.Met. Second edition. [Pp. viii + 312, with 162 figures, including 47 plates.] (London: Chapman & Hall, Ltd., 1938. 18*s.* net.)

To write a book on Foundry Practice, is, to say the least, a difficult task. To write one which deals adequately with the fundamental metallurgical knowledge and at the same time enlightens the uninitiated in the diversity and magnitude of actual foundry practice is a tremendous task. In this book both are done admirably.

By a careful perusal of its pages the practical man will find a solution to many of his problems; at the same time gaining a command of those fundamental principles of metallurgy which will enable him to approach his future problems from a more scientific angle. The metallurgist should certainly not miss such an opportunity for obtaining first-hand knowledge and suggestions on actual foundry procedure.

In the reviewer's opinion the greatest value of this book is to the student. He gets an insight into practically every subject of foundry practice from sand-testing to actual moulding practice. The examples dealing with this latter branch are obviously chosen with considered care and much forethought. This applies particularly to the chapter on "Loam Moulding," which in itself makes the acquisition of the book to the student almost imperative.

It is a great pity that the authors have not more space at their disposal. Perhaps they could then indicate a conclusive line of thought on such controversial subjects as the "effect of non-metallic inclusions on graphite size," etc. Perhaps too they could state in the non-ferrous section why the constituent metals of a non-ferrous alloy are added in the order stated. To take a concrete example; why, in making gun-metal, is tin added before zinc? Theoretically, either of the two latter metals can deoxidise copper. Zinc oxide is volatile and therefore more readily removed than tin oxide, which, once formed, is very persistent. Further, the result of deoxidation of copper by either zinc or tin will result in a metallic loss which is far less expensive in the case of zinc than of tin.

H. T. P.

Chemical Kinetics. By FARRINGTON DANIELS. The George Fisher Baker Lectureship in Chemistry at Cornell University. [Pp. viii + 273, with 41 figures.] (Ithaca, New York: Cornell University Press; London: Humphrey Milford, 1938. 15*s.* net.)

BOOKS on chemical kinetics are usually written to give an account of some particular aspect of the subject as seen by the author or according to a scheme which is now fairly well standardised in view of the divisions into which this branch of physical chemistry has been divided. The present volume breaks away from all these schemes, for it is not only the prerogative but the duty of the George Fisher Baker Lecturer to describe his own work and opinions on the subject. Thus interesting and informative sidelights are thrown on the field, which must usually be omitted in the more formal text. Prof. Farrington Daniels has adhered to the tradition with the result that a number of selected topics are dealt with in a comprehensive manner. The first chapter gives a general introduction to the whole book so that the non-specialist may duly appreciate what is to follow. Then comes a detailed account of a famous reaction—the thermal decomposition of nitrogen pentoxide—with a full description of experimental methods. The relationship between gaseous and liquid phase reactions is next discussed with the nitrogen pentoxide decomposition as a prominent example. Photochemistry gets a large share of the text, again with full experimental methods. Discussion is not, however, confined to gas reactions, the formation of Vitamin D and photosynthesis by chlorophyll being fully considered. Electrical activation, the relationship of infra-red spectroscopy to kinetics, the calculation of activation energies and isotopic indicators all come within review. An extensive field is therefore covered in a manner which cannot fail to be stimulating to specialist and non-specialist alike.

H. W. MELVILLE.

Reaction Kinetics. A General Discussion held by The Faraday Society, September 1937. [Pp. 268, with numerous figures.] (London: Gurney & Jackson, for the Society, 1938. 12*s*. 6*d*. net.)

THE title for this discussion is to some extent a misnomer, for the topics dealt with only include a very small section of chemical kinetics. With such a wide field at its disposal the Society had of course to delimit the boundaries rather carefully. The most important section is the first dealing with theoretical methods of treating reaction velocity. The transition state method is well to the fore and the papers on it give a good résumé of its present stage of development. Criticism of the shortcomings of the theory, also presented at the meeting, give the whole that balanced outlook which is so necessary at the present juncture. This, in fact, was one of the main objects of the meeting. The remainder of the discussion is devoted to reactions in solution with the intention of providing experimental evidence in support of the foregoing theory. There is however not much connection between the two, so that section 2 may be read and understood without having to digest section 1. The papers deal with a variety of topics, such as diene synthesis effect of high hydrostatic pressure on reaction rate, dipole moments and reactions velocity, ionisation of C—Hal bonds and proton transfer and H^+ ion reactions.

H. W. MELVILLE.

Inorganic Colloid Chemistry. Vol. III. The Colloidal Salts. By H. B. WEISER. [Pp. viii + 473, with 74 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 30*s*. net.)

WITHIN the confines of the term colloid chemistry may be included a multitude of phenomena. This book is no exception. It forms the third volume of a Treatise on Inorganic Colloid Chemistry and although its modest sub-title is The Colloidal Salts there is much of interest in it for physical chemists besides the more formal colloidal part which is naturally predominant. Nearly every inorganic sol is mentioned together with methods of preparation and properties. As such the clearly written account of these sols will prove of great assistance to those working in this or in neighbouring fields. The general reader will, however, be more interested in finding what contribution colloid chemistry has made to many of the more familiar phenomena of chemistry. His need is adequately met. Prof. Weiser has contrived to introduce into this necessarily formal text thorough and impartial discussions on the formation of precipitates of barium sulphate, the silver halides in photography and adsorption indicators. The famous arsenious sulphide sol gets its due share of space, and even lithopone and allied pigments as well as

Prussian Blue find a place. The vexed question of the mode of action of semipermeable membranes comes up for discussion in the section on copper ferrocyanide, while the last part of the book becomes very practical in dealing with silicate gels, inorganic soil colloids and cement.

One great advantage of the book is that not only is it complete in itself, but each chapter and even each section is sufficiently self-contained so that it may be read without having to consult the remainder of the text. With such a volume of literature at his disposal, Prof. Weiser is to be congratulated in providing an interesting and informative account of this branch of colloid chemistry.

H. W. MELVILLE.

The Elements of Physical Chemistry. By F. W. GODDARD, M.A., F.C.S., and E. J. F. JAMES, M.A., D.Phil. [Pp. viii + 251, with 67 figures.] (London: Longmans, Green & Co., 1938. 5*s.*)

PHYSICAL chemistry is the foundation of a true study of inorganic or organic chemistry, since it embraces the principles which underlie chemical reaction. It is, therefore, of the highest importance that post-matriculation students in schools should have a sound knowledge of this branch of chemistry, a fact which is recognised by teachers and examiners. This little book is intended for students who have passed the matriculation stage and are preparing for Higher School Certificate and University Scholarships. It claims to deal only with the elements of the subject, but the term " elements " is rather elastic, and it is convenient to grade the book as regards subject-matter according to examination syllabuses. The book covers adequately the work required for Higher School Certificate, but candidates for University Scholarships would be well advised to supplement it with the reading of larger texts and original papers, as is indeed suggested by the authors in their preface. The subject-matter is presented logically, accurately, and concisely, and the book has been written in a readable style, so far as this is possible when the work has been condensed into such a small compass. No student should find any difficulty with the mathematics involved. A useful part of the book is the collection of questions from various examination papers which is included. Answers are provided to the numerical examples.

A. J. M.

Textbook of Organic Chemistry By G. H. RICHTER. [Pp. viii + 712.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 20*s.* net.)

THE Author says in the preface that " the electron theory of valence has not been unduly emphasised, not that it is unimportant, but rather that it is best adapted to a more advanced exposition." This perhaps makes one expect to find the book to consist, as did the books of yore, of a list of preparations. But this is not so. The Author has a surprising facility in making even the dull facts of aliphatic chemistry interesting and I believe the book will be a boon to those students who have to learn their organic chemistry as " External " students (in the London University sense of the word) and to those who wish to have help in revising their lecture notes. The book would provide an excellent " refresher " course for works chemists.

It is made clear throughout the book that organic chemistry can be applied to the manufacture of drugs, dyes, perfumes and explosives, but the applications are not allowed to spoil the main story. The numerous derivations of chemical names and the notes on the occurrence of organic compounds add interest to the book. The style is clear and the English very readable, except in a few instances: for example, it is unpleasant to find on page 191 that ethylene dibromide is converted into the diacetate "by reacting it with potassium acetate." The book contains large numbers of useful tables of melting and boiling points, density and so on. Reaction kinetics are presented in an attractive way. The cracking of petroleum, rubber substitutes, the Grignard reagent are treated with considerable skill.

There are some mistakes, but not many. The most obvious one is the omission of a benzene nucleus in the formula for acriflavine. Another is the statement that "the alkaloids are all optically active."

In some respects the book is unsatisfactory. The Obermiller type of theory is held to be the best medium for explaining aromatic substitution reactions. Rearrangements of the Hofmann (aromatic) type are given as they were thirty years ago. Diazo coupling of phenols is explained by the K. H. Meyer and Dimroth theories. The omission of "the electron theory of valence" is overdone: the simplest conceptions have now entered the most elementary treatments of the subject. Stereochemistry is generally weak.

Nevertheless, the book has so many excellent features that it deserves an immediate success.

E. E. TURNER.

Organic Syntheses. Vol. XVIII. REYNOLD C. FUSON, Editor-in-Chief. [Pp. vi + 103.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 8*s.* 6*d.* net.)

THE latest volume of this well-known series describes tested laboratory methods for the preparation of twenty-nine organic compounds of importance as starting materials in organic research. The range of compounds is considerable and includes allylamine, diphenyl selenide, *l*-histidine, *p*-nitrobenzaldehyde, α-phenylacetoacetonitrile and taurine. An appendix giving later references to preparations described in preceding volumes is included.

O. L. B.

Chemistry of the Proteins. By DOROTHY JORDAN LLOYD, M.A., D.Sc., F.I.C., and AGNES SHORE, B.Sc., A.I.C. [Pp. xii + 532, with 101 figures, including 3 plates.] (London: J. & A. Churchill, Ltd., 1938. 21*s.* net.)

IN the second edition of this book, which was first published in 1926, two chapters dealing with the industrial aspects of protein chemistry have been abandoned, but the remaining chapters have been greatly enlarged and slightly rearranged to take account of recent advances in knowledge of the chemistry of the proteins. As before, the book commences with a survey of the nature and properties of the amino acids, but the classification of proteins receives consideration before the methods of protein analysis, which are the subject of a lucid and concise review. Following a discussion of the methods employed in establishing the homogeneity and identity of the proteins, consideration is given to the subject of nutrition in a section which includes an interesting survey of the work of Rose and his co-workers on indispensable amino acids. A brief but lucid account of the proteolytic enzymes provides a useful introduction to the argument concerning the size, structure and shape of the protein molecule, a subject which is first discussed in terms of the results of X-ray and ultracentrifuge methods of experiment. Due consideration is also given to the theoretical contributions of Wrinch and Bergmann, as well as the related evidence derived from the study of protein films and osmotic pressure measurements. Such more precise knowledge of the structure of the protein molecule is utilised in subsequent discussion of the theory of protein solutions, the sol-gel transformation, denaturation and the combination of proteins with acids and alkalis. In view of the wide field covered by the authors, it is not to be expected that they will be able always to command the agreement of the specialist. They have, however, succeeded in compiling an interesting and exhaustive review of the present state of protein chemistry, which is made all the more valuable by the excellent lists of references at the end of each chapter.

J. B. S.

Brewing: Science and Practice. Vol. I: Brewing Materials. By H. Lloyd Hind, B.Sc., F.I.C. [Pp. xiv + 505, with 63 figures, including 27 plates.] (London: Chapman & Hall, Ltd., 1938. 50*s.* net.)

Bearing in mind the great importance of brewing in this country, and the agricultural and subsidiary industries dependent on it, it is surprising that authoritative text-books on the subject appear at such long intervals. The last was by Sykes and Ling, over thirty years ago. When improvements in plant and processes and the vast amount of research bearing on brewing published in British and foreign scientific and technical journals are taken into account it will be realised that a new text-book is long overdue.

Brewing in the main is still regarded as an art and the scientific aspect of it somewhat of a curiosity. This outlook is to some extent due to the protected character of the industry. Competition is not felt so keenly as in more open undertakings, and profits for the last twenty years have been gratifying to brewing proprietors; hence it is felt that it may be well to leave good alone. But the advances in our knowledge on brewing, which are so well set out in this admirable book, indicate most clearly that the art of brewing is being changed slowly but surely to a biochemical and engineering proposition.

Mr. Hind is one of the few who has had the all-round experience on the technical and scientific side which enables him to write authoritatively on so wide a subject. The book will appeal alike to brewer, brewery chemist and student, not only in this country and the colonies, but in America also, where brewing technique is making such rapid strides since the repeal of the hasty and ill-thought-out Volstead Prohibition Act.

The volume includes twenty chapters, the first of which is historical and appropriately describes the Queen's College Brewery, Oxford, the oldest working brewery in this country. Five chapters on the different types of English and foreign barleys follow, including their characteristics and chemical composition. The biochemistry of malt and wort comprising colloids, hydrogen ion concentration, the carbohydrates, proteins and enzymes of barley and malt—subjects of intensive investigation here and abroad—are clearly set out and remarkably well described. An account of the subtle and interesting changes which take place in the malting process, the different kinds of malt, their analyses and interpretation of analytical results is followed by another covering malt adjuncts and brewing sugars. Four chapters are devoted to hops, their cultivation, treatment, chemical composition and brewing value. Brewing waters of varying types, their analyses, treatment and influence on the composition of wort are dealt with in an ample and lucid manner.

We are impressed by the clarity and easy literary style in which Mr. Hind treats the many and in some cases abstruse subjects which come within the ambit of this work. The documentation is sufficient, the illustrations very good, and the volume is well bound and opens easily.

All those concerned with brewing will await the appearance of the second volume with interest. Doubtless it will have the same high standard of the first, and the two will constitute a text-book of the first importance.

JULIAN L. BAKER.

Problems in Organic Chemistry. By E. H. HUNTRESS, Ph.D. International Chemical Series. [Pp. xii + 270.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 12*s*. 6*d*. net.)

THE book is planned on American teaching lines in which, as is said in the Preface, some "courses consist of experimentally illustrated lectures accompanied by weekly quiz sections and systematically planned weekly recitation and conference sections conducted with small groups of students." This shows at once that as far as recommended books go this one will not be put on the list in many University Colleges in this country, although it might appeal to Technical Colleges, where students cannot take their instruction so leisurely. I am not at all sure that the book will not be used more or less secretly by students who are bent on acquiring finger-tip knowledge.

Each topic is reviewed in skeleton—to set the stage, as it were, for the veritable bombardment of questions which is to follow. The questions are sound enough, though failure to answer some of them should not be penalised by the recorder. Many of the questions obviously fit in with the American printed question paper, in which blanks are left for a brief answer, but others require some reflection and would undoubtedly enable anyone to test his own knowledge or that of someone else. Some of the questions I found to be quite unnecessary, for example that on p. 253: "Write the structural formulas . . ." and the names of 111 miscellaneous organic compounds are put below. Surely even a student would do better to plough through a few pages of the index in his own familiar text-book.

I have no experience of this system of teaching chemistry, but can well believe that Prof. Huntress has devoted much thought to the preparation of his book. After all it is becoming increasingly difficult to make would-be researchers realise that they must know a lot of facts before they begin to elaborate theories and I'm not sure that this book wouldn't benefit many of our own students very considerably.

E. E. TURNER.

Experiments in Organic Chemistry. By L. J. DESHA and L. H. FARINHOLT. International Chemical Series. [Pp. xii + 233, with 23 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 10*s*. 6*d*. net.)

THIS book has a number of good points, but not enough to make it likely to replace existing works of the same approximate price. Most of the space is devoted to preparative work and considerable ingenuity has been shown in arranging a continuous practical programme for students who, it is evidently expected, will be in a hurry. There is a rather breathless air about the programme, but a student who did all he was told, or invited to do, would have acquired fair skill both at making experiments and in discovering what they were done for. I think the scheme will appeal to American teachers but probably not to English ones, this being said without any suggestion that either body of teachers has achieved finality as regards the best way of instructing beginners in the difficult technique of organic chemistry.

The book describes some excellent small-scale preparations, a few being distinctly unusual—for example, the first stage of the Hantzsch pyridine synthesis. A number of useful "tips" for facilitating various operations are given: one might mention the use of glass-wool in the distillation flask in vacuum distillation, this being said to prevent bumping. Shredded filter paper is used to mitigate the horrors of slimy filtrations.

In accordance with modern practice a little biochemistry is introduced and the book concludes with a reasonable scheme for the identification of organic compounds.

E. E. TURNER.

Micromethods of Quantitative Organic Elementary Analysis.

By JOSEPH B. NIEDERL, Ph.D., and VICTOR NIEDERL. [Pp. xvi + 271, with 53 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 15s. net.)

PREGL'S microanalytical methods were adopted in 1925 at Washington Square College, New York University, and in a few years became incorporated in the teaching curriculum of the Graduate School. It was found that if chemistry students without previous training in microchemical technique, as well as laboratory technicians, were to be enabled to follow the procedures, certain changes and adaptations from the original Pregl instructions had perforce to be introduced.

In this way concise working procedures have been developed by simplifying and shortening the instructions without, however, impairing their intrinsic value and accuracy. These outlines of the Pregl technique have been revised every year until they reached the form presented in this book, which is primarily a laboratory manual for teaching and practising the methods of quantitative organic microanalysis of pure compounds under ordinary laboratory conditions. The book differs from the latest edition of Pregl's treatise, notably in the omission of "historical" details, and in the alternative methods offered for some analytical determinations. Modifications of the classical procedure have been incorporated where experience has proved their value, and in a few cases, new approaches are described. The introductory sections on microchemical balances and weighing are comprehensive, and constitute the most helpful treatment of these important matters that the present reviewer has yet seen. All experimental methods are arranged in the form of clear and succint statements of (1) principle, (2) apparatus, (3) reagents, (4) procedure, (5) remarks—the last-mentioned containing valuable criticisms with full references to the original literature. There is an interesting and useful final chapter on the teaching of the subject, with advice and data on the installation of a microchemical laboratory for quantitative work. The whole of the matter is well set out and the printing is commendably free from error. In the appendix is a collection of standard working formulæ and tables (including logarithms) for calculation purposes. This excellent book should be available in every organic microanalytical laboratory.

W D

An Introduction to Microchemical Methods. By CECIL WILSON, M.Sc., Ph.D. [Pp. xii + 196, with 93 figures.] (London: Methuen & Co., Ltd., 1938. 7*s.* 6*d.* net.)

THE qualitative and quantitative manipulation of amounts of substances far smaller than those usually employed for analytical purposes is no new art; a seniority of fifty years or more is quite respect-worthy in the chronology of modern methods of scientific investigation. Moreover, for long some of these methods have been in daily use in industrial laboratories, and have proved their worth by their economies of space, time, money, and matter. Thus the technique, as applied to well-known methods, has much to recommend it. But microchemistry means more than this; it includes, for example, the processes of colorimetry and nephelometry, the use of the microscope in the identification of crystalline precipitates, the application of spot-tests, the employment of spectrographic methods, and the determination of such physico-chemical constants as are of particular analytical value. That there is a rapidly increasing demand for instruction and practice in work of this type is not surprising; for not only does an acquaintance with the appropriate apparatus and its use provide a foundation which can later form a basis for the development of specialised methods of control or investigation, but also it provides valuable training in careful and precise manipulation, while at the same time it offers a wide and most interesting field for exploration. The standard works of Behrens, Emich, Pregl, Feigl, and Chamot are well known and indeed indispensable, and the journal literature of microchemistry is now substantial; but for the beginner no more useful book than Dr. Wilson's could be desired. It deals briefly but adequately with the principal phases of the subject, and the diagrams are both numerous and clear. The author insists that after working through such a course the student does not become an expert, but is merely suitably equipped as a learner. He would also doubtless advise that whenever possible the technique should be acquired under the guidance of a skilled experimenter; but to hold this view is by no means to diminish the welcome which this book deserves.

A. A. E.

Newer Methods of Volumetric Chemical Analysis. Edited by WILHELM BÖTTGER. Translated by RALPH E. OESPER. [Pp. xiv + 268, with 18 figures.] (New York: D. van Nostrand Co., Inc.; London: Chapman & Hall, Ltd., 1938. 18*s.* 6*d.* net.)

So much has been done in the last two decades in developing volumetric methods, that this work would indeed be useful were it nothing but a guide to certain important sections of the recent literature of the subject—a respect in which it is more than up to date, since it contains numerous references to as yet unpublished work. The scope of the book is, however, far wider than this, for it gives full working details of a large number of the newer processes, and an authoritative discussion of the following topics: (i) Elimination of errors in acid-alkali titrations. In favourable cases using a weight burette an accuracy of 0·05 per cent. can be achieved. (ii) Use of ceric sulphate. (iii) Stamm's method of using alkaline permanganate so that only the rapid change $MnO_4' \rightarrow MnO_4''$ is involved. (iv) Iodate, Bromate and kindred methods. The largest part of the book (52 pp. discussion + 8 pp. bibliography) is devoted to these versatile and useful reagents. (v) Titrations (mostly potentiometric) with chromous salts, which are more powerful reducing agents than titanous salts. (vi) Oxidation-Reduction Indicators. (vii)

Adsorption Indicators. This part briefly summarises procedures accessible in the literature, and deals mainly with hitherto unpublished studies and their theoretical implications.

Some of the fields described have been opened up too recently for their practical value to be assessed at the moment, but a large proportion of the methods described are of established utility. All who use volumetric methods professionally, for research work, or in connection with advanced college courses, will value this symposium, written as it is by authors who have founded or contributed to the development of outstanding new methods.

T. B. SMITH.

A Laboratory Manual of Qualitative Analysis. By JOHN H. YOE, Ph.D. [Pp. ix + 219, with 7 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 12*s*. 6*d*. net.)

THIS book combines the functions of an elementary text-book of Qualitative Analysis and a course of study covering about 200 hours work in the laboratories of the University of Virginia. The details of the course are contained in Part I (21 pp.) and will have little interest for students working in other laboratories. Part 2 (84 pp.) deals with the Analysis of Cations. Part 3 (51 pp.) the Analysis of Anions. Part 4 (29 pp.) Organic Reagents in Inorganic Analysis and Part 5 (31 pp.) Qualitative Analysis by Dry Methods, tables of data, etc.

The book is essentially a practical one and contains no theoretical matter, but the working instructions appear to cover every contingency likely to arise during the course. Numerous exercises and questions are given at the end of each chapter and anyone working through them would acquire a very sound knowledge of the subject. The book is well produced but rather expensive.

J. N. S.

Elementary Analytical Chemistry. By C. G. LYONS, M.A., Ph.D., A.I.C., and F. N. APPLEYARD, B.Sc., Ph.C., F.I.C. Thirteenth edition. [Pp. vi + 282 with 15 figures.] (London: J. & A. Churchill, Ltd., 1938. 7*s*. 6*d*. net.)

LYONS and Appleyard's *Elementary Analytical Chemistry* has now reached its thirteenth edition, an adequate testimony to the value of the book. It covers the ground necessary for a pass degree and includes sections on Qualitative, Volumetric, Gravimetric, and Qualitative Organic Analysis, and Inorganic Preparations. The book has been re-set, the sections on qualitative analysis clarified and largely re-written and new matter including the use of ceric sulphate and titanous chloride added to the chapter on volumetric exercises. Criticisms of an established work of this kind are often based more on matters of personal taste and usage than on faults or omissions in the book. Partly on these grounds the reviewer suggests that, since a student usually begins with inorganic preparations, the instructions here might be a little more detailed. Where equivalent weights of salts are to be taken, formulæ showing the amount of water of crystallisation ought to be given and the chapter would be better placed at the beginning than at the end of the inorganic section. The examination for acid radicals (pp. 136–7) might usefully be elaborated, with fewer cross-references to other parts of the book, and it is useful to have a table treating Group III as a whole when the ammonia-ammonium chloride separation is not wholly successful. Only one slip has been noticed, *viz.* stannic chloride (p. 42) should read stannic sulphide. The book is excellently produced and may be recommended with confidence.

J. N. S.

GEOLOGY

Geology of Glasgow and District. By W. J. McCallien, D.Sc., F.R.S.E. [Pp. x + 190, with 88 figures and 1 coloured plate.] (London and Glasgow: Blackie & Son, Ltd., 1938. 5*s*. net.)

Students of geology in the Glasgow district are fortunate in having within easy access of the city a rich variety of rock-types of many different ages. They are fortunate also in possessing in Dr. McCallien's recently published book an up-to-date and compact account of the physical features and geological structure of the district. It will be specially welcomed by the growing number of young people in all walks of life who are being drawn to the study of their geographical and geological surroundings. Dr. McCallien deals in turn with the crystalline schists of the Glasgow Highlands, with the Silurian, Old Red Sandstone and Carboniferous rocks of the south-western part of the Midland Valley, and with the deposits of glacial and recent times; discusses the evolution of the scenery of the district; and provides a concise account of the various igneous episodes in the geological record. His book includes a final section devoted to the description of itineraries to localities of special geological interest, carefully selected in order to provide the student with a general introduction to the various sedimentary and igneous rocks. It is accompanied by a glossary of the technical terms used, an index and a colour-printed geological map of the district on the scale of 8 miles to an inch. Special value attaches to the numerous excellent illustrations drawn by the author.

A book of this nature must necessarily be very largely a compilation from the publications of previous workers and brief reference to some of the pioneers of geological research in the Glasgow district is given in the second chapter. The compilation has been carefully and methodically carried out and Dr. McCallien is to be congratulated on the successful accomplishment of his difficult task. The present reviewer ventures to make a few minor criticisms for consideration when a second edition is called for. The chapter on the Highland rocks would be better placed near the end of the volume, since it deals with problems and structures which are difficult for the beginner to grasp. A section dealing with the economic resources of the district as a whole would be a useful and valuable addition. To the details of the different excursions there might be added references to maps and other publications where further information can be obtained. There are also certain paragraphs (notably in the earlier chapters) which appear somewhat out of place in a book of this kind, while certain phrases, such as the reference to "post-dyke river valleys" on p. 114, should certainly be reworded.

M. M.

Introductory Economic Geology. By W. A. Tarr, Ph.D., Sc.D. Second edition. [Pp. xii + 645, with 257 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 30*s*. net.)

Evidence of the need for a work of this kind is shown by the issue of a revised edition after a lapse of eight years, the subject being one with a notoriously scant bibliography. Although the title presupposes the book to be introductory in character, no mention as to standard of knowledge is made. Thus Fig. 2 makes use of a number of mineralogical terms; on p. 22, a petrological term is introduced; and one would have preferred a clear exposition of some elementary mineralogy and petrology before the description of magmatic phenomena involving the use of these terms. The book omits to give any

Table of Strata, and no hints as to field or laboratory studies are included. It is still more disquieting to find so important a subject as metamorphism referred to under various headings, but receiving no explanation as such.

The first half of the book deals with metallic minerals of economic importance, the larger mineral deposits in various countries being dealt with adequately, with useful diagrams, the sequence of this portion of the book being well set out. One is surprised to find the gold deposits of the United States of America described in greater detail than those of the classic Witswatersrand, which far outweigh in importance the American examples. The photograph of a concentrating plant failed to convey much to the reviewer, while Fig. 69 is too complicated for use in an introductory treatise.

The second portion deals with non-metallic materials, of which the first section, dealing with coal, is very readable, while the same remark may be used for petroleum and natural gas. The simple section (Fig. 165c) across an oilfield would have been of greater use if placed earlier in the chapter, while under "clays" a diagrammatic section of a continuous kiln would have added to the value of the work. Aggregates as such are not referred to, although out of 83 million tons of limestone quarried in the States in a single year 51 million tons were in the form of crushed stone. Some micro-photographs of typical building sands would improve this portion of the book, while the statements as to the behaviour of Portland Stone in London give a most misleading impression of our leading building stone. It must of course be admitted that a work of this nature covers an enormous field, and the problem of what to leave out must have been a pressing one. It can, however, be recommended to those requiring an outline knowledge of the subject, while for the referencing and indexing there can be nothing but praise.

B. H. KNIGHT.

Mineral Tables. By ARTHUR S. EAKLE. Third edition, revised by ADOLF PABST. [Pp. vi + 73.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 7*s*. 6*d*. net.)

THIS volume was first issued in 1904, and, although thirty-four years have since elapsed, the need for a third edition is a testimony to the labours of the authors. The tables depend solely upon physical properties as a means of mineral identification, and microscopic and X-ray analysis are intentionally omitted: but this does not detract in any way from the real utility of the work, more particularly to the prospector, mining geologist and the civil engineer who needs some knowledge of elementary mineralogy.

The Tables are not in any sense a systematic study of common minerals, neither do they attempt to cover the whole vast field of the mineral kingdom. They depend on the method of elimination for the rapid "running down" or identification of minerals in the field. Doubtless it is possible to collect many specimens which these tables would fail to identify with certainty; but for the handiness of this volume there can be unstinted praise.

B. H. KNIGHT.

BOTANY

Plant Form and Function. By F. E. FRITSCH, D.Sc., F.R.S., and E. J. SALISBURY, D.Sc., F.R.S. [Pp. viii + 668, with 445 figures.] (London: G. Bell & Sons, Ltd., 1938. 17*s*. 6*d*. net.)

THIS volume combines the substance of two well-known works by the same authors, *An Introduction to the Study of Plants* and *An Introduction to the Struc-*

ture and Reproduction of Plants. Those who know the quality of those textbooks are not surprised to learn of the many requests for the union of the two in a single volume. In its preparation the authors have taken the opportunity of bringing the subject matter up to date and have also extended the ground covered. More space has been given to plant physiological aspects and the ecological side has been more fully treated. Further additions include a chapter on the British Flora and the extension of the section on taxonomy by the inclusion of additional families. The brief but useful appendix has been amplified to include more practical directions for physiological work.

We naturally find in the book the high qualities which these authors have taught us to expect. Insistence on viewing the plant as a whole, an admirable balance of parts, accuracy of fact and modernity of outlook, all are here. In addition there is the easy style and also the refreshing originality of the illustrations.

Few slips have been observed. On p. 40 it might be useful to distinguish between cell sap and the tissue sap to which the cryoscopic method can be applied. The first footnote on p. 83 is a little ambiguous, and on p. 211 the difference between large and small calories might be brought out. The legend of Fig. 413 is not fully explanatory of all the differently marked squares in the genetical chessboard above it. In a later edition it would be useful to give in the appendix the osmotic pressure of various concentrations of cane sugar, placing them side by side with those for a salt which appear on p. 635. Data for the comparison of the plasmolysing action of a non-electrolyte and an electrolyte would thus be available.

Prof. Fritsch and Prof. Salisbury have had many years' experience of academic teaching and of active research in the laboratory and the field; they also have a wide acquaintanceship with scientific authorship. With this rich endowment they have produced what, for its scope, is certainly the best introduction to general botany in the English language. Furthermore it is a real introduction, for the references to literature are admirably chosen to guide the student in his further work.

V. H. B.

Vergleichende Morphologie der niederen Pflanzen. Erster Teil: Formbildung. By Dr. B. SCHUSSNIG. [Pp. viii + 382, with 470 figures.] (Berlin: Gebrüder Borntraeger, 1938. RM. 36, paper covers; RM. 38, bound.)

BY contrast to the lack of general treatises in the first decade of this century, the student of Thallophyta has now at his disposal a considerable number of standard works dealing with these plants from diverse points of view. Schussnig's book, however, approaches the subject from a rather new angle and, moreover, includes a consideration both of Fungi and Algæ. The present volume deals essentially only with cell-structure and vegetative organisation, and it is a little disturbing that neither a list of the cited literature nor an index is available, a defect that one trusts may be remedied by the rapid appearance of the second volume. The subject-matter, illustrated by a large number of illustrations, a considerable number of which are originals, is clearly presented and often stimulating, but would hardly be intelligible to any one who had not already acquired an appreciable knowledge of the forms involved. This is essentially a book for the specialist and, viewed as such, one need not perhaps deplore too much the introduction of a considerable number of new terms, in part rather cumbersome.

It is of considerable value to have the holophytic and heterotrophic Thallophyta brought into juxtaposition, but after reading the book one cannot get away from the impression that the Fungi take a decidedly second place in the subject-matter, although Lichens are dealt with at some length. External form, too, is rather scantily treated, and it is to internal structure that most attention is devoted. Schussnig develops the view that the parenchymatous structure of the higher Pharophyceæ is actually derived from a system of threads, as has long been accepted for Florideæ. It is difficult to ascertain the basis upon which this view is founded and, since we are told that it will be considered in greater detail in connection with the reproduction organisation in the second volume, a critical discussion of its validity may well await the publication of this volume.

F. E. Fritsch.

A Textbook of General Botany for Colleges and Universities. By Richard M. Holman and Wilfred W. Robbins. Fourth edition. [Pp. xviii + 664, with frontispiece and 482 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 20*s*. net.)

This is a new edition of a well-known text-book incorrectly described on its cover as the third edition. It would seem best to deal in this review more especially with the new portions of the work. These generally seem to be of too slender a nature to be regarded as really satisfactory. The theory of solute absorption has been scarcely more than defined although from the preface we are led to expect " greater weight " to be given to it than in previous editions. The underlying idea in absorption, suction pressure, is only vaguely hinted at. The rise of sap is not treated very convincingly, mention is not made of the recent work of Haines indicating that gas bubbles may be formed in wood elements, and in one paragraph on p. 138 it is suggested that no bubbles are formed, while in another paragraph on the same page suggestions are made as to how such bubbles could be dissolved when they are formed; the activity of living wood elements is postulated from premises from which such activity would not seem necessarily to follow.

A discussion on hormones in relation to growth phenomena seems to be limited to a paragraph and the word does not appear in the index or in the glossary. The latter is a useful addition to the book.

E. M. C.

An Introduction to Biology. By E. J. Hatfield, M.Sc. [Pp. 540, with 168 figures.] (Oxford: at the Clarendon Press; London: Humphrey Milford, 1938. 6*s*. 6*d*.)

This book is designed to cover the various school certificate examinations, although no attempt has been made to confine the subject matter entirely to the syllabuses of these examinations. The treatment is primarily physiological, function rather than structure being stressed; nevertheless, although animals and plants are not dealt with as types, sufficient morphology is introduced to enable function to be understood; carefully selected experiments are introduced wherever possible, and the student is throughout urged to learn for himself from observation of living things.

It is unfortunate that the book contains a number of inaccurate or loose statements. Among these may be noted the information that protozoa occur in the air, that sometimes stems are entirely devoid of leaves, that the shell of the Radiolaria is external and that charr and minnows inhabit ponds. Moreover, it is highly improbable that roots, in the inclined sieve experiment (Fig. 71), would grow *up* the slope of the sieve, while in the klinostat experiments it is surely more instructive to place seedlings with their radicles parallel to the axis of rotation, and not at rightangles to it.

While it is, unfortunately, desirable that attention should be drawn to such deficiencies, it is hoped that they will not create an unfavourable impression of the book; they are of a type which can easily be corrected in a second edition: in fundamentals the subject matter is sound and the treatment lucid. The merit of the book lies in its freshness of outlook and in the clear picture which it draws of the principles underlying the science of living things, plant or animal. It *is* an introduction to biology, a book which should give any intelligent school child an insight into himself and the living things around him, and this, and not preparation for examinations, is surely the primary object of teaching Biology in schools.

F. W. J.

Soilless Growth of Plants. By Carleton Ellis and Miller W. Swaney. [Pp. 155, with frontispiece and 58 figures.] (New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1938. 13*s*. 6*d*. net.)

During the last few years there has awakened a growing realisation that soil is not the only medium on which plants can satisfactorily be grown, but that water, supplied with the essential nutrient salts, can support adequate growth. This is no new knowledge—the method has been used for many years in experimental work—but the fact that it has commercial possibilities is now attracting attention from many quarters. On the face of it the process is simple, but only those rendered wise by experience realise the inherent difficulties, and there is a danger that inexperience may under-rate these difficulties and lead to failures, causing disappointment and financial loss. From the experimental side the method is invaluable as it provides a measure of control of available salts which enables very exact work to be done, and the importance of traces of such elements as boron, manganese and copper could hardly have been determined otherwise. On the commercial side, for large-scale work, engineering skill as well as horticultural knowledge is essential, and the combination of the two may not always be available.

Nevertheless, much writing in the popular press has brought the method before the public, and there is a growing demand for information. The volume under review summarises the various ways in which plants can be grown without soil, either in actual solutions or in inert media such as sand to which the necessary chemicals are added. Somewhat too much stress is laid on successful cultures, and the uninitiated reader may fail to grasp the possibilities or the reasons of failure. It is clearly shown, however, that the method is very elastic, and that a variety of culture solutions, containers and methods of manipulation may give equally good results.

In such a comprehensive sweep, however, it has not been possible to give full details of the commercial aspects of the problem, and there is room for a parallel volume in which that side of the matter is specifically dealt with, successes and failures being treated impartially. It is difficult to forecast the possible future of the method, and much may depend upon the frank acknowledgement and discussion of the difficulties with a view to providing a sound basis on which the work may be developed.

Winifred E. Brenchley.

Cryptogamic Botany. Vol. I: Algæ and Fungi. Vol. II: Bryophytes and Pteridophytes. By GILBERT M. SMITH. McGraw-Hill Publications in the Botanical Sciences. [Vol. I: pp. viii + 545, with 299 figures; Vol. II: pp. viii + 380, with 224 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. Vol. I, 24*s*. net; Vol. II, 18*s*. net.)

IN the presentation of the different groups the type basis has in general been adopted in this work, though a more general treatment is furnished of the Diatoms and Slime Fungi.

Of the first volume 350 pages are devoted to the Algæ, 15 to the Slime Fungi, 147 to the Fungi proper and 10 pages to the Lichens. In the second volume the Bryophyta claim 113 pages, the Ferns 119 pages and the other Pteridophyta 134 pages. A book mainly based on the type system necessitates a selection that challenges criticism, the more so that this method of presentation, more perhaps than any other, is liable to fail in emphasising the salient biological principles that each group especially illustrates. In the Volvocaceæ the types the author has selected are *Volvox* and both *Pandorina* and *Eudorina*, but *Gonium* is omitted. In the Chateophoraceæ *Stigeoclonium* is alone described, despite the interesting series which this group presents of comparative differentiation of the thallus. The Xanthophyceæ, Chrysophyceæ and Diatoms, here grouped as the Chrysophyta, together with the Pyrrophyta and Euglenophyta, are collectively accorded 77 pages, whilst the whole of the Chlorophyta, including the Characeæ, are dealt with in about 120 pages.

In the second volume the comparative morphology of the thallus of the Hepaticæ, especially of the leafy Liverworts, receives but scant treatment, though far the most important and instructive feature of the group.

One feels indeed when perusing these pages that, though the descriptions are clear and the illustrations mostly admirable, there is a lack of balance in the treatment, which seems too often to be uninspired by a guiding principle or theme to render each section a connected whole. We fear that the student using these volumes may well fail to develop that grasp of a mental framework without which the facts of structure or life-history are liable to degenerate into mere information.

E. J. S.

AGRICULTURE

The Principles of Soil Science. By A. A. J. DE SIGMOND, Ph.D. Translated from the Hungarian by A. B. YOLLAND, B.A., Ph.D., and edited by G. V. JACKS, M.A., B.Sc. [Pp. xiv + 362, with 4 folding plates and 34 figures.] (London: Thomas Murby & Co., 1938. 22*s*. 6*d*. net.)

PROF. DE SIGMOND of the University of Budapest is not only one of the leading authorities on soil science, but is particularly associated with the progress that has been made in the classification of soil types. We are, therefore, fortunate in having in this English translation a presentation of the author's views. As Sir John Russell aptly expresses it in a brief foreword, "Professor de Sigmond has put us under a debt of gratitude by summarising his own wide experience and discussing in the light of his own knowledge the work that is being done by others."

The first 77 pages treat of the origin of soils, the factors involved in their formation, and the types of formation. The chemical characteristics of soils are dealt with in the second part (pp. 79–146), whilst Part III (pp. 147–310) is devoted to soil taxonomy and Part IV treats of soil cartography.

The third section is not merely the largest, but is the most important, as presenting for the first time in the English language the author's views on soil classification. The soils are here divided into 25 Orders which, together with the chief types into which each is subdivided, provide some fifty soil categories, based primarily on chemical composition. The characterisation is sometimes rather meagre, as is well seen in the description of dune soils, which is so curtailed as to be actually misleading. As Sir John Russell points out, certain English soils do not easily fit into the system, a type of difficulty not unknown when using taxonomic monographs of flowering plants even in the country of their origin. The wider diffusion that this translation will afford to the author's views is the best means of ensuring such adjustments in the system as may be necessary to permit of their wider application.

It is a tribute alike to Author, Translator and Editor that the work is at once readable and lucid.

E. J. SALISBURY.

The Study of the Soil in the Field. By G. R. CLARKE, B.Sc., M.A. Second Edition. [Pp. 192, with 1 plate and 8 figures.] (Oxford: At the Clarendon Press; London: Humphrey Milford, 1938. 6*s.* net.)

IT is fully recognised now that the study of the soil in the laboratory, while it has many major problems to investigate, can never be more than the study of *soil material.* The *soil* can only be studied *in situ,* and Mr. Clarke's book is unique in giving in small compass detailed guidance for field study and for the recording of observations and maps. Soil surveying is not an easy task and students approaching it are fortunate in having the experience of many years of work on the subject presented in this book. This second edition is amplified in detail and contains additional information and advice on soil sampling, on the preliminary surveying of the ground, and on the classification and recording of soil colours. There is a notable addition to the section dealing with soil structure in which the author gives, along with the Russian and American classifications of structure, a scheme that has been evolved in this country largely by the author himself.

The first edition of this book received a well-merited welcome. A book of this usefulness is so well worth being kept up to date and improved as further experience in the subject is gained, that the second edition is assured of success.

N. M. C.

The Soils of Palestine: Studies in Soil Formation and Land Utilisation in the Mediterranean. By A. REIFENBERG, Ph.D., Dipl. Agric. Translated by C. L. WHITTLES, M.A., Ph.D. [Pp. viii + 131, with 8 plates and 4 figures.] (London: Thomas Murby & Co., 1938. 14*s.* net.)

THE soil of Palestine has in the past undergone the same faulty treatment that is meted out to so many soils in other parts of the world; the abandonment of terrace cultivation and the destruction of trees has exposed the soil to the forces of erosion and the practice of burning dung as fuel has further contributed to the impoverishment of the land. The investigation of the properties of the soils is an obvious preliminary to the task of restoring the land's ancient fertility. In the present volume the author describes the effects of weathering on soil formation and gives an account of the evolution and characteristics of the various soils found in Palestine, including especially the "terra rossa." This is followed by sections on soil formation under the Mediterranean climate, as compared with that under other climates. In a chapter dealing with soils and agriculture attention is drawn to the problem of irrigation and its attendant perils due to the high salt content of Palestine water. A final chapter deals briefly with the subject of Zionist colonisation.

P. H.

Mother Earth. By G. W. ROBINSON, Sc.D. [Pp. viii + 202, with 1 plate, 6 figures and 2 maps.] (London: Thomas Murby & Co., 1937. 5*s*. 6*d*. net.)

THE soil has received less generally recognised study than any part of man's environment. The geologist has long since considered certain very broad principles of its formation but has otherwise ignored it. Most of those concerned in any way with the products of the soil have (of necessity) grouped soils very roughly according to their ease of cultivation or their power to produce crops. The soil, as Prof. Robinson says, has not "the strange magnificence of the outer universe or the curiosity of the inner recesses of the atom." But, in fact, it presents a major sphere of scientific enquiry: the soil is a natural object as worthy of study "for its own sake" as plant, or mineral, or rock.

Scientific interest in the soil has not developed to a great extent even among the most prominent of those who are interested in the Land and its practical problems. Prof. Robinson thinks (and we agree) that their interest should be aroused and that once aroused they themselves will appreciate this sphere of study: hence this book. In a series of letters to Prof. Stapledon the writer discourses in a most attractive manner upon some of the major themes of pedology. Disclaiming any conception of the book as a text-book or systematic course of instruction Prof. Robinson does, however, virtually cover the ground of an introductory course in the subject. Pedologists already familiar with its subject matter should read this book for its philosophical conceptions, teachers of soil science should urge their students to read it, but in view of its purpose the crucial consideration is: has Prof. Stapledon read it and if so what has he to say. As an original presentation of the principles of pedology the book should be very welcome, but we shall only know of the failure or success of its avowed purpose when we have a book of correspondence between Prof. Robinson and his distinguished colleague.

N. M. C.

Science in Agriculture. By JOHN W. PATERSON, B.Sc., Ph.D. [Pp. viii + 288, with 165 figures.] (London, New York, Toronto: Longmans, Green & Co., 1938. 6*s*. 6*d*.)

THIS book aims at taking the scientific basis of farming to provide a course in general science. This conception of teaching general science by using as far as possible the phenomena of the farm as material for illustration and experiment is certainly very commendable.

Dr. Paterson covers a great amount of subject matter in a book which should be very useful to the teacher who desires to use agricultural material in a general science course. But the book suffers very much from the fact that it is the agricultural material rather than the general science that influences the sequence and the content of its fifty chapters. Beginning with the atmosphere and combustion, the book passes straight on to the plant, rainfall, rock weathering and soils, fertilizers, crops, cropping and crop diseases, animal husbandry and dairy products. This dominance of "agriculture" and of the agricultural sequence partially defeats the intention to provide a "grounding in general science." A chapter is included on "Matter and Energy" and another on "Chemical Constitution," but these chapters give the impression of being reluctantly included as unavoidable parentheses. The statement in the text that "separate study of chemistry . . . will be of undoubted advantage to the student" reads almost as an apology for the exposition of chemical constitution which is offered in this book. A chapter of seven pages is certainly inadequate to explain chemical constitution, and the picture of a chemical laboratory in the chapter does not seem to help at all. Moreover, chemical formulæ and equations are used in the book before the attempt is made to explain them.

This book should be very useful to the teacher in providing material. As a book for the student it could usefully be revised to allow the general science rather than the agriculture to determine its sequence and the content of its earlier chapters.

N. M. C.

Poultry Husbandry. By MORLEY A. JULL. Second edition. [Pp. viii + 548, with 213 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 24*s.* net.)

OF the various branches of agriculture in Great Britain the poultry industry has attained the third place in economic value, with an annual output of some £30 millions. With the doubling of the poultry population to approximately 60 million birds during the past twenty years the mortality rate has risen from 5 to 20 per cent. The estimated loss of laying stock alone is about £4 millions per annum. This serious position is due to the inherited disease known as fowl paralysis and secondly, to the prevalence of parasitic infections. Fowl paralysis has been disseminated throughout the breeding stock of the utility strains upon which the industry's prosperity had been built; it will be combatted by selective breeding from progeny tested stock. The spread of parasitic disease is largely due to the farmer's neglect or ignorance of the principles of sanitation in the housing of his birds and husbandry of his land. Specialist poultry farming on limited acreage inevitably turns the land "bird-sick," *i.e.* it becomes infested and continually re-infested with disease-producing parasites. Veterinary teaching has for years emphasised that the prevention of parasitic disease requires the recognition of livestock as a crop to be rotated over the land. Now that this teaching more recently has been reinforced by Prof. Stapledon's principles of grass husbandry, science is beginning to explain why general farming is more likely to succeed in this country than specialist farming.

The poultry farmer of this country will find little guidance in tackling this big problem by studying the book under review. Fowl paralysis is regarded as a virus infection; and the author wages war against infectious disease mainly by means of disinfectants projected through a variety of gadgets. His disregard of the inexpensive systematic scrubbing with hot soda water and soft soap is interesting. Even human indifference to work cannot be exploited on the poultry farms of this country by American labour-saving devices. From Prof. Jull's descriptions and illustrations it would appear that the housing for poultry should be on a scale worthy of the architecture of Washington; in this country such buildings are deemed more appropriate for agricultural colleges and government stations where the taxpayer can see something for his money, but does not have to use them for his own livelihood. The American systems of feeding are more likely to interest our corn importers and offal merchants. Prof. Jull's text and illustrations of the dressing, grading, packing, and marketing of poultry are most useful. But the British poultry farmer is more concerned about how to rear live birds and to keep them alive until he wants them dead; moreover it would appear from its mortality statistics that the American poultry industry is equally interested in the same problem. Possibly in the next edition of this admirably appointed book Prof. Jull will deal more vigorously with current problems of disease.

TOM HARE.

ZOOLOGY

Text-book of Zoology. By W. F. WHEELER, M.A. [Pp. xii + 356, with 182 figures.] (London: William Heinemann, Ltd., 1938. 10*s*. 6*d*.)

WRITTEN by a schoolmaster for use in schools, and mindful of general requirements for the Higher School Certificate, this attractive-looking volume might well be expected to meet the needs and convenience of a considerable number of scholars and teachers. Size, type, paper and *format* are admirable; but, when one looks into it, one cannot escape the suspicion that it is less the Certificate than the University Scholarship which has determined the distinctive features of its scope and treatment. Coming on the heels of Prof. James Gray's address to the Science Masters' Association, and the publication of the Spens Report, it has a special significance as evidence of the trend towards academic specialisation of the latest recruit among school subjects.

Two-thirds of the book deal with some twenty-five selected types, half of them in full anatomical detail, and bristling with all the terms of academic morphology and biochemistry. Ecological aspects are avoided, the author considering " potted ecology " as " not only valueless but dangerous." His avowed aim is to lead his pupils onwards from the observation of particulars to " the formulation of general conclusions," but, instead of reserving such wholesome exercises for the class-room, he proceeds to interpolate three chapters of concentrated embryology (Amphioxus, Frog, and Chick), which go well beyond the point where observation is practicable in schools, and formulates the generalisations himself, basing them upon a far wider field than that of the course provided.

Take as an example his treatment of the vascular system. Like the other chapters of this section it is essentially a condensation (unacknowledged) of the corresponding section of De Beer's *Vertebrate Zoology*, and, like it, is centred in the divergence of " Therapsid " and " Sauropsid " types—these terms being nowhere defined—the hearts of which the author proceeds to explain. But, unlike the University student, the schoolboy has not been prepared for all this by the dissection of reptiles and birds, so that the claim to regard this chapter as a "correlation of facts ascertained" is pure blarney. In this case it is reduced to nonsense by the careless statement that in Therapsida "only the right systemic arch persists" (p. 241). We may well apply the author's own words to such "potted" morphology: it is both valueless and dangerous, and the smarter the boy who imbibes it, the greater the injury.

In fact, errors large and small are so numerous that, in spite of some good features, it is certain to lead ambitious boys astray, unless first corrected by an alert and cautious teacher. How can the Hydrozoa be properly understood when their dimorphism is based on hydranth and blastostyle instead of on hydranth and medusa? The spire of a snail's shell is described as "dextral" without any explanation of what this means, while the figure is that of an abnormal individual, with its shell on the wrong side of the body, and its spire sinistral! The metapleures and epipleures of Amphioxus are mixed up, and its gill-slits credited with "vascularised diverticula" of their walls. The aftershafts of feathers are said to be located on their "dorsal" side. Spelling, of course, is no longer an element in education, but the chances of our clever boy will scarcely be increased if, in addition to such errors, he follows his guide in words like "nuchel," "proboscces," "occulo-motor," "chordæ tendinæ," and "retrices," and in such combinations as "an hollow," "an hair," indefinitely repeated. If these are trifles, what will the systematists say to the following "precise definition" of a species: "Two organisms are said to belong to the same species if, as a result of their interbreeding, there is produced no diminution of fertility. It is not sufficient that two organisms breed. . . . This process must be continued through many generations and *it must be proved that fertility is not diminished.* This being so the organisms are assigned to the same species"!

W. G.

Animal Biology. By L. L. Woodruff. Second edition. [Pp. xvi + 535, with frontispiece and 312 figures.] (New York and London: Macmillan & Co., Ltd., 1938. 16*s.* net.)

General Biology. By P. D. Strausbaugh, Ph.D., and B. R. Weimer, Ph.D. [Pp. xii + 555, with 284 figures, including 13 coloured plates.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 18*s.* 6*d.* net.)

The previous edition of Woodruff's *Animal Biology* has been considerably revised for the production of this new volume. The book starts with a discussion on the scope of biology, on the physical basis of life, and on metabolism. This is followed by a survey of the animal kingdom occupying about 100 pages. This is pithy but from its very brevity contains wide generalisations, sometimes at the expense of complete accuracy. It includes some good diagrams, particularly of vertebrates. Then comes about 70 pages of outline physiology of vertebrates. The rest of the book deals largely with the philosophical side of animal biology and is much the most interesting, since problems rarely tackled in books of this nature are discussed at some length; the chapter on the continuity of life being particularly good. In all probability an adolescent would find all this part very stimulating. Such a full survey is given of the theories as to the origin of life that it is surprising to find no mention of the recent work on the constitution of viruses. In discussing Eugenics, the author has not given a really clear statement of the problem as it exists to-day. Thus in a book which deals so well with medical and economic zoology, it appears somewhat illogical to state that "from the standpoint of permanent advance—racial rather than individual—the path to progress is chiefly through Eugenics, the science of being well born" (p. 314), while not making it clear that only in a good environment are the inherited potentialities of the members of a race given full opportunity.

The second book starts with a chapter on the nature of life, following which the attributes of living, as opposed to non-living matter, are dealt with in turn. The general plan is for plants and animals to be dealt with side by side as working machines, and in this way the fact that the properties of protoplasm are exhibited equally by plants and by animals is stressed, and the differences between the two kingdoms pointed out. Flowering plants and man are used for the bases of the discussions, but reference is also made to other forms. This arrangement has proved very successful and so much information is included that it is disappointing to find the mineral requirements of living organisms, especially plants, scarcely mentioned in the chapters on metabolism. After the physiological part (170 pp.) there is a long and fairly full chapter on heredity, followed by a survey of the animal kingdom and a brief comparative account of sexual reproduction in Thallophyta, Bryophyta, Pteridophyta and Spermatophyta. The book is concluded by discussions of living organisms in relation to their environment and of evolution. The illustrations are good and, apart from its sketchy treatment of morphology, this is an excellent book. The style is lively throughout and the "human interest" paragraphs incorporated make it extremely stimulating.

Students preparing for the Intermediate or Higher School examinations should find these books both interesting and useful, although they do not completely fulfil the requirements of any English syllabus. They are to be especially commended for including man among the animals discussed and for introducing accounts of human evolution and of the economic aspects of biology. Their main deficiency lies in their failure to discuss many of the underlying principles of morphology and their tendency to give catalogues of animals. Further, although both books deal at some length with the mechanism of heredity, in the course of which diagrams of meiosis are given and crossing over briefly touched upon, no mention of chiasmata is made in spite of their presence being characteristic of meiosis in the majority of plants and animals.

Both books have bibliographies and are well indexed.

N. H. Howes.

General Zoology. By H. L. Wieman. Third edition. McGraw-Hill Publications in the Zoological Sciences. [Pp. x + 497, with 271 figures.] (London: McGraw-Hill Publishing Co., Ltd., 1938. 21*s*. net.)

This work constitutes an almost complete rewriting of the previous edition. Since this is a companion book to a course of instruction in theoretical and practical Zoology, it is perhaps unfair to assess its value as an isolated volume. It forms part of an introductory course for pre-professional students and its contents hardly fit into the scheme of instruction used in England. Alone it would make rather difficult reading for a beginner since many topics are suddenly dealt with without adequate introduction, *e.g.* fibrinogen, before blood is referred to at all, and ptyalin without previous reference to enzymes, and the book appears curiously desultory and unequal in its treatment of the subject as a whole. These apparent faults could be compensated by a parallel course of lectures as, indeed, is intended by the author. The evolutionary aspects are well treated. There is a bibliography and a glossary, not in previous editions, and an index.

N. H. Howes.

Manual for Comparative Anatomy. By L. P. Sayles. [Pp. xii + 214, with 55 figures.] (New York and London: Macmillan & Co., Ltd., 1938. 7*s*. 6*d*. net.)

One of the greatest difficulties experienced in teaching practical vertebrate anatomy is to avoid the course degenerating into little more than a sketching class, in which the student satisfies himself that he is working hard if he produces the requisite number of drawings at the end. The tendency is always to forget the reason for the work amidst a wealth of detailed drawing. Prof. Sayles has made a very important attempt to overcome this difficulty by providing a handbook which, to use his words, places a stress on dissections and practical examinations and not on drawings. In order to achieve this object, he has introduced into this manual a number of questions, without giving the answers, which, apart from stimulating the student to ask further questions of a similar nature, keeps him constantly in mind of the points illustrated by the work, the problems arising from them and most important of all the functions of the parts examined. This method of approach might well be extended to all branches of comparative anatomy.

The manual is planned to illustrate the course of study of vertebrate anatomy at the City College of New York. The first part of the course illustrates a series of vertebrate types, which is followed by detailed comparative studies of the organ systems. This seems an admirable method of study and although some of the types used, such as Necturus, are not readily available in British laboratories, this book could nevertheless be adapted to be used as a valuable ground plan for courses of vertebrate anatomy.

J. A. Moy-Thomas.

The Human Value of Biology. By Johan Hjort. [Pp. xii + 241, with 29 figures, including 2 plates.] (Cambridge, Mass.: Harvard University Press; London: Humphrey Milford, 1938. 10*s*. 6*d*. net.)

This small volume, by the distinguished Norwegian scientist who has contributed so much to oceanography and fishery research, is so comprehensive in its scope and so crammed with interesting facts and original views that it is no easy matter to give an account of it in a short review. It deals with the philosophy of science and the history of biological theory, sketches in brief a system of biological thought, and applies these biological principles to the interpretation of human sociology. It is in fact an epitome of the author's outlook on life, an expression of his individual philosophy. And Dr. Hjort's life has been so full and varied, he has played so large a part in marine research and in practical affairs and he has throughout paid so much attention to questions of scientific method that this matured account of his views on biology and its importance for mankind is of the highest interest and value.

In the matter of philosophy Dr. Hjort is a sturdy supporter of Kant and will have nothing to do with modern theories of relativity, which he regards as sophistical. In biology his point of view—a very sound and fruitful one—is an extension of that of Cuvier and Claude Bernard. He lays great stress upon the fundamental fact that animals are ecological specialists, adapted in structure and function for a particular mode of life in a particular complex of environmental conditions both animate and inanimate.

And not only does each kind inhabit a specialised environment but inside that it has its own subjective or individual environment—the environment as it appears to the individual animal. "As Cuvier's functional schema led to a new comparative anatomy and physiology, so will the idea of reciprocity between the animal and its environment lead to a new comparative biology. This science must look upon animals as specialists. Every form of life in nature shows the double peculiarity of Schwann's cells, the possession not only of general vital functions but of a certain special function of its own" (p. 146). Looked at in this way the whole animal kingdom may be regarded as showing differentiation, division of labour, in such a way as to exploit all the possibilities of the environment. As Hjort puts it, "if the multiplicity of living organisms is regarded as a whole, then it is precisely differentiation, specialisation, that has made it possible for living organisms to utilise the conditions of life found on the earth" (p. 147).

So too in human societies we find the same principle of division of labour for the full exploitation of environment—a principle well known to Adam Smith whom Hjort rightly regards as a great biologist in the tradition of Kant and Cuvier, and one ignored by Karl Marx, whom Hjort regards as a theorist dealing with abstractions.

These important biological and sociological views which Hjort has formed from his rich experience are illustrated by examples drawn from his oceanographical researches, his population studies on fish and whales and the story of man's exploitation of them which he knows so fully and intimately.

E. S. Russell.

The Behaviour of Animals: An Introduction to its Study. By E. S. Russell, O.B.E., D.Sc., F.L.S. Second edition. [Pp. viii + 196, with 6 plates and 26 figures.] (London: Edward Arnold & Co., 1938. 10*s.* 6*d.* net.)

It certainly is a welcome fact that only four years after the first appearance of Dr. Russell's lucid little book a second edition has proved to be necessary. This may be regarded as a proof of the growing interest in animal psychology and of a growing understanding of the fact, that the physiological and behaviouristical interpretations of animal behaviour give only a one-sided and incomplete view of what happens when the animal seeks its food or flees from danger. Dr. Russell has utilised this opportunity to add a new (tenth) chapter in which he explains his views on the perception of animals and their functional meaning, views which come near to those of von Uexküll and his "Umweltlehre." Those objects or events, in respect of which the animal shows behaviour, are called "valent," are said to have "valence" for it, and these are just the objects or events, which in ordinary life have a biological significance for the animal. This valence may change, according to the needs of the animal, as is illustrated by examples taken from the observations of Kirkman with black-headed gulls and of Brock with the hermit crab *Pagurus arrosor*. It is to be hoped that this second edition may find the same warm welcome among biologists as the first edition, so that Dr. Russell may soon have the opportunity to add more interesting expositions when a third edition is called for.

J. A. Bierens de Haan.

The Study of Heredity. By E. B. Ford, M.A., B.Sc. Home University Library, No. 187. [Pp. 256, with 5 figures.] (London : Thornton Butterworth, Ltd., 1938. 2*s*. 6*d*. net.)

There is probably no branch of biology productive of more " introductory " books than heredity ; and this for several reasons. First, it is an extremely active sphere of investigation, with changing concepts and data ; it is too one which has a great interest to the man in the street—in fact, inheritance runs the weather and golf a close third in conversation where parents may be assembled. Again it is somewhat mathematical in its methods and it is usually assumed that the ordinary person is incapable of understanding simple arithmetic and each author tries to present the subject matter in a new and more appetising form.

The Home University Library is to be congratulated on having brought its list up to date with this volume, for the last fifteen years have seen a tremendous advance in our knowledge of heredity. Mr. Ford is to be congratulated too on having written a book on this admittedly difficult subject clearly, concisely and without unnecessary conventionalisms.

In the first four chapters Mr. Ford presents the main facts, cytological and genetical, which form the basis of our ideas about heredity. The examples are fresh and well distributed in plant and animal kingdoms. Moreover, they are lucidly described without the doubtful aid of often confusing and conventionalised diagrams. Chapters V and VI discuss the nature of heredity and of variation in the light of the foregoing and prepare for the information produced in Chapter VII concerning the way the genetic factors work. This chapter contains much up-to-date material since it is only recently that investigations have been fruitful in this field.

Finally, in the last three chapters, the discussion centres round " heredity in the field " : selection, practical applications for the plant and animal breeder, evolution. I suppose it is possible to write a book about heredity without discussing evolution, but it has not yet been done. If evolution must be included then embryology should be given as prominent a place. *The Study of Heredity* is eminently readable and a worthy addition to the Home University Library.

H. R. H.

Evolution. Essays on Aspects of Evolutionary Biology, presented to Professor E. S. Goodrich on his 70th birthday. Edited by G. R. de Beer. [Pp. viii + 351, with 3 plates and 43 figures.] (Oxford : at the Clarendon Press ; London : Humphrey Milford, 1938. 15*s*. net.)

This delightful volume reflects great credit on those who have contributed to its pages. It is a pity that the plan of the book did not allow of its including an essay by Prof. Goodrich himself, for he is the soundest comparative anatomist in Britain. Two of the essays call for some criticism, notably the editor's own contribution on Embryology and Evolution. Dr. de Beer has thought fit to incorporate in it his peculiar ideas of the non-validity of both the biogenetic law and the germ layer theory; this is unfortunate because these theories, in the opinion of almost all comparative embryologists, together constitute one of the corner-stones of the theory of evolution.

The second essay to which exception must be taken is that of Prof. J. B. S. Haldane on the nature of interspecific differences. Prof. Haldane is a brilliant writer, and has given a lucid exposition of Morgan's theory of the origin of species from sports and of the origin of sports themselves. Morgan thinks that sports arise by chance, and that when two different sports are crossed then corresponding chromosomes approach each other in the ripening germ-cells (a process known as "meiosis") and that pieces of both break off and change over. These pieces are called "genes," and it is assumed that each "gene" has the power of making some particular change in the resulting hybrid. But this whole account is out of date, for Mohr (described by Dr. Bateson as the foremost "drosophilist" in Europe) has shown that sports, compared with normal individuals, are characterised by a lack of vigour proportionate to the amount of their divergence from the specific type; from individuals thus deficient healthy natural races could not possibly have arisen. Moreover Walker and Agar have shown that the so-called "meiosis" is really a longitudinal division of the chromosomes; the real meiosis occurs later in the nuclear cycle when the chromosomes have shrunk to little spherical structures, a condition in which breaking and crossing over are unthinkable.

The other essays are excellent, being written by men each one of whom is a master in his particular branch of his subject. Specially to be mentioned are those of Dr. W. K. Spencer and Dr. Tucker on "Echinodermata" and "The Origin of Birds" respectively.

The essays cover every aspect of Zoology; thus we have one by Dr. J. Z. Young on the origin of the nervous system and the adaptation of the organism to its environment; Dr. Elton, who might be called the founder of the science of animal Ecology in this country, contributes an essay on animal numbers and adaptation, and, finally, Dr. A. M. Carr Saunders writes one, which we consider the most important of all, on Human Evolution and the control of its future.

E. W. MacBride.

Snakes of the World. By Raymond L. Ditmars, Litt.D. [Pp. xii + 207, with 85 plates.] (New York and London: Macmillan & Co., Ltd., 1937. 8*s.* 6*d.* net.)

Dr. Raymond Ditmars is one of the world's greatest experts on snakes, and from his position as curator of mammals and reptiles at the New York Zoological Park has perhaps done more than anybody to popularise these cold-blooded vertebrates. His *Snakes of the World,* which was first published in 1931, and is now brought up to date, is perhaps the most successful of his general books on reptiles—all the more so because, while giving an excellent picture of snake life from the point of view of habits, distribution and general classification it never descends to a mere catalogue, and the pages of useful information contained in it are written in an extremely readable style, with plenty of authoritative and reliable opinions.

The book appears to be strictly designed for those with an interest in snakes. It does not attempt at all widely to deal with the zoological principles exhibited by this interesting group of creatures. Certainly the excellent prints at the end of the book can have few rivals in any other work on the group, and alone are worth the comparatively low price at which the book is distributed.

It is to be hoped that Dr. Ditmars will be able to bring out further editions of this important natural history work.

James Fisher.

Catalogue of Indian Insects. Part 23: Chalcidoidea. By M. S. Mani, M.A. (Res.). [Pp. iv + 174.] (Delhi: Manager of Publications, 1938. Rs. 3/2 or 5*s.* 6*d.*)

The main purpose of the present work is to provide a classified list of the species of Chalcidids hitherto recorded from India. Since this is a relatively little-known group in any part of the world, such a list is always useful and particularly so in India, whose fauna has been so largely described in scattered publications, often dealing with one or two Indian species at a time. Information as to the distribution and hosts of the 600 species is also included.

The Catalogue is prefaced by a key to the families, subfamilies and tribes of the group. This key follows Ashmead's one very closely and suffers, therefore, from the same disabilities. These will be particularly felt in the Pteromalidæ, where the classification of Thomson and Kurdjumov is superior. The use of the suffix *-ariæ* for tribes is unusual in the Hymenoptera and it is to be hoped that in future editions the names of the tribes will be revised since the suffix has frequently been added to an incorrect root. A certain number of misprints has been noted, particularly in the tribal names; the most widespread one, however, is the omission of the accent from the name of Dr. Ch. Ferrière.

O. W. R.

MEDICINE

Textbook of Comparative Physiology. By Charles Gardner Rogers, Ph.D., Sc.D. Second edition. McGraw-Hill Publications in the Zoological Sciences. [Pp. xviii + 715, with 158 figures, including 1 coloured plate.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 30*s.* net.)

Any author who undertakes a textbook of comparative physiology sets himself a task of no mean proportions, namely to review the physiology of the entire animal kingdom, and to endeavour to find general principles. When it is considered to what size the literature of mammalian physiology alone has now expanded, it is clear that no work of comparative physiology can hope to be profound without being unwieldy, or comprehensive without the risk of appearing superficial.

The author has had a good deal of experience in teaching the subject, and it would seem that he has exercised a wise choice of topics of a sort to encourage thought, and to act as a counterweight to the teaching of zoology along purely morphological, and therefore largely statical, lines. It should also be useful as a means of bringing to the forefront a study of physiological principles other than that associated with the problems of medicine, for however important this viewpoint may be, nothing but harm can in the long run come to the subject of physiology as a whole if the touchstone of merit is considered to be merely its value in connection with the requirements of medicine or the physiology of man.

The first edition of the work appeared in 1926, and the author tells us that he has rewritten the major part of the book. A glance at its contents is enough to indicate that this is certainly the case. It would be impossible adequately to review these contents in a few words—they range from electrons and neutrons to chromosome maps and experimental embryology. The printing and illustrating is technically good, and the style clear and readable. It suffers from one definite fault—the lack of references. Many besides students will use the book, and it is to be hoped that, when the next edition comes along, the author may consider whether he could entertain the formidable task of introducing these, if for no other reason than that of showing when the facts given were discovered. C. L. E.

Laboratory Manual of General Physiology. By Philip H. Mitchell and Ivon R. Taylor. McGraw-Hill Publications in the Zoological Sciences. [Pp. xvi + 142, with 29 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 8*s*. 6*d*. net.)

This book consists of brief descriptions of 254 physiological experiments, selected for students taking physiology for the first time and is designed to be used in conjunction with Mitchell's *General Physiology*. The subject matter is that usually associated with the teaching of human physiology and biochemistry, but a wider range is covered in somewhat less detail than in a medical course.

The set-up of each experiment is described succinctly and accurately, but there is no theoretical discussion since this is essentially a laboratory manual. Teachers of the subject might well find this book a source of fresh ideas for practical courses since several of the experiments are new and aptly illustrate the appropriate points. N. H. Howes.

Introduction to Physiological Chemistry. By Meyer Bodansky, Ph.D., M.D. Fourth edition. [Pp. x + 686, with 41 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 20*s*. net.)

In the fourth edition of this well-written and most useful textbook a few additions and minor improvements have been introduced. Chapter VI of the last edition has been split into two, one on Digestion, and the other on Chemistry of Enzymes, the latter having been rewritten by Dr. Oscar Bodansky. In those branches of the subject which are rapidly developing, as for example the chemistry of Vitamin E, the most recently available results have been incorporated, though here it is interesting to note the progress of knowledge has been so rapid that what is the latest information one month is out of date the next. These modifications have involved an increase of about 25 pages as compared with the last edition. However, the book remains of reasonable size and price. At the same time it is remarkably comprehensive. It is a good book to refer to when information is wanted on any biochemical fact or theory. W. O. K.

Science and Nutrition. By A. L. Bacharach, M.A., F.I.C. Changing World Library, No. 11. [Pp. xiv + 154.] (London: C. A. Watts & Co., Ltd., 1938. 2*s*. 6*d*. net.)

To-day it is assumed that most men and women are acquainted with vitamins, familiar words in the advertisement columns of the press and subject of table gossip, where they are classed as patent medicines, food fads, brown bread and orange juice.

In this little primer on nutrition—the author has approached the subject by way of the more serious angle of biochemistry—introducing the readers to the meaning of food, and the nature of proteins, carbohydrates and fats, involving the use of simple chemical formulæ, which conceivably may be deterrent to even ordinary intelligent people, so notoriously are they shocked by abstract things and symbols. It is to be hoped that this may not be so in this case, since the study of diet deficiency implies some knowledge of the physiology of digestion and the necessities of full healthy diet.

The book can be thoroughly recommended; it is easily expressed—the subject being so arranged that the interest grows to the end of the last chapter. It is a subject in which we are all deeply concerned, and certainly it is by such introductions as these that readers become interested and encouraged to extend their study.

Prof. J. C. Drummond, Professor of Biochemistry at London University, has written a useful foreword. From *The Times* of December 14, 1938, it is announced that the food group of the Society of Chemical Industry is forming a Nutrition Panel of prominent experts, with Prof. Drummond as chairman and Mr. A. L. Bacharach as secretary, for the study of food in relation to health and disease.

P. J.

ABC of the Vitamins. By Jennie Gregory, M.S. [Pp. xii + 94, with 56 charts.] (London: Baillière, Tindall & Cox, 1938. 13*s*. 6*d*. net.)

This is scarcely a book in the ordinary sense, for apart from the preface and introduction it contains no continuous narrative. Its affinities are rather with the atlas or dictionary, or perhaps better, the illustrated catalogue. It attempts to convey the important known facts about the vitamins in as simple and direct a way as possible, making use of drawings, maps and diagrams both to explain and to emphasise the meaning. Each of its large pages deals with some special aspect of the subject, and the essential facts are stated clearly and concisely. For convenience the volume is divided into a series of chapters; let us take for example Chapter 2 which deals with Vitamin A. Of the pages which it contains, the first deals with the physiological functions of the vitamin, the next two with aspects of Vitamin A deficiency, then come Vitamin A and vision, Vitamin A and carotene, and finally Vitamin A and infections. Other chapters deal in a similar way with—"The B vitamins," "The antiscorbutic factor," Vitamin D, Vitamin E, experimental methods in vitamin research and lastly some general relationships and miscellaneous information.

As will be seen the range of the book is a wide one, and if it had been written in the ordinary way a much larger volume would have been required to contain all the information which it gives. The value of a work of this kind depends on the accuracy of its facts and the judgment with which these facts have been selected and arranged. In the reviewer's opinion, high marks should be awarded on both these scores. It is of course inevitable that in any such compendium of potted knowledge oversimplifications should creep in. But the intelligent reader will realise this and use the volume for what it is, an illustrated guide book to the vitamins, and supplement it when necessary with further reading. The book should prove most useful to many varieties of readers, to doctors and other experts on diets, who wish to confirm their facts before giving advice, to medical students on the night preceding their biochemical examination, and even to the man in the street who wants to be sure he is eating the right amount of the right sort of food to cover his vitamin requirements. For this is eminently a practical book, an instrument easy to use and quick to yield the required result.

W. O. K.

Manual of Psychiatry and Mental Hygiene. By AARON J. ROSANOFF, M.D. Seventh edition. [Pp. xviii + 1091, with 87 figures, including 1 plate.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 37*s*. 6*d*. net.)

IT is eleven years since the sixth edition of this well-known textbook was published, and during this interval many important developments have occurred in Psychiatry. The preparation of a new edition taking account of these developments has therefore been a formidable task, and it may be said at once that the author is to be congratulated on having carried out this task in a remarkably satisfactory manner. Naturally the book has had to be enlarged, by nearly 300 pages, and the great majority of chapters have been entirely rewritten. The author has finally succeeded in producing a work which is one of the best textbooks available to the student, and one which in general presents an excellent picture of Psychiatry as it stands to-day.

Psychiatry is a subject in which there are many opposing schools of thought, and it is probably impossible for any textbook to cover the ground in a way which would be satisfactory to each of those schools. The author has made a valiant attempt to surmount this difficulty, however, and his outlook has been catholic and without obvious prejudice. Although his own view would seem to be mainly " organic " and " physiological," he has devoted considerable space to the " psychological " conceptions which have bulked largely in the recent history of psychiatry.

Since the last edition the author has moved materially from the standpoints which he then accepted. Notably he has largely thrown over the Kraepelinian classification which he regards as no longer corresponding to modern views. On the other hand, great importance is attached to cerebral birth trauma as a cause of psychoses which develop comparatively late in life, and to mal-development in the sphere of sex. These various new points of view are set out very fairly and with substantial supporting evidence which will enable the advanced student to estimate their validity.

The book is provided with numerous illustrations, with appendices containing intelligence tests, various tables, a comprehensive index and glossary, extensive reference to the literature, and in general contains a mass of well-ordered information in an easily accessible form.

B. H.

HISTORY OF SCIENCE AND PHILOSOPHY

History of Hindu Mathematics: A Source Book. Part II: Algebra. By BIBHUTIBHUSAN DATTA and AVADHESH NARAYAN SINGH. [Pp. xvi + 314, with 18 figures.] (Lahore: Motilal Banarsi Das, 1938. Rs. 7/8 or 13*s*.)

THE first instalment of this *History of Hindu Mathematics* was published in 1935, and it dealt with the evolution of numeral notation and arithmetic in India down to the seventeenth century of our era. The present volume covers the corresponding developments in algebra. There are introductory chapters on the technical terms, symbols and fundamental operations of Hindu algebra; but about three-quarters of the book is devoted to the theory of equations.

Like its predecessor, this volume contains numerous passages selected from the Hindu mathematical classics, and from unpublished manuscripts. These extracts are translated from the Sanskrit into English, grouped according to topics, and arranged in chronological order, with full explanations of the algebraic processes involved. References to previous memoirs on the subject are provided in footnotes. About half of the subject matter of the book is here published for the first time.

It may be regretted that so little is said of possible external influences on the development of Hindu algebra. However, as a source book and commentary, the work (which is well printed and adequately indexed) may be recommended to serious students of the subject, to whom the name of Dr. Datta will already be familiar through his labours in this field.

A. A.

The Chemical Studies of P. J. Macquer. By L. J. M. Colebby, M.A., M.Sc., Ph.D. [Pp. 132.] (London: George Allen & Unwin, Ltd., 1938. 6*s*. net.)

Since the author has not provided a preface it is not clear why he has selected Macquer for treatment in a monograph. It is quite true, as he says on the first page, that Macquer "was for many years his country's foremost authority on chemical matters, and by the variety of his interests, the accuracy of his observations, and the influence of his works, he is entitled to rank as one of the greatest of French chemists." It is stated that Macquer was descended from a noble Scottish family which had sacrificed its wealth and country for attachment to the Jacobite cause, although no authority for this assertion, which is controverted by Thomas Thomson, is given. The various publications of Macquer are briefly but adequately analysed, and it is made clear that he forwarded the cause of chemical science by his researches on arsenic, Prussian blue, solubilities, the combustion of the diamond (where the importance of the *flame* of a burning diamond in the theory of the combustion of carbon might well have been emphasised), etc.; his services in chemical technology in connection with dyeing, porcelain, assaying, glass, wine manufacture and as one of the earliest rubber technologists are discussed, and the great value of his books, the *Élémens de Chymie* and the *Dictionnaire de Chymie*, both of which were translated into English and other languages, is made evident. The book is provided with that adequate documentation now absolutely essential in any work on the history of science, however small, and it is clear that the author has in his accounts of Macquer's contributions gone to the original sources. The spelling "Margraaf" is given throughout although "Marggraf" seems more correct.

J. R. P.

Aristotle. Parts of Animals, with an English translation by A. L. Peck, M.A., Ph.D. **Movements of Animals and Progression of Animals,** with an English translation by E. S. Forster, M.A. Loeb Classical Library. [Pp. 556.] (London: William Heinemann. Ltd.; Cambridge, Mass.: Harvard University Press, 1937. Cloth, 10*s.* net; leather, 12*s.* 6*d.* net.)

This new volume of the Loeb Classical Library has the same useful features as its predecessors—a convenient size for slipping in the pocket and the original text on the left-hand page facing the translation on the right.

The *De Partibus Animalium* is of course one of the great classics of animal biology. The title is not very descriptive, for it deals more with physiology than with anatomy; it is a treatise on the functions of the tissues and organs. As Dr. F. H. A. Marshall points out in a Foreword, "as the earliest text-book on animal physiology in the world's history, this treatise will ever make its appeal, not only to the classical philosopher, but to all who are interested in the origin and growth of biological science." There is a valuable technical introduction and analysis by the translator, Dr. Peck. The translation is very readable, being freer and more colloquial than that by Ogle in the Oxford series.

The volume also contains two minor works of Aristotle, on animal movements and methods of locomotion, translated by Mr. Forster, of which the latter is of some interest as illustrating Aristotle's keenness of observation and interest in common things. It is a great mistake to think of Aristotle as merely a theoretical philosopher; he was actually a first-rate naturalist with an extraordinarily wide practical acquaintance with animals and their ways.

E. S. Russell.

The Nature of Man: Studies in Optimistic Philosophy. By Élie Metchnikoff. Revised by C. M. Beadnell, C.B., F.Z.S. [Pp. xviii + 210, with 18 figures, including 6 plates.] (London: C. A. Watts & Co., Ltd., 1938. 5*s.* net.)

This book is said to be a revised edition of a work by the Russian scientist Élie Metchnikoff; the English translation of which was edited by Sir P. Chalmers Mitchell. The dates and original title are not given, but the reviser C. M. Beadnell refers to the book as a product of the Victorian age and thinks it may consequently be received by the modern generation as a "has been" and "quite out of date." In other words is this a book that is likely to assist the young student? The subtitle is probably mistaken. The philosophy is extreme pessimism and often reactionary and anti-scientific. It seems more suited to the Trappist monk passing his time in his grave in profound contemplation of death. Indeed, the object of the book is to encourage an intensive search for the "instinct of death," an example of which is shown in the ephemerid flies. The hypothesis and arguments are those of the last century. The book has only a slight historical value and did not call for revision. To discuss whether increase of knowledge is desirable, is not merely silly, it is immoral.

P. J.

Critical Realism : Studies in the Philosophy of Mind and Nature. By G. DAWES HICKS, M.A., Ph.D., Litt.D. [Pp. xxiv + 346.] (London: Macmillan & Co., Ltd., 1938. 15*s*. net.)

IN this book Prof. Dawes Hicks collects a number of articles written at various times during the last twenty years which he has selected because he finds that " together they form a coherent whole, and may be regarded as the working out in detail of a point of view respecting the relation of mind to nature " which he has gradually attained. In addition to an account of the general point of view of critical realism, the book contains a considerable amount of discussion and criticism of the work of other philosophers—*e.g.* Broad, Bertrand Russell, Bradley, Stace, Meinong, Lotze—and a treatment of certain specific problems such as the nature of images and the philosophies of Spinoza and Leibnitz. There are also two chapters of particular interest to the scientifically minded on " The Dynamic Aspect of Nature " and " Professor Eddington's Philosophy of Nature," respectively.

The general idea of critical realism may be broadly indicated in the following way. Realism, according to Prof. Perry, whose definition Prof. Dawes Hicks accepts with a minor proviso, stands for the principle that " things may be, and are, directly experienced without owing either their being or their nature to that circumstance." The adjective " critical " is used somewhat in the Kantian sense as relating to an examination of the conditions of knowledge of such " things." We have, then, a situation in which there is a knowing mind, an independently existing object, and a cognitive act. The knowing mind is " essentially an activity that finds and discovers, and whose being is constituted by the awareness of what it finds and discovers," and the question whether it is " rightly conceived as constituting a substantial identity of existence " is left aside as an irrelevant metaphysical one. The main consideration is devoted to the independently existing object and the cognitive act, and to the relations between them.

In a brief review there is no opportunity of referring to the many interesting and important questions dealt with, and all that it is appropriate to do in a journal devoted to science is to indicate the difficulty of reconciling the author's viewpoint with modern physics. It seems impossible, for example, to justify what has become known as the " principle of rejection of unobservables " if there are physical things whose being or nature is independent of their being experienced. Modern physics recognises no qualities of an object whose nature is not determined by their being experienced. Position, velocity, mass, size, colour—indeed, all physical qualities—are relative to the conditions under which they are observed, and the whole character of physics is essentially determined by the postulate that not merely have we not been able to discover the absolute nature of these qualities, but the qualities themselves have no absolute meaning. The realist interpretation of the Michelson-Morley experiment, for instance, which demands the recognition of the Fitzgerald-Lorentz contraction as an actual objective physical fact, is powerless to explain the Kennedy-Thorndike experiment. Until an explanation of physical experiments compatible with the realist position as described by Prof. Dawes Hicks is shown to be admissible, it is impossible for physicists to adopt that position.

H. D.

MISCELLANEOUS

Personality: a Psychological Interpretation. By Gordon W. Allport. [Pp. xiv + 588, with 32 figures, including 1 plate.] (London: Constable & Co., Ltd., 1938. 16*s.* net.)

Personality in Formation and Action. By William Healy, M.D. Thomas W. Salmon Memorial Lecture. [Pp. 204.] (London: Chapman & Hall, Ltd., 1938. 8*s.* 6*d.* net.)

No book devoted to this important subject has yet appeared as valuable and as interesting as Prof. Allport's. The author insists throughout that, if psychology is to be true and complete, it must combine the positions both of the exact sciences and of those, like history and literature, which explore the significance of some *particular* event in nature or society. Even those psychologists who are occupied in the investigation of individual mental differences and of its most recent extension, factorial analysis, "have, as it were, studied hearts removed from their natural setting" (p. 16). He denies that mental "factors" in all individuals are identical, that they must be independent of one another and not overlap. To the doctrine of factors he apposes what he terms the doctrine of "functional autonomy," according to which "the dynamic sub-structures of which a personality is composed are unique integrations formed in the individual course of experience and heredity" (p. 245). He refuses to suppose that a very few basic (*e.g.* nutritional and sexual) motives or a rather larger number of instincts, common to all and presumably innate, can suffice to explain the endless variety of human interests, or that the driving forces of the adult personality are fundamentally of an infantile and archaic nature.

Like many other American psychologists, the author substitutes for the instinct the concept of "drive"—an internal organic impulse originally leading to the alleviation of some fundamental segmental organic tension. But he insists that the drive can serve only as a starting-point for a theory of motivation, and that it is supplanted in the mature personality by such motives as interests, sentiments, complexes, values, ambitions, attitudes, tastes and inclinations. These he includes under the not indefensible "omnibus" term "traits." But here, again, he insists that "no single trait—nor all traits together—determine behaviour all by themselves. . . . Only one maximally integrated activity takes place at any one time, and this activity is the product of a final convergent path wherein all available energy . . . is channelized to meet the present demand" (p. 313). These "traits" are flexible, interdependent dispositions, reached only at a relatively complex level of anaylsis.

But, in his view, "no two persons ever have precisely the same trait . . . The end product of unique determination can never be anything but unique" (p. 297). This, however, does not preclude the scientific aim of discovering general laws: "a general law may be a law that tells how uniqueness comes about" (p. 558).

The author grants the possibility of deducing by abstraction "common" traits, in contradistinction to "individual" traits (which are the only true traits). They are "those aspects of personality in respect to which most mature people within a given culture can be compared" (p. 300). He prepares an inventory of common traits suitable for this purpose.

Enough has been said to indicate the wise, sane, philosophical tone and the high practical, as well as theoretical, value of the book. Indeed, in great part it constitutes a critique of the principal modern views and schools of psychology in relation to the problems of personality. The index of subjects, which must have been entrusted to an amateur, badly needs revision in subsequent editions: it includes such useless headings as "Aotes," "barriers," "catastrophe," "*Cogito ergo sum,*" "Drosophila," "eunuchoid," "future," "ink blot," "mouth," "nicknames," "parson," "regions," "steam-boiler," "stereotype," "trend," and "weak pattern."

Dr. Healy's book is far less ambitious and less systematic than Prof. Allport's. But it makes delightful reading and testifies to the author's close study of modern psycho-pathology, neurology, sociology and industrial psychology. It is the fruit of thirty years' experience as a psychiatrist by one who may be described as the founder of the modern child guidance clinic and who has earned for himself a world-wide reputation as "the wise physician." He throws doubt on the "birth-trauma" hypothesis. He confesses to finding little clinical value in Jung's sub-divisions of the extravert and introvert. He insists that besides the urge for self-preservation (life) and the sexual urge (love) there is a third—the "will-to-experience" (the striving for self-enrichment). He bewails the general exaltation of emotional impulses and the distrust of intelligence, and he suggests that the University student should receive instruction in "the biological bases of personality formation, the instinctual urges and desires and the unconscious motivations that rule mankind . . ." (p. 193).

CHARLES S. MYERS.

Social Psychology. By DANIEL KATZ and RICHARD L. SCHANCK. [Pp. xvi + 700, with 1 plate and 20 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 18*s.* 6*d.* net.)

THIS is an excellent book and more than it modestly claims to be—a text-book for college students; for it will not only appeal to them as a stimulating text, but also to most people who are interested in social problems from one or other of their many angles. It should have a very wide circle of readers indeed. Written in a concise and clear style, it is illustrated throughout by apposite concrete examples which keep the mind constantly fixed upon the empirical data of everyday life from which its theoretical conclusions are drawn; and, even if it were for this reason alone (though it is not), the book is full of life and reality. It begins (Part I) with a description of the social world in which we all live; the world of human competition and co-operation, of folkways, mores and taboos, of fads and fashions, of conformity and non-conformity, of crime and custom. It continues (Part II) with a detailed analysis of the springs of human conduct, the motives that sway us in our social interactions with others. This section is documented with experimentally discovered evidence. The chapters of the next section (Part III) deal with the individual members of social groups from a genetic and clinical point of view, their socialisation, their assimilation of cultural values and prohibitions, their personality conflicts and problems. The final chapters (Part IV) discuss the changing world of the social engineer and planner. They include the study of society and culture, the eras of the rural community, of the growth of publics, and of the divisions of social classes, political parties, and forms of government. The authors write that they "have attempted to bring to the reader a social psychology which adequately covers both the subjective and objective aspects of the individual as he adjusts in the social and material world around him". They have certainly succeeded in their attempt in an admirable manner.

F. A.

Scientists are Human. By David Lindsay Watson. [Pp. xx + 249.] (London: C. A. Watts & Co., Ltd., 1938. 7*s.* 6*d.* net.)

This is a book that should be read by all those who believe that they are entitled to be called scientists. At the present time when fashions in science are so marked, it is salutary to be reminded that science is an activity of human beings and therefore likely to be distorted by the contemporary world outlook. This, of course, is not a new idea and the author mentions Spengler as having shown how science reflects the cultural processes affecting equally the social customs, art, architecture, philosophy and mathematics of any given age.

First comes a chapter throwing doubt even on the arguments of the classical logicians, and then a discussion of the scientific genius showing also how, whenever science becomes respectable, it tends to become fossilised through the agency of the universities, and the learned societies whose referees delay publication of papers that do not conform to the prevailing ideas. Thus Waterston's paper establishing the identity of temperature and average molecular velocity in a gas was stated by the Royal Society referee to be "nothing but nonsense, unfit even for reading before the Society," and Fourier's great paper on the propagation of heat was rejected by a committee of the French Academy, and this committee consisted of Laplace, Lagrange and Legendre!

There are many passages that might be quoted to show that the author has more than ordinary insight: for instance (p. 78), "The mystical is merely that which requires a leap of the imagination, which cannot be conveyed unambiguously in the language already acceptable in a given field. It follows that all important new work can rightly be accused of being mystical" and (p. 82), "The institutions and conventions of the scientific movement are at present organised to suit the unselfconscious man—the man who is incurious about how his mind works or how his motives come to him. The recent return of philosophy to a reputable status in the minds of scientists is a sign that the next oscillation has begun, and that the extroverts are losing their grip. There are always jobs to be done in science with which only the introvert personality can hope to grapple successfully."

After demolishing the faulty view that knowledge consists only in measurement, the author develops his own views on the similarity of forms and ideas and concludes, "It is my belief that the science of the future will more and more turn away from its present mechanistic devices towards the use of the intuitive discernment of similarity—as the foundation on which its whole structure rests" (p. 191).

A piquant, controversial, and sometimes biting commentary on those men of genius, pot-boilers, successful plagiarists and exhibitionists that are to be found in the world of science.

G. B. B.

Physical Science in Modern Life. By E. G. Richardson, B.A., Ph.D., D.Sc. [Pp. 256, with 60 figures, including 16 plates.] (London: The English Universities Press, Ltd., 1938. 8*s.* 6*d.* net.)

The author is to be congratulated on producing a book that should interest everyone. It is entirely non-mathematical, in the sense that no mathematical symbols are used, although the mathematics is really there. As would be expected, the mechanical part predominates, the chapters on Streams and Eddies and on Supersonics being particularly instructive. The chapter on Heat does not lend itself to such a variety of treatment, but this is compensated in that on Light, where infra-red photography, the photo-electric cell and television are treated.

On the borderline between Chemistry and Physics the subject of Colloids finds a place, and this chapter will appeal to all who are interested in matters varying from quicksands to fogs.

The author departs from the usual course in giving an interesting chapter on Sensation, of which one or two points call for comment. It is doubtful whether the fact that one becomes unconscious of a long-continued noise but conscious of its cessation, is due to the fatigue of the transmitting nerves; for it is common experience that the ticking of a clock in a room soon ceases to be heard although its stopping is immediately noticed. The feeble impulses due to the clock ticking can hardly produce fatigue in the transmitting nerve.

There can scarcely be anyone who would not enjoy spending a few instructive hours in reading the book.

S. G. S.

Physical Geography for Indian Students. By Cyril S. Fox, D.Sc., M.I.Min.E., F.G.S. [Pp. xii + 564, with 284 figures and 5 folding plates.] (London: Macmillan & Co., Ltd., 1938. 7*s.* 6*d.*)

Since the publication of H. F. Blanford's *Rudiments of Physical Geography*, last revised in 1884, there have been few, if any, books devoted exclusively to the physical aspects of geography in India, and Dr. Cyril S. Fox of the Geological Survey of India set himself the task of filling the gap by preparing a full and authoritative work. He secured the collaboration of several of his colleagues, including D. N. Wadia and J. A. Dunn, as well as of the Surveyor General of India and the Director of the Zoological Survey, each of whom prepared chapters. But the project never materialised and the matter was used instead to prepare an Indian edition of Simmons and Stenhouse's *Class-Book of Physical Geography*. Much new wine has thus been poured into an old and indeed old-fashioned bottle still half full of old wine. Excellent modern half-tone blocks appear side by side with old steel engravings and records of modern researches on the same page as references to work of fifty years ago. Indian students will be puzzled by the picture of a sundial at Sandringham and many other English allusions. The somewhat rigid methodology of the original book is retained: each chapter is introduced by a number of practical exercises. Some of these were never successful in England (*e.g.* " brush a piece of natural chalk with a stiff tooth brush in water and examine the sediment for small shells ") and will be incomprehensible in India. Similarly the author seems to have been unduly influenced by the viewpoints of the past, as when geomorphology remains unmentioned and a mountain in Burma is called Puppa Doung after the manner of seventy years ago. Despite these blemishes there is much of value in the book and it may be cordially recommended to British readers for the excellent series of photos and descriptive notes of geomorphological features in India.

L. D. S.

The South: Its Economic-Geographic Development. By A. E. Parkins. [Pp. x + 528, with 154 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 25*s*. net.)

The author indicates in his preface that his purpose is to describe, from an evolutionary point of view, the civilisation of the South, and to interpret it in its regional setting and its historical antecedents. Later he says explicitly that he has "made no conscious effort to keep the material . . . within the recognised boundaries of any one or two academic subject-matter fields." One might be pardoned for suspecting from this an excessive discursiveness and a lack of focus in the book. In fact, there is nothing of the sort. The central problem is the growth in area, in intensity and in complexity of the economic life of the South, and every chapter adds its quota of material and helps to give definition to the general picture. If a fault can be found, it is perhaps that the initial treatment of the physique, climate and soils of the region is rather slight as a foundation for the treatment of the human aspects superimposed upon it. But there is something of the reformer as well as of the economic geographer in Professor Parkins. His study of the soils, the agriculture, the forest resources and the mineral wealth, for example, does not stop short at their nature and distribution but brings out the mistakes that have been made, and are being made, in their exploitation, and points the way to wiser development. A word of special commendation is due for the effort the author has made to find solid ground for assessing the real cost to the community of inland water transport and for the thoroughness of his comparison of the advantages of the South, and of New England for the cotton-manufacturing industry. The work is copiously and appropriately illustrated and contains an adequate index and an excellent bibliography. Altogether it is a valuable contribution to the geographical literature on North America.

R. O. B.

Physiography of Eastern United States. By N. M. Fenneman. [Pp. xiv + 714, with 197 figures and 6 folding plates.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 36*s*. net.)

The term "physiography" is used in this book, and commonly in North America as approximately equivalent to "geomorphology," which is used in this country. It is the study of the forms of the land surface in their distribution and evolution, and is thus the area in which geography and geology overlap. It is only part of "physiography" as that term was used by Huxley, and later workers.

This book is made up of a well-arranged series of descriptive accounts of the natural landscapes of the eastern half of the United States, based on the very numerous memoirs of Federal and State surveyors, and of American geologists and geographers. The physiographic provinces are those adopted by the Geological Survey, with a few small variations of boundaries. These "provinces," and the principal "sections" into which they are subdivided, are shown on the eight plates of maps included. These are supplemented by about 200 figures in the text, mainly diagrammatic maps and sections but including a number of landscape photographs. A careful reading of any section, with its illustrations which are essential parts of the description, will enable anyone familiar with the terminology of modern geomorphology to visualise the landscapes clearly in as much detail as the scale allows. The description of a "section" is usually preceded by a paragraph on its general relations and ended by a summary of its physical history; but the greater part of the book is occupied by detailed descriptions of the several landscape-groups (or "tracts") such as The Black Belt of Alabama, The Everglades of Florida, and so on. These are grouped in a rational order, well-proportioned to the scientific rather than to the human importance of the areas, and well indexed; so that it is easy to find the account of any particular district.

Together with the companion volume on the Western United States this book forms an invaluable reference work on the topography of that country, well illustrated and abundantly documented so that the reader can readily refer to the original works. It is likely to take rank as the standard work on its subject. It is a great monument to the abilities and industry of its author, who has long been recognised as one of the leading students and teachers of geomorphology in America.

C. B. F.

The Air and Its Mysteries. By C. M. BOTLEY. [Pp. xvi + 296, with 1 coloured plate, 16 black and white plates and 23 figures.] (London: G. Bell & Sons, Ltd., 1938. 8*s*. 6*d*. net.)

HERE is another book that aims at giving to the intelligent general public a simplified version of what is known about the air. It should be noticed at once that it is the air, and not merely the physics of the air (meteorology), that forms the subject matter. Meteorology, it is true, is dealt with very faithfully, but the book goes far beyond that, ranging, for example, over the problem of flight, the influence of wind on plant and animal life, the function of air in human breathing, and Wagner's thunderstorm music. This mention of Wagner brings up the second major characteristic of this particular book: throughout the work the author has enriched the treatment by drawing on a wide acquaintance not only with science and human affairs, but also with literature, music and art. The result is very satisfying; the content is sound and the style attractive. In short, if the reviewer's enjoyment of it is a fair criterion, this is a "popular" book which should become very popular indeed.

R. O. B.

Elements of Statistical Method. By ALBERT E. WAUGH. [Pp. xvi + 381, with 43 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 21*s*. net.)

THIS book provides instruction in the use of the simpler statistical tools with a bias towards economic applications. It covers the description of frequency distributions, the use of the normal curve tables, treatment of "historical data" (*i.e.* time series and index numbers), and simple and multiple correlation. The last two chapters of the book contain useful notes on the collection and presentation of statistical data.

Especial care is devoted to emphasising the dangers of rash applications of statistical method to data which do not justify such treatment.

The tone of the book is somewhat didactic. Rules of procedure are sometimes given with inadequate explanation. This would have a confusing effect on the reader. An example is the "rule" for adjusting N in small samples given on page 153. This feature does not, however, seriously detract from the value of the book in providing a pleasant path for persons needing to acquaint themselves with the use of elementary statistical method.

A technical error was noticed on page 267. In the equation in the middle of the page, reading

$$r' = \ldots = 0{\cdot}441$$

r' should be replaced by $(r')^2$. The incorrect value of r' is used in the subsequent pages.

N. L. J.

Dictionary of Scientific Terms. By C. M. BEADNELL, C.B., F.Z.S. [Pp. x + 235.] (London: C. A. Watts & Co., Ltd., 1938. Library edition, 5*s*. net; "Thinker's Library" edition, 1*s*. net.)

To compress the nomenclature of science into 235 small pages, so that the results may appeal to the specialist, is no longer possible. The chemist and the botanist will not turn to Beadnell for information about chemical and botanical terms respectively. On the other hand, the chemist who meets with unfamiliar botanical terms will find something to satisfy his curiosity, and *vice versa*. The person, however, who will find the book most useful is the educated non-scientist who is not prepared to skip every passage which contains a few technical terms, and it is clear from the author's preface that these are the readers for whom especially he has prepared his volume. For this purpose it may be recommended.

The book suffers from the inevitable defects of compression. The chemist (for example, the reviewer) feels vaguely embarrassed when he finds "oxygen" paragraphed together with "oxygnathous," "oxyntic," and "oxyphilic," and regrets the absence of a definition of "oxidation," a term which often causes serious difficulty to the non-specialist.

Some of the definitions will not satisfy the critical. "Amphoteric" is quite well explained by "capable of acting as acid or base," but scarcely by the alternative definition, "Neutral—not acid nor alkaline."

"*Test.* (1) Testa (*q.v.*)" should not be followed by "*Testa.* (1) Test (*q.v.*)."

The dictionary gives rather more copious information on the biological than on the physical and chemical side.

One cannot expect a whole technical library for a shilling, at which price the book appears in "The Thinker's Library," and those who expend a modest "bob" will find that they have received good value for their money. The printing and production of both editions is excellent, and the binding of the five shilling Library Edition is very substantial.

K. C. B.

The Official Year-Book of the Scientific and Learned Societies of Great Britain and Ireland. Fifty-fifth annual issue. [Pp. viii + 169.] (London : Charles Griffin & Co., Ltd., 1938. 10*s.* net.)

ONCE again we greet the appearance of this valuable reference-book, which collects and adequately indexes a mass of information regarding the societies' officers, membership fees, meetings and publications. The writer uses it constantly and has very rarely known it to fail him. Its slim bulk, pleasant print and good binding are, moreover, advantages too often lacking in volumes of its class. A few minor improvements might be considered for the next edition. The address given most prominently is in some cases not that at which correspondence can be sure of immediate attention, but merely the meeting-place of the society ; for instance, "The Hunterian Society, Simpson's Restaurant, Cheapside, E.C.2" ; the Secretary's name and address appearing later on. Any special qualifications, as well as the fees, required for membership, should surely be mentioned ; the number of members might be noted in more cases ; and an effort might be made to give a postal address for every scientist or official mentioned. These improvements depend, no doubt, less on the editor, whose work is admirably done, than on the full co-operation of those officers of societies who are asked for information. Perhaps some of them, reading this notice, may be moved to devote particular attention to their returns for the next issue, and make the volume an even more complete conspectus of the world of English science.

P. J. E.

CORRESPONDENCE

The Editor,
"Science Progress."

Dear Sir,

Please permit me to reply to Prof. MacBride's attack in your columns on my *More Difficulties of the Evolution Theory*. Parturiunt montes, nascitur ridiculus mus. Although Prof. MacBride says he has not space to "expose all the errors" in my book, he devotes about one-twelfth of his review to such "errors," in the remainder he airs his views on evolution. He points out two "errors" and one untenable argument of mine.

My first "error" is the assertion that the human os coccyx is not a relic of an ancestral tail. Prof. MacBride thinks that this is an error because "in the embryo it appears as a short tail which is regarded by every comparative anatomist as evidence that the human skeleton has been evolved from that of a simian ancestor." Quite apart from the fact that apes have no tail and monkeys are not the only vertebrates that have long tails, some comparative anatomists, *e.g.* Profs. Max Westenhof and A. Fleischmann, do not believe that the os coccyx is the remnant of a long-tailed ancestor.

My second "error" is, "According to Mr. Dewar . . . all these varied types (of fossil horses) wandered in from the north." This is a travesty of what I say, *viz.*: "Since the Palæocene 37 new families of mammals make their appearance in the rocks of Europe and North America. There is no fossil proof of the origin of any of these from any other known family. They seem to be immigrants from the North; they make their appearances at various times. From this it is not unreasonable to suppose that the various types of horses found fossil in Europe and North America may likewise have been immigrants from the North, and that the one-toed species were the latest immigrants."

In his attempt to controvert my assertion that the unfossiliferous nature of all pre-Cambrian rocks, coupled with the large number of fossils in the Cambrian rocks, is a very serious objection to the evolution theory, Prof. MacBride writes: "If we reflect that . . . all pre-Cambrian rocks are of indurated material in which no delicate structure could survive, the value of this argument of Mr. Dewar's must be apparent!" That this statement of Prof. MacBride is incorrect is easily demonstrated. For example, the pre-Cambrian Cuddapah group of India consists of "Normal sedimentary deposits" (P. Lake). The entire series of Cuddapah rocks," writes D. N. Wadia (*Geology of India*, p. 72), "are totally unfossiliferous, no signs of life being met with in this vast pile of marine sediments. This looks quite inexplicable, since not only are the rocks very well fitted to contain and preserve some relics of the seas in which they were formed, but also all mechanical disturbances, which usually obliterate such relics, are absent from them. . . . (In) . . . formations immediately subsequent to the Cuddapahs . . . we find evidence of fossil organisms which, though the earliest animals to be discovered, are by no means the simplest or the most primitive."

Yours, etc.,
Douglas Dewar.

The Editor of
"Science Progress."

Dear Sir,

I should like to deal with Mr. Douglas Dewar's reply to my review on Evolution which appeared in your last issue.

I wish first of all to get into Mr. Dewar's head that evolution is no longer a theory but a proven fact experimentally demonstrated in the laboratory. I consider that this is an epoch-making event, the greatest discovery ever made in Biology.

He asserts that the coccyx in the human skeleton cannot be the vestige of the tail of a Simian ancestor because Prof. Fleischmann does not think so. The coccyx in the human skeleton is represented in the embryo by a tail of decent length and no sane man can doubt for an instant that the coccyx must be the remnant of the tail of an ape-like ancestor.

The succession of horse-like ancestors found in North America, the so-called lineage series, was what convinced Huxley finally of the truth of the Evolution theory during his visit to America in 1878. Mr. Dewar tries to get over this evidence by supposing that the succession of horse ancestors wandered in from the north and that there is no evidence that one was derived from the other. With all respect, Mr. Dewar does not know what he is talking about. In these " lineage series " we find a great mass of beds deposited in *one place* and in them we find the whole series of ancestors from the four-toed precursor to the modern one-toed horse. It is impossible to say where one species ends and the next one begins, the gradation is so perfect. No "wandering in from the North" will explain this; evolution took place in the locality where the beds were deposited. There are equally good lineage series in Britain, but as they are concerned with sea urchins—animals unfamiliar to the laity—the lineage series of the horse are those most usually described to the non-zoological public.

Finally in his book Mr. Dewar adduced as a conclusive argument against evolution the unfossiliferous character of the Cambrian rocks. From his reply it is obvious that this was a slip of the pen, and that he meant to say pre-Cambrian. But even so his statement is not true. In the Huronian rocks of North American fossils have been found and in the Torridonian rocks of Scotland well-preserved worm tubes appear bearing unmistakable impressions of the bodies of their former occupants.

Faithfully yours,
E. W. MacBride.

BOOKS RECEIVED

(Publishers are requested to notify prices.)

The Theory of Functions. By E. C. Titchmarsh, M.A., F.R.S., Savilian Professor of Geometry in the University of Oxford. Second edition. London: Oxford University Press, 1939. (Pp. x + 454.) 25*s*. net.

College Algebra. By Louis J. Rouse, Ph.D., Assistant Professor of Mathematics, University of Michigan. Second edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1939. (Pp. xiv + 462, with 22 figures.) 11*s*. net.

Trigonometry. By Howard K. Hughes, Assistant Professor of Mathematics, and Glen T. Miller, Instructor in Mathematics, Purdue University. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. (Pp. viii + 268, with 95 figures.) 7*s*. 6*d*. net.

Introduction to the Calculus. Part I. By Samuel Beatty, M.A., Ph.D., Professor in the Department of Mathematics and Dean of the Faculty of Arts, University of Toronto, and James T. Jenkins, B.A., Head of the Department of Mathematics, Jarvis Collegiate Institute, Toronto. Toronto: The University of Toronto Press; London: Humphrey Milford, 1938. (Pp. 650, with 170 figures.) 25*s*. net.

Applied Mathematics in Chemical Engineering. By Thomas K. Sherwood, Associate Professor of Chemical Engineering, and Charles E. Reed, Assistant Professor of Chemical Engineering, Massachusetts Institute of Technology. Chemical Engineering Series. New York and London: McGraw-Hill Publishing Co., Ltd., 1939. (Pp. xii + 403, with 113 figures.) 24*s*. net.

Everyman's Astronomy. By Mary Proctor, F.R.A.S. London: The Scientific Book Club, 1939. (Pp. x + 246, with 23 plates.) 2*s*. 6*d*. net.

An Introduction to the Study of Stellar Structure. By S. Chandrasekhar, Yerkes Observatory. Astrophysical Monographs. U.S.A.: University of Chicago Press; Great Britain and Ireland: Cambridge University Press, 1939. (Pp. x + 509, with 38 figures.) 50*s*. net.

Admiralty Weather Manual 1938. Published by order of the Lords Commissioners of the Admiralty. London: H.M. Stationery Office, for the Hydrographic Department, Admiralty. (Pp. xx + 496, with 167 figures, including 71 plates.) 10*s*. 6*d*. net.

The Drama of Weather. By Sir Napier Shaw, Honorary Member and sometime President of the International Meteorological Committee. Second edition. Cambridge: at the University Press, 1939. (Pp. xiv + 307, with frontispiece and 109 figures.) 10*s*. 6*d*. net.

Physical and Dynamical Meteorology. By David Brunt, M.A., B.Sc., Professor of Meteorology in the University of London. Second edition. Cambridge: at the University Press, 1939. (Pp. xxiv + 428, with 119 figures.) 25*s*. net.

Physics of Today. By John A. Clark, Chairman of the Standing Committee on Science for the Public High Schools of New York City, Frederick Russell Gorton, Head of the Department of Physics and Astronomy, Michigan State Normal College, and Francis W. Sears, Assistant Professor of Physics, Massachusetts Institute of Technology. London: Constable & Co., Ltd., 1939. (Pp. vi + 644, with 23 plates and 750 figures.) 8*s*. 6*d*. net.

Introductory College Physics. By Oswald Blackwood, Ph.D., Professor of Physics, the University of Pittsburgh. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1939. (Pp. xiv + 487, with 425 figures.) 17*s*. 6*d*. net.

Modern Magnetism. By L. F. Bates, Ph.D., D.Sc., F.Inst.P., Lancashire-Spencer Professor of Physics, University College, Nottingham. Cambridge: at the University Press, 1939. (Pp. x + 340, with 97 figures.) 16*s*. net.

The Fundamentals of Electro-Magnetism. By E. Geoffrey Cullwick, M.A., A.M.I.E.E., A.M.E.I.C., M.A.I.E.E., Professor and Head of the Department of Electrical Engineering in the University of Alberta, Canada. Cambridge: at the University Press, 1939. (Pp. xxvi + 352, with 3 plates and 145 figures.) 18*s*. net.

Electricity. By T. F. Wall, D.Sc., D.Eng., M.I.E.E., A.M.I.C.E., Lecturer-in-Charge, Electrical Engineering Department, the University of Sheffield. The Home University Library. London: Thornton Butterworth, Ltd., 1939. (Pp. 256, with 54 figures.) 2*s*. 6*d*. net.

The Nature of the Atom. By G. K. T. Conn, M.A., B.A., Ph.D. Blackie's Tracts on Recent Physics. London and Glasgow: Blackie & Son, Ltd., 1939. (Pp. x + 115, with 3 plates and 15 figures.) 3*s*. 6*d*. net.

The Wave Nature of the Electron. By G. K. T. Conn, M.A., Ph.D. Blackie's Tracts on Recent Physics. London and Glasgow: Blackie & Son, Ltd., 1938. (Pp. x + 78, with 3 plates and 17 figures.) 3*s*. 6*d*. net.

Electron Optics. By the Research Staff of Electric and Musical Industries Limited. Compiled and written by Otto Klemperer. Cambridge Physical Tracts. Cambridge: at the University Press, 1939. (Pp. x + 107, with frontispiece and 47 figures.) 6*s*. net.

Theory and Applications of Electron Tubes. By Herbert J. Reich, Ph.D., Associate Professor of Electrical Engineering, University of Illinois. New York and London: McGraw-Hill Publishing Co., Ltd., 1939. (Pp. xviii + 670, with 512 figures.) 30*s*. net.

The Principles of Electric Power Transmission by Alternating Currents. By H. Waddicor, B.Sc., A.M.I.E.E. Fourth edition. London: Chapman & Hall, Ltd., 1939. (Pp. xxii + 458, with 164 figures.) 21*s*. net.

Electricity Meters and Meter Testing. By G. W. Stubbings, B.Sc., F.Inst.P., A.M.I.E.E. Vol. VI of a Series of Monographs on Electrical Engineering under the Editorship of H. P. Young. London: Chapman & Hall, Ltd., 1939. (Pp. x + 216, with 62 figures.) 12*s*. 6*d*. net.

Circuits and Machines in Electrical Engineering. By John O. Kraehenbuehl, M.S., E.E., Associate Professor of Electrical Engineering, and Max A. Faucett, M.S., E.E., Assistant Professor of Electrical Engineering, University of Illinois. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1939. (Pp. viii + 691, with 433 figures.) 22*s*. 6*d*. net.

Tensor Analysis of Networks. By Gabriel Kron, Engineering General Department, General Electric Co., Schenectady, N.Y. General Electric

Series. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1939. (Pp. xxiv + 635, with 251 figures.) 37*s.* 6*d.* net.

Supersonics. The Science of Inaudible Sounds. By Robert Williams Wood, Professor of Experimental Physics in the Johns Hopkins University. The Charles K. Colver Lectures, 1937. Providence, R.I.: Brown University, 1939. (Pp. viii + 158, with 42 figures.) $2.00.

Applied Acoustics. By Harry F. Olson, E.E., Ph.D., Acoustic Research Director, Research Laboratories, RCA Manufacturing Co., Inc., and Frank Massa, B.S., M.Sc., Sound Engineering Division, RCA Manufacturing Co., Inc. Second edition. Philadelphia: P. Blakiston's Son & Co., Inc., 1939. (Pp. xviii + 494, with 278 figures.) $5.50.

Electro-Acoustics. By Prof. Dr. Erwin Meyer, at the Institut für Schwingungsforschung, Technische Hochschule, Berlin. With a Foreword by Prof. C. L. Fortescue, O.B.E., M.A. London: G. Bell & Sons, Ltd., 1939. (Pp. xii + 117, with 84 figures, including 12 plates.) 10*s.* net.

A Primer of the Internal Combustion Engine. By H. E. Wimperis, C.B., C.B.E., M.A. Second edition. London: Constable & Co., Ltd., 1939. (Pp. xvi + 143, with 60 figures.) 5*s.* net.

Carburation and Carburettors. By Charles H. Fisher, A.M.I.Mech.E., M.I.A.E. London: Chapman & Hall, Ltd., 1939. (Pp. xii + 316, with 132 figures, including 13 plates.) 18*s.* net.

Aircraft Design. Vol. I: Aerodynamics. By C. H. Latimer Needham, M.Sc.(Eng.), B.Sc., F.R.Ae.S., M.I.Ae.E., Lecturer in Aeroplane Design, Battersea Polytechnic. London: Chapman & Hall, Ltd., 1939. (Pp. xii + 215, with 145 figures, including 8 plates.) 13*s.* 6*d.* net.

The Engineers' Manual. By Ralph G. Hudson, S.B., Professor of Electrical Engineering, Massachusetts Institute of Technology. Second edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1939. (Pp. iv + 340, with numerous figures.) 13*s.* 6*d.* net.

The Nature of Crystals. By A. G. Ward, B.A., Scholar of Trinity College, Cambridge. Blackie's Tracts on Recent Physics. London and Glasgow: Blackie & Son, Ltd., 1938. (Pp. xii + 114, with 4 plates and 52 figures.) 3*s.* 6*d.* net.

Photography. Its Principles and Practice. By C. B. Neblette, F.R.P.S., Counselor and Administrative Head, Department of Photographic Technology, Rochester Athenæum and Mechanics Institute. Third edition—second printing. New York: D. van Nostrand Co., Inc.; London: Chapman & Hall, Ltd., 1939. (Pp. xii + 590, with 229 figures.) 30*s.* net.

Photography by Infrared. Its Principles and Applications. By Walter Clark, Ph.D., F.I.C., F.R.P.S., Kodak Research Laboratories. London: Chapman & Hall, Ltd., 1939. (Pp. xii + 397, with frontispiece and 103 figures, including 47 plates.) 25*s.* net.

Photographic Chemicals and Solutions. By J. I. Crabtree and G. E. Matthews, Kodak Research Laboratories, Rochester, N.Y. London: Chapman & Hall, Ltd., 1939. (Pp. viii + 360, with 95 figures.) 21*s.* net.

The Theory and Practice of General Science. By H. S. Shelton. London: Thomas Murby & Co., 1939. (Pp. 123.) 3*s.* 6*d.* net.

General Science Test Papers. By Harold Goodburn, Science Master, King's School, Canterbury. London: Methuen & Co., Ltd., 1939. (Pp. viii + 87.) 1*s.* 6*d.*

Intermediate Chemical Calculations. By J. R. Partington, M.B.E., D.Sc., Professor of Chemistry in the University of London, Queen Mary College, and Kathleen Stratton, M.Sc., Lecturer in Chemistry in Queen Mary College. London: Macmillan & Co., Ltd., 1939. (Pp. x + 239.) 6*s*. 6*d*.

Intermediate Chemistry. By T. M. Lowry, C.B.E., M.A., D.Sc., F.R.S., Professor of Physical Chemistry in the University of Cambridge, and A. C. Cavell, B.A., B.Sc., Senior Science Master at Uppingham School. Second edition. London: Macmillan & Co., Ltd., 1939. (Pp. xvi + 880, with 188 figures.) 12*s*. 6*d*.

Gmelins Handbuch der anorganischen Chemie. 8. Auflage. Herausgegeben von der Deutschen Chemischen Gesellschaft. System-Nummer 68: Platin, Teil B, Lieferung 1: Physikalische Eigenschaften des Metalls (bis Thermische Eigenschaften). Berlin: Verlag Chemie, G.m.b.H., 1939. (Pp. 72, with 7 figures.) Price abroad, RM.8.25.

Annual Reports on the Progress of Chemistry for 1938. Vol. XXXV. London: The Chemical Society, 1939. (Pp. 447.) 13*s*. net.

The Economics of Chemical Industries. By Edward H. Hempel, Ph.D., D.C.S., Research Associate in Industrial Engineering, Columbia University. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1939. (Pp. x + 259.) 15*s*. net.

The Chemical Formulary, Vol. IV. Editor-in-Chief, H. Bennett. New York: Chemical Publishing Co. of N.Y., Inc.; London: Chapman & Hall, Ltd., 1939. (Pp. xiv + 632.) 25*s*. net.

Metallurgy for Engineers. By E. C. Rollason, M.Sc., Senior Lecturer in the Department of Metallurgy, University of Birmingham. London: Edward Arnold & Co., 1939. (Pp. viii + 272, with 155 figures.) 10*s*. net.

Protective Coatings for Metals. By R. M. Burns, Ph.D., Assistant Chemical Director, Bell Telephone Laboratories, and A. E. Schuh, Ph.D., Director of Research, United States Pipe and Foundry Company. American Chemical Society Monograph Series. New York: The Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1939. (Pp. 407, with 89 figures.) 32*s*. 6*d*. net.

Steel and its Heat Treatment. Vol. II: Engineering and Special-Purpose Steels. By D. K. Bullens and the Metallurgical Staff of the Battelle Memorial Institute. Fourth edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1939. (Pp. x + 491, with 189 figures.) 25*s*. net.

Plastic Working of Metals and Power-Press Operations. By E. V. Crane, Ph.B., M.E. Second edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1939. (Pp. xii + 450, with 369 figures.) 25*s*. net.

Sampling and Analysis of Carbon and Alloy Steels. Methods of the Chemists of the Subsidiary Companies of the United States Steel Corporation as revised to 1937. New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1938. (Pp. 356, with 20 figures.) 22*s*. 6*d*. net.

Press Tool Making. Including Hardening and Tempering. By Ernest Perry. Second edition. London: The Technical Press, Ltd., 1939. (Pp. viii + 96, with 35 figures.) 3*s*. 6*d*. net.

The Fundamentals of Chemical Thermodynamics. Part I: Elementary Theory and Electrochemistry. By J. A. V. Butler, D.Sc., Lecturer in Chemistry in the University of Edinburgh. Third edition. London: Macmillan & Co., Ltd., 1939. (Pp. xvi + 271, with 68 figures.) 7*s*. 6*d*.

Physico Chemical Experiments. By Robert Livingston, Associate Professor of Physical Chemistry, University of Minnesota. New York and London: Macmillan & Co., Ltd., 1939. (Pp. xii + 257, with 70 figures.) 10*s*.

Experimental Methods in Gas Reactions. By A. Farkas, Dr.Phil.Nat., Dr.Ing., Dept. of Physical Chemistry, Hebrew University, Jerusalem, and H. W. Melville, D.Sc., Ph.D., Fellow of Trinity College, Assistant Director, Colloid Science Laboratory, Cambridge. London: Macmillan & Co., Ltd., 1939. (Pp. xvi + 389, with 266 figures.) 30*s*. net.

Physical Constants of Hydrocarbons. Vol. I: Paraffins, Olefins, Acetylenes, and Other Aliphatic Hydrocarbons. By Gustav Egloff, Director of Research, Universal Oil Products Company, Chicago, Illinois. American Chemical Society Monograph Series. New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1939. (Pp. 403.) 45*s*. net.

Colloidal Phenomena. An Introduction to the Science of Colloids. By Ernst A. Hauser, Ph.D., Associate Professor of Chemical Engineering, Massachusetts Institute of Technology. New York and London: McGraw-Hill Publishing Co., Ltd., 1939. (Pp. xx + 294, with 123 figures.) 18*s*. net.

Theoretical Electrochemistry. By N. A. McKenna, B.A. London: Macmillan & Co., Ltd., 1939. (Pp. xiv + 469, with 109 figures.) 15*s*.

The Principles of Electrochemistry. By Duncan A. MacInnes, Associate Member, The Rockefeller Institute for Medical Research. New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1939. (Pp. 478, with 147 figures.) 30*s*. net.

The Electrochemistry of Gases and Other Dielectrics. By G. Glockler, Professor of Physical Chemistry, Institute of Technology, and S. C. Lind, Professor of Physical Chemistry, Dean of the Institute of Technology, University of Minnesota. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1939. (Pp. xiv + 469, with 82 figures.) 30*s*. net.

The Elements of Fractional Distillation. By Clark Shove Robinson and Edwin Richard Gilliland, Associate Professors of Chemical Engineering, Massachusetts Institute of Technology. Third edition. International Chemical Series. New York and London: McGraw-Hill Publishing Co., Ltd., 1939. (Pp. xii + 267, with 104 figures.) 18*s*. net.

Lehrbuch der organischen Chemie. Von Paul Karrer, Professor an der Universität Zürich. Sixth edition. Leipzig: Georg Thieme, 1939. (Pp. xxiv + 989, with 6 figures, including 1 plate.) RM.34.–, paper covers; RM.36.–, bound; 25 per cent. reduction abroad.

Qualitative Organic Chemistry. By Neil Campbell, Lecturer in Organic Chemistry, University of Edinburgh. London: Macmillan & Co., Ltd., 1939. (Pp. x + 213, with 14 figures.) 8*s*. 6*d*.

The Plant Alkaloids. By Thomas Anderson Henry, D.Sc., Director, Wellcome Chemical Research Laboratories. Third edition. London: J. & A. Churchill, Ltd., 1939. (Pp. viii + 689.) 42*s*. net.

The Chemistry of Milk. By W. L. Davies, Ph.D., D.Sc., F.I.C., Research Dairy Chemist and Analyst, National Institute for Research in Dairying, Shinfield, near Reading. Vol. X of a Series of Monographs on Applied Chemistry under the Editorship of E. Howard Tripp, Ph.D. Second edition. London: Chapman & Hall, Ltd., 1939. (Pp. xiv + 534, with 27 figures.) 25*s*. net.

Handbook of Food Manufacture. By Dr. F. Fiene, Food Research Chemist, and Saul Blumenthal, B.S., Consulting Food Chemist and Director of Shirley Laboratories, New York City. New York : Chemical Publishing Co., Inc. ; London : Chapman & Hall, Ltd., 1938. (Pp. vi + 605.) 25*s.* net.

The Structure and Composition of Foods. Vol. IV : Sugar, Sirup, Honey, Tea, Coffee, Cocoa, Spices, Extracts, Yeast, Baking Powder. By Andrew L. Winton, Ph.D., sometime State and Federal Chemist, and Kate Barber Winton, Ph.D., sometime State and Federal Microscopist. New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1939. (Pp. xxxvi + 580, with 134 figures.) 45*s.* net.

Casein and its Industrial Applications. By Edwin Sutermeister and Frederick L. Browne. Second edition. American Chemical Society Monograph Series. New York : Reinhold Publishing Corporation ; London : Chapman & Hall, Ltd., 1939. (Pp. 433, with coloured frontispiece and 50 figures.) 32*s.* 6*d.* net.

An Introduction to Crystal Chemistry. By R. C. Evans, M.A., Ph.D., B.Sc., Demonstrator in the Department of Mineralogy and Petrology, University of Cambridge. Cambridge : at the University Press, 1939. (Pp. xii + 388, with 113 figures.) 18*s.* net.

Geology. Principles and Processes. By William H. Emmons, Professor of Geology, George A. Thiel, Associate Professor of Geology, Clinton R. Stauffer, Professor of Geology, University of Minnesota, and Ira S. Allison, Professor of Geology, State Agricultural College, Corvallis, Oregon. Second edition. New York and London : McGraw-Hill Publishing Co., Ltd., 1939. (Pp. xii + 451, with frontispiece and 468 figures.) 21*s.* net.

The Birth and Development of the Geological Sciences. By Frank Dawson Adams, Ph.D., D.Sc., F.R.S. London : Baillière, Tindall & Cox, 1938. (Pp. vi + 506, with frontispiece, 14 plates and 79 figures.) 22*s.* 6*d.* net.

Geology of London and South-East England. By G. M. Davies, M.Sc., F.G.S., Reader in Geology, Birkbeck College, University of London. London : Thomas Murby & Co., 1939. (Pp. viii + 198, with 4 plates and 63 figures.) 7*s.* 6*d.* net.

Earthquakes and Other Earth Movements. By John Milne, F.R.S., F.G.S., Professor of Mining, Geology, and Seismology in the Imperial College of Engineering, Tokyo, Japan, 1876–95. New edition, revised and re-written by A. W. Lee, D.Sc., A.R.C.S., D.I.C. London : Kegan Paul, Trench, Trubner & Co., Ltd., 1939. (Pp. xvi + 244, with 83 figures, including 24 plates.) 10*s.* 6*d.* net.

Introduction to Floral Mechanism. By S. G. Jones, D.Sc., Department of Botany, University of Glasgow. London and Glasgow : Blackie & Son, Ltd., 1939. (Pp. xii + 274, with frontispiece, 2 plates and 71 figures.) 10*s.* net.

Floral Morphology. A New Outlook, with special reference to the Interpretation of the Gynæceum. By E. R. Saunders, F.L.S., Sometime Fellow and Lecturer in Botany, Newnham College, Cambridge. Vol. II, with additional illustrations and index for Vol. I. Cambridge : W. Heffer & Sons, Ltd., 1939. (Pp. xiv + 477, with 42 figures.) 10*s.* 6*d.* net.

Spore Discharge in Land Plants. By C. T. Ingold, Ph.D., F.L.S., Lecturer in Charge of the Department of Botany at University College, Leicester. Oxford : at the Clarendon Press ; London : Humphrey Milford, 1939. (Pp. viii + 178, with 75 figures.) 7*s.* 6*d.* net.

Diseases of Fruits and Hops. By H. Wormald, D.Sc., A.R.C.S., D.I.C., Plant Pathologist and Assistant Director, East Malling Research Station, Kent. London: Crosby Lockwood & Son, Ltd., 1939. (Pp. 290, with 40 plates and 24 figures.) 17*s.* 6*d.* net.

A Gallery of Gum Trees. By A. W. D'Ombrain, M.B., Ch.M., D.O.M.S. London: H. K. Lewis & Co., Ltd., 1938. (Pp. 53, with 21 illustrations by Tydfyl Evans.) 14*s.* 6*d.* net.

Hundert Jahre Zellforschung. Von L. Aschoff, E. Küster, W. J. Schmidt. Protoplasma-Monographien, Band 17. Berlin: Verlag von Gebrüder Borntraeger, 1938. (Pp. xii + 285.) RM.16.–.

German-English Botanical Terminology. By Helen Ashby, Ph.D., F.I.C., formerly Lecturer in Plant Pathology, The Horticultural College, Swanley, Eric Ashby, D.Sc., D.I.C., Professor of Botany, The University, Sydney, Dr. Harald Richter and Dr. Johannes Bärner, of the Biologische Reichsanstalt, Berlin-Dahlem. Murby's German-English Terminologies. London: Thomas Murby & Co., 1938. (Pp. xi + 195.) 10*s.* net.

Manual of Indian Silviculture. Part I: General Silviculture. By H. G. Champion, M.A., Silviculturist, Forest Research Institute, Dehra Dun. Part II: Silvicultural Systems. By Sir Gerald Trevor, C.I.E., Inspector-General of Forests to the Government of India. London: Oxford University Press, 1939. (Pp. xvi + 374, with 33 plates, 8 diagrams and 2 maps.) 30*s.* net.

Soil Analysis. A Handbook of Physical and Chemical Methods. By C. Harold Wright, M.A., F.I.C., formerly Senior Agricultural Chemist, Nigeria. Second edition. London: Thomas Murby & Co.; New York: D. van Nostrand Co., Inc., 1939. (Pp. x + 276, with 10 figures.) 12*s.* 6*d.* net.

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SCIENCE PROGRESS

THE COSMOLOGICAL CONTROVERSY

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MORE than twenty years ago the fundamental equations of the theory of relativity reached a settled form which has lasted to the present day. One of the consequences deduced from them is that matter is subject to a force called *cosmical repulsion.* This is of the nature of a scattering force, which tends to make all objects increase their mutual distances apart and is not directed from any one absolute centre; but as viewed from an arbitrarily chosen point it amounts to a repulsive force from that point which increases proportionately to the distance. Clearly the best chance of detecting such a force is to observe objects as remote as possible. There is no sign of cosmical repulsion in the systems ordinarily studied in dynamical astronomy, presumably because they cover too small a distance; but it was suggested that it might be manifested in the motions of the spiral nebulæ.

This prediction, when it was made, was necessarily conditional. Firstly, the status of the spiral nebulæ at that time had not been definitely settled; though a number of astronomers believed them to be external galaxies (as Sir William Herschel had done a hundred years before), others believed them to be comparatively small objects at no more than ordinary stellar distances. Secondly, even anticipating our present knowledge of their enormous distances, there was no guarantee that such distances would be sufficient to make the cosmical repulsion appreciable, for the theory did not suggest any particular magnitude of the force. Just as relativity theory provides the form of Einstein's law of gravitation but does not determine the "constant of gravitation," so it provides the form of the law of cosmical repulsion but does not determine the "cosmical constant" which fixes the amount of repulsion. Thirdly, even if strong enough to be detected when acting alone, the repulsion might be outweighed by the gravitational attraction between the spiral nebulæ which tends to hold them together; only if the mean density throughout the region surveyed is so low that the gravitational attraction is less than the cosmical repulsion, will the net effect be a scattering apart. There was therefore no certainty, but there was a reasonable hope of finding the repulsion effect.

By 1922 some forty radial velocities of spiral nebulæ had been determined, which showed that with three or four exceptions (easily accounted for) these objects had large velocities of recession such as would be produced by a repulsive force. Thus the existence of the cosmical-repulsion effect seemed highly probable. More precise confirmation was obtained in 1929, when Hubble introduced estimates of the distances of the nebulæ and was able to show that the speed of recession was proportional to the distance, as the theory required. The survey now covers more than 200 nebulæ and extends up to distances of the order 250 million light-years and receding velocities of 40,000 km. per sec. Besides the satisfaction of observing a phenomenon which theory had shown to be likely to occur, we obtain from these astronomical measurements a determination of the magnitude of the cosmical repulsion (previously unknown) and of the cosmical constant on which it depends. The fact that it is the only direct observational method of determining this fundamental constant of nature greatly enhances the importance for physics generally of the study of the distances and velocities of the extragalactic nebulæ.

The scattering apart of the galaxies may well rank as one of the classic examples of a phenomenon, rather surprising according to the ideas of the time, which was foreseen by theory and afterwards found in observation. This kind of success rightly gives increase of confidence in a theory. It is, of course, true that there have been cases in which an apparent confirmation has flattered to deceive; but if we are hesitating as to the lines on which further advances of science should be attempted we can scarcely do better than take agreement of this kind as a guide. Most of the frontiers of scientific theory are beset by difficulties and uncertainties which can only be met in a tentative way; in comparison the "expanding universe theory" is a straightforward advance which gives no particular occasion for doubt. It is therefore somewhat of an anomaly that cosmology should be so widely regarded as a precarious region of thought, where physicists are baffled for lack of a clear lead and are ready to encourage the wildest hypothesis as a way out of the difficulties. I will try in this article to lay some of the dust which is obscuring the present situation.

We may consider first the view, advocated especially by E. P.

Hubble, that the observed radial velocities are spurious, the red-shift of the spectral lines being due not to Doppler effect but to some other unassigned cause. The professional sceptic who, when the observations agree with theory, falls back on the suggestion that the effect is not a confirmation of the theory but is due to some cause at present unknown to science, requires no answer. But Hubble's objection is more specific ; he claims that there is observational evidence discordant with the view that the red-shift is a Doppler effect, but consistent with the view that it is a distance effect in a static universe. This evidence is derived from statistics of the magnitudes of faint nebulæ, the red-shift of the light being so large that it sensibly affects their luminosities. But I do not think that any real discordance has been established ; the agreement of observation with the relativity theory seems to be as close as could be expected on any reasonable estimate of the accidental and systematic errors of the investigation. So far as I have been able to ascertain, most of the expert observers are of this opinion. But it is not essential to press this criticism of the data. Hubble assumed a perfectly uniform large-scale distribution of the nebulæ ; primarily therefore a " discordance " would be, not a reason for throwing over the existing theories of physics, but an indication of the deviation of the distribution from uniformity. The indication is that the average density in a somewhat large region surrounding our own galaxy is 30 per cent. less than in the whole region accessible to observation. As already stated, I do not think that any weight can be attached to this determination, but it is not inherently improbable. To those familiar with the irregularity of distribution of the brighter nebulæ, the extent to which it is evened out in the large-scale distribution is rather surprising ; and the natural conclusion from Hubble's investigation is that the system of the galaxies is much more uniform than had previously been suspected.

It is no part of the relativistic theory of cosmogony to assume a uniform large-scale distribution of matter throughout the universe. As to that we are content to accept whatever data astronomical observation may provide. Naturally the case of uniform density is the first example to be worked out in detail—as in other subjects. If attention has mostly been confined to this, it is because we are still waiting for observation to suggest a type of non-uniformity which it would be of practical importance to examine. But, besides the " uniform spherical world," the growth of condensations and rarefactions, which has an important bearing on problems of evolution, has also been very extensively studied by a number of investigators.

If the uniform distribution continues indefinitely, the part of the universe included in our present astronomical survey bears to the whole universe about the same ratio that Ireland bears to the whole surface of the earth. It has been objected that it would be a rash generalisation for an Irishman to suppose that the rest of the earth is just like Ireland. Nevertheless it seems to me that the Irishman who has a correct impression of what the earth as a whole would be like if the rest of it were like Ireland is in a somewhat better position than one who has a false impression of what it would be like. We are not sure whether the system of galaxies comes to an end shortly beyond the limits of our present survey or whether it continues; and in preparation for further advances we have to consider both possibilities. In practice this means that we have to make an extensive study of the second alternative, since many of its consequences are not at all obvious without mathematical analysis; whereas in regard to the first alternative there is little to be said except that we recognise it as a possibility.

Another type of critic accepts the genuineness of the nebular velocities—that is to say he accepts the expansion of the system of the galaxies as a fact—but he attributes it to some other cause than the cosmical repulsion predicted by relativity theory. Prominent among these are the kinematical cosmologists led by E. A. Milne. Before dealing with kinematical cosmology it is desirable to make clear certain points which seem not to be generally appreciated.

It would be meaningless to speak of the expansion of the universe without a clear understanding as to the standard of length with which its dimensions are being compared. When we contemplate slow changes of material systems, of the dimensions of space, or even of the fundamental constants of nature, it is implied that we have a standard of length defined throughout all time. We do not look upon a particular bar of material, such as the Paris metre, as the ultimate standard for such purposes; the fact that we feel anxiety as to its permanence shows that we have in mind a more ideal standard with which it might be compared. This ideal is a physical structure not necessarily permanent but reproducible, *e.g.* a rod of some particular kind of crystal containing in its length a specified number of lattice intervals.

A general definition of the standard of length in physics is arrived at from the following considerations. The definitions of length and time-extension stand at the beginning of physics, and the other physical quantities presuppose that these have been defined. Thus when we are specifying the standard of length, the terms "length" and "time" and the usual derived physical quantities are not yet available. The quantitative part of the specification must therefore consist of pure numbers only. The method of specifying physical structure by pure numbers is developed in quantum theory—numbers of elementary particles whose mutual arrangement is defined by quantum numbers. The necessary standard of length is therefore a *quantum-specified* standard. All such standards are fundamentally equivalent; for the extension of a quantum-specified structure is determined by the quantum equations as a definite numerical multiple of the theoretical unit h/mc. Even if h, m, and c individually vary with time, two quantum-specified standards preserve a constant ratio to one another. A time-periodicity in a quantum-specified structure furnishes similarly the standard of time-extension.

The quantum-specified standard accepted in theoretical physics (relativity theory and quantum theory) is also recognised in practical metrology, *e.g.* the wave length of cadmium light or the grating-space of calcite. Acceptance by the practical physicists is important; because sooner or later theoretical conclusions must be submitted to practical test, and if the theorist and the experimenter are using "length" and "time," and the other physical quantities depending on them, with different meanings the test is vitiated. The quantum-specified standard is reproducible in the remotest nebula and at any time past or future, so that it suffices for the utmost demands of cosmogony. There is therefore no excuse for the cosmogonist to deviate from the usage in other branches of physics.

Certain difficulties arise in the *precise* definition of length, which have important applications. For example, Einstein's theory of the gravitational field depends on the fact that a short standard is precisely defined, but not a long standard. But in the present connection questions of precision are irrelevant, and it is amply sufficient to define length and time 10^9 years ago with an accuracy of, say, 1 per cent. Much of the recent confusion over cosmogony arises from the idea that the reckoning of time and space in the remote past is altogether arbitrary. We find, for example, the suggestion that the cramped time-scale now accepted might be extended by a logarithmic transformation of time-reckoning which would put back the "beginning" from $2 \cdot 10^9$ years ago to minus infinity. Such arguments were familiar in the days of Chaucer:

Ye conne by argumentes make a place
A myle brood of twenty foot of space.
Lat see now if this place may suffyse,
Or make it roum with speche, as is youre gyse.

Kinematical cosmology seems to me perverted from the start by this mistaken impression that the customary reckoning of length and time becomes ambiguous in applications on so vast a scale; so that we need to introduce a new system of metrical ideas designed specially for the cosmological problem. It contemplates physicists, who are not allowed to use quantum-specified standards, but contrive to set up (in very specialised conditions) a system of space and time reckoning by interchanging light signals and communicating to one another the results. But by developing the subject on this basis cosmogony is cut off from the rest of physics, whereas our aim should rather be unification. It is perhaps the intention of the kinematical cosmologists to remedy this later by transforming the rest of physics to the same basis. But the transformation can scarcely include the practical physicist, who will not easily be persuaded to give up the use of his standards when called upon to verify the results.

The actual results reached in Milne's kinematical cosmology are only surprising to those who have not realised that if you alter the meaning of words you can make any statement true. As Milne does not define length and time in the customary way, his conclusions require translating. A number of writers have furnished translations, and it appears that when translated there is little, if any, difference from the relativity theory of the expanding universe. Ostensibly there is one point of difference; the relativity formulæ give an accelerated increase of the radius of the universe, whereas Milne contemplates a universe whose radius increases uniformly. But the uniform rate refers to Milne's own time reckoning and is not necessarily inconsistent with an accelerated expansion as correctly defined.

If there is any real difference it is purely incidental. The relativity formulæ cover both uniform expansion and expansion with arbitrarily chosen acceleration. The criterion which decides against uniform expansion and fixes the precise law of acceleration is the actual rate of conversion of bound energy into radiant energy, or vice versa, occurring in the universe. In relativity theory, which does not separate cosmogony from the rest of physics, we freely employ our general knowledge that the rate of conversion is comparatively slow, and so select the case which corresponds approximately to the actual conditions of the universe. Milne, on the other hand, excluding all physical knowledge except that which is explicitly derived from his cosmological postulates, does not reach the point of discovering and applying this criterion, and selects the case (uniform expansion) which he finds simplest to work out.

According to a widespread impression, one of the most interesting tasks awaiting the 200-inch reflector now being constructed is to decide between two conflicting theories of cosmogony. I do not think that any question of that kind is pending. The new telescope will doubtless furnish very valuable data, which may or may not fulfil the anticipations of the present theory. It will be particularly important if the data go far enough to test the acceleration of expansion which relativity theory predicts; though I am afraid this can scarcely be expected, owing to the difficulty of separating an acceleration effect from the effect of a slight non-uniformity of density. But there exists no alternative prediction—except in so far as the opponents of a theory naturally predict its downfall. No theory *predicts* uniform expansion. One theoretical investigation *assumes* uniform expansion; but that is quite a different matter. The position is similar to that of testing experimentally a prediction of quantum theory as to the law of force between the atoms of a gas. It will be remembered that Maxwell assumed a law of force varying as the inverse fifth power, because it led to equations which were easily integrable. But we should scarcely describe the experiment as an attempt to decide between the quantum prediction and the Maxwellian assumption.

Other misunderstandings have arisen through not recognising the essential differences between pure mathematics, experimental science and observational science—a type of misunderstanding particularly liable to arise in a subject in which all three types of investigators have a legitimate share. We predict a repulsive force sensible at great distances, and expect therefore to find objects at great distances moving away. But there is no actual necessity that bodies should be found moving in the direction in which the field of force points. We might perhaps regard it as good fortune that, when Newton was contemplating the causes of motion of the moon, his attention was struck by falling apples rather than by a display of rockets. In experimental science we can to a large extent control the conditions; but in observational science we must simply recognise that we may be at the mercy of impish tricks of Nature to deceive us, and do our best under the circumstances. There are many possibilities, which cannot be strictly disproved, but which we may not unreasonably relegate to the category of "far-fetched."

Again there are some who demand in cosmological study a kind of security unknown in the rest of physics. It is made the occasion for a lecture on the fallibility of human knowledge, and the possibility that even the most deep-rooted principles accepted by physicists may be mistaken. I do not regard cosmology as one of the more precarious fields of advance; but it is not suggested that in the expanding universe and its implications we have found the solid rock which will endure when the present system of modern physics, comprised in wave mechanics and relativity theory, has crumbled to ruins. It has rather been our aim to unify cosmology with the rest of physics, so that it will stand or fall or evolve as part of it.

The observed expansion of the system of the galaxies gives a welcome confirmation of the existence of cosmical repulsion; but any attempt to reverse the argument and deduce the existence of cosmical repulsion solely from the astronomical observations would be entirely unconvincing. Many have expressed doubts as to the existence of cosmical repulsion; but such opinions are discounted by the fact that they are based on a survey of observational evidence which is admittedly insufficient to establish it, and not on an examination of the theoretical arguments which are much more convincing. It may therefore be of interest to give a brief indication of why it is found necessary to introduce a cosmical constant in general relativity theory.

The energy-density, momentum and stress of a physical system are grouped together in an expression $T_{\alpha\beta}$ which is called the "energy tensor." A crucial step in relativity theory is the expression of $T_{\alpha\beta}$ in terms of the "fundamental tensor" $g_{\alpha\beta}$ which specifies the metric of space-time. It is characteristic of energy and momentum that they satisfy a law of conservation; we have therefore to seek a tensor expression which is a function of $g_{\alpha\beta}$ (and its derivatives) and satisfies the conditions of conservation. These conditions are almost sufficient to determine a unique expression $K_{\alpha\beta}$, which was accordingly identified with $T_{\alpha\beta}$ in the earliest form of the theory. But it was later noticed that the fundamental tensor $g_{\alpha\beta}$ itself satisfies the law of conservation, so that we can add or subtract from $K_{\alpha\beta}$ an arbitrary multiple of $g_{\alpha\beta}$ without upsetting conservation. In effect then, knowing nothing about energy and momentum except that they are conserved, we deduce that the most general possible identification is $T_{\alpha\beta} = K_{\alpha\beta} - \lambda g_{\alpha\beta}$, where λ is an arbitrary constant or zero. The constant introduced in this way is the cosmical constant; and, in deriving the equations of motion corresponding to this identification of the energy tensor, we obtain an additional term involving λ which can be interpreted as a repulsive force, namely cosmical repulsion.

The constant λ may be regarded as fixing a zero from which energy, momentum and stress are reckoned. The "reckoned" energy $T_{\alpha\beta}$ is then the difference between the actual "absolute" energy $K_{\alpha\beta}$ and the "absolute" energy in a standard zero condition. If we are contemplating a limited region of space it is natural to take emptiness as the standard zero condition—the energy in a region is that which we should have to take away in order to leave it a complete vacuum. But a standard zero condition *for the whole universe* cannot be defined in that way. A region can be emptied by transferring the matter elsewhere; but to empty the universe would involve, not transference, but annihilation of matter and energy. It would be absurd to define our reckoning of energy by reference to a fictitious process which conflicts with the most important property of energy, namely its conservation. Without entering further into technicalities, we may be content to notice this very important point which arises in seeking a self-consistent definition of the zero of energy reckoning. The requirement that the zero condition shall correspond to a possible rearrangement of the matter of the universe decides that the cosmical constant λ shall not be zero.

Although relativity theory made no definite prediction of the value of the cosmical constant, it was noticed that the observed recession of the galaxies was of an order of magnitude which might be expected on general grounds. If the recession were to continue at the rate at present observed the distances of the galaxies would be doubled in 1300 million years; or, following the more usual mathematical mode of expression, they would be increased in the ratio e in 2×10^9 years (e being the Napierian base). We call the time in which the dimensions of the system of the galaxies become multiplied in the ratio e the *time of relaxation* of the system. Let us express this time in terms of the natural unit of time furnished by atomic physics. The natural unit of length is the quantity e^2/mc^2, at one time thought to represent the radius of an electron but now more usually associated with the range of nuclear forces; this is about 3×10^{-13} cm. Light takes about 10^{-23} sec. to travel this distance, and this furnishes us with an atomic unit of time. The time of relaxation (2×10^9 years) is roughly 10^{39} atomic units. We recognise 10^{39} as a ratio already familiar to us in another case in which there is an outstanding difference of order of magnitude between two natural units. If we compare the strong electrostatic attraction between a proton and electron with the very weak gravitational attraction between them, we find that the ratio is also roughly 10^{39}. It can scarcely be without significance that the two forces—gravitation and cosmical repulsion—which by their order of magnitude stand altogether apart from the forces treated in atomic physics, both introduce the scale-ratio 10^{39}. This coincidence strengthens our belief that the observed recession of the nebulæ is not to be explained as an accident of time and circumstance, but is a simple fundamental phenomenon.

The foregoing comparison of scale, in various equivalent forms, has been familiar for many years. I have stated it here in a particularly simple form which is due to P. A. M. Dirac. Dirac, however, drew from it very different conclusions, because he took 2×10^9 years to be the age of the universe, apparently overlooking its more direct significance as the time of relaxation. There is no ground for adopting 2×10^9 years as the age of the universe—which indeed would scarcely allow time for the ages of terrestrial rocks, which appear to be well established. On the other hand, it seems clear that the age of the universe (reckoned from a time when the evolution of the primordial material had begun to make sensible progress) cannot be a large multiple of the time of relaxation. Consequently, in arguments as to order of magnitude, the age and the time of relaxation are more or less interchangeable at the present epoch. But if any significance is to be attached to the coincidence of the two scale-ratios, it must apply at all epochs; and an important difference arises because the age necessarily varies with time, whereas the relaxation-time is, or at least may be, a permanent constant of the system. (Actually, according to relativity theory, the relaxation-time is not strictly constant but approaches asymptotically a constant limiting value; it appears, however, that the expansion is already so far advanced that we may take the present determination of the relaxation-time to refer to the constant limiting value.) Since Dirac associated the ratio of electrostatic and gravitational forces with the age of the universe, instead of with the constant limiting relaxation-time, he was obliged to suppose that it varied with time. This led him to an unnecessarily complicated and fantastic theory.

Using the value of the cosmical constant obtained from the observed recession of the spiral nebulæ, we can derive the total mass of the matter in the universe by a well-known formula given by Einstein. Converting the mass into atomic units we obtain the cosmical number N, which represents the number of elementary particles (protons and electrons) in the universe. The cosmical number is about 3×10^{79}, so that it is of the order of magnitude of the square of the fundamental ratio 10^{39}. In other words, the electrical scale of force is $\sqrt{N}$ times the gravitational scale, and the cosmical scale of time is $\sqrt{N}$ times the atomic scale. Or, if we consider instead the equivalent ratio of the scales of length, the equilibrium radius of the universe (Einstein radius) is about $\sqrt{N}$ times the so-called electron-radius e^2/mc^2.

The last-mentioned comparison gives us a clue to the way in which these two scales of very different order of magnitude arise. If we have N particles each with an independent initial probability distribution spread over a region of linear dimensions (standard deviation) R, their centroid will have an initial probability distribution spread over a region of dimensions $R/\sqrt{N}$. Now it must be remembered that when we study a system of particles (*e.g.* an atom or nucleus) we are concerned primarily with their distribution relative to the centroid of the system. The fact that the centroid itself must have a probability distribution, and is not a definite point in space, is commonly ignored; it is treated just as though it were a fixed geometrical origin. There are, however, various empirical terms and constants in the quantum equations which current theory has not traced to their source; and it must be presumed that the uncertainty of position of the centroid used as origin, though not introduced explicitly, is actually allowed for in these terms. But it can only be allowed for in this way if it is a standard quantity—the same in all problems to which the recognised quantum equations apply. Thus from the point of view of quantum theory, it is the uncertainty of position of the physical origin from which the positions of the individual particles are measured which is the fundamental constant of nature determining directly or indirectly the scale of atomic systems; this constant, which we will call ε, likewise determines the spread R of the initial probability distribution of the individual particles by the foregoing condition that $R/\sqrt{N}$ is of order of magnitude ε. It is only when the system comprises all the particles in the universe that we have a direct interest in evaluating R. It represents, as we have said, the spread of the initial probability distribution of a particle; by *initial* distribution we mean the probability distribution before any observational information is supplied. How closely do we know the position of a particle when we have no idea where it is? The question sounds nonsensical; but it has a definite answer, namely R. Since the particle may be anywhere, the uncertainty R of our knowledge of its position corresponds to the dimensions of "anywhere"—anywhere that it is possible for a particle to be—*i.e.* of space. It is, I think, quite definitely deducible in this way that space must have a radius of curvature of order $\sqrt{N}$ times the atomic unit e^2/mc^2. As we have seen, the astronomical results are in agreement with this prediction.

This study of the relations of the fundamental constants of nature can be carried much further by a precise investigation in which the elementary principles of quantum theory and relativity theory are combined. In particular a purely theoretical argument appears to indicate that the cosmical number is precisely $2 \times 136 \times 2^{256}$, which agrees satisfactorily with the observational value. Although it is usually described as "the number of particles in the universe," we should bear in mind that particles as classically conceived are not recognised in modern physics, and N really represents the greatest possible number of independent wave systems of the particular type associated with protons and electrons. Provided we grasp correctly the principles which determine this analysis into waves, it is not unduly sanguine to believe that we can compute their total number.

I forbear to enter further into these developments, since the main purpose of this article has been to deal with the more elementary controversial issues. But to have omitted all reference to the quantitative developments, even though they may still be in a rudimentary form, would have given a misleading idea of the present situation. The relativity theory of the expanding universe is not a conclusion for which we seek to secure general acceptance as the termination of a successful research. What is much more important is that it is the beginning of general advance in theoretical physics, which it is clearly in our power to continue.

SUBMARINE GEOLOGY

By E. C. BULLARD, Ph.D.

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The geology of the continents may be studied quite simply. The facts are there for all to see, no elaborate technique is required and no apparatus more expensive than a hammer. It is therefore not surprising that a vast amount of information has accumulated in the century that has elapsed, since it was realised that the history of the earth could be read in the nature and arrangement of its rocks.

The picture given by these investigations is of great complexity. The events at a single place usually show numerous alterations of marine and terrestrial conditions, periods of compression and periods of volcanicity without any obvious regularities. Numerous attempts have been made to suggest principles such as Continental Drift guiding the development of the earth, but on the whole they have not gained general support, as there were really no cogent reasons for believing them to be of general application. Such theories were necessarily based on a study of the rocks near the surface of that part of the earth that is not covered by water. The most promising way to extend geology would therefore appear to be to study the interior of the earth and that part that is under the oceans. Seismology has proved the key to the first part of this programme and has given a detailed numerical knowledge of certain properties of the rocks all the way to the centre of the earth. The intensive study of the second part is a development of the last few years, and is still in its infancy.

The nature of the problems may be made clearer by considering what might be expected of the ocean basins on various theories without necessarily taking any of them very seriously. Great areas of marine sediments are found on the present continents; it is therefore certain that they were once under the sea. It is thus probable that extensive areas of what is now sea were once land, so that part of submarine geology may be expected to be very like continental geology. The sediments on the land are, however, almost without exception, shallow water deposits, having been deposited in water from a few inches to a few hundred feet deep. The rarity of typical abyssal deposits on the continents suggests that either the ocean basins are permanent, or else that the development can only proceed in one direction, that is, that continents may founder, but that they cannot rise from the ocean. On the view that the oceans are permanent, submarine topographic features should show no signs of erosion, and if there are fold mountains they should show smooth rounded curves broken perhaps by abrupt fault scarps, but not dissected by the network of valleys that is typical of mountain scenery on land. On the second view the ocean bottom would be simply a drowned continent. Again, if Wegener's view that the Atlantic Ocean has been formed by the drift of North America away from Europe is correct, the floor of the Atlantic Ocean should consist of basic igneous rocks with perhaps some blocks of continental rocks inserted in them.

The problem is therefore to study the form and nature of the ocean floor and, if possible, its structure. Owing to the covering of water these studies cannot be made by the methods that have been used on the continents, and the information must be collected by lowering instruments on to the sea floor or by carrying them in ships. The first requirement of submarine geology is therefore to develop instruments and methods for collecting information about the submerged rocks.

SUBMARINE TOPOGRAPHY

The determination of depths by running out a wire carrying a weight till it reaches the bottom is a slow and inaccurate process in deep water, and little progress could be made in detailed submarine mapping till the development of the echo sounder. In the form of this instrument used for deep-sea sounding a pulse of sound is produced by a hammer striking an anvil. The sound travels to the bottom of the ocean and is reflected back to the surface, where it is received on a hydrophone and the depth is calculated from the time taken for the up and down journey. In depths less than two or three hundred fathoms the sound pulses used are usually of supersonic frequency and are produced by magnetostriction or by the piezo-electric effect. Such instruments are often arranged to record the depth continuously as the ship travels along. In one respect the measurement of depth at sea is simpler than the measurement of height on land; for the latter requires a line of levels extending all the way to the sea, whilst the depth of the sea can be determined directly by measurement at a single point. The first result of the great increase in the number of deep-sea soundings has been to emphasise the great irregularities of much of the ocean floor. It was formerly thought that, on the whole, the floor of the oceans consisted of great plains with very little relief, but the more numerous soundings now available show many parts to have differences of level comparable to those in the Himalayas. This is illustrated by Fig. 1, which shows the soundings in a small portion of the Atlantic 500 miles north-east of the Azores. It is clear that until many more soundings have been obtained it is impossible to say whether the topography consists of submarine volcanoes, of undenuded fold mountains, or of fault scarps. The detailed mapping of such a feature would involve not only the taking of soundings but the determination of their relative positions. For this purpose the ordinary methods of astronomical position finding and dead reckoning would be insufficiently precise, and it would be necessary to moor buoys to mark fixed points to which the whole work could be tied. These complications have so far prevented the detailed survey of any portion of the ocean away from the continental shelf.

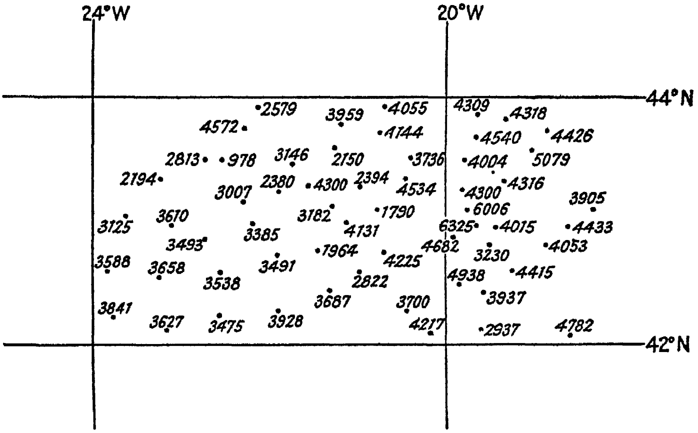

FIG. 1.—Soundings, in metres, showing a range of over 5000 m. in about 100 miles.

Near the continents detailed survey is considerably easier as buoys can easily be moored. The positions of the buoys are determined by starting from a point in sight of land and running piano wire over the stern of the ship. As the wire runs out it turns a measuring wheel which determines the length paid out. In this way the distance between points can be found with an accuracy of a few tenths of one per cent. The process is similar to a traverse survey on land and was developed by the Hydrographic Department of the British Navy. The positions of the ship relative to the buoys may be determined either by taking bearings on them or by measuring their distances. For the latter purpose the buoy contains a hydrophone and a wireless transmitter. If a small bomb be exploded near the ship the sound travels to the buoy and causes the hydrophone to actuate the wireless transmitter. From the time interval between the explosion of the bomb and the receipt of the wireless signal the distance of the ship from the buoy may be calculated. This method of position finding has been developed by the U.S. Coast and Geodetic Survey and used by them to construct remarkable charts of the seas off the coasts of the U.S.A. As in most other parts of the world, there is a Continental Shelf sloping very gently to a depth of about 100 fathoms. The depth then increases in a few miles to 1000 fathoms, and then more gently to the 3000 or 4000 fathoms characteristic of the deep oceans. Detailed mapping of the relatively steep slope from 100 to 1000 fathoms shows surprisingly rough and complicated features. Examples on a small scale are shown in Figs. 2 and 3. Besides the minor corrugations, there is also revealed a series of valleys cutting into the slope but not penetrating far beyond the 100-fathom line. Similar features have been found in many parts of the world and they must be regarded as normal features of the edges of the continents. The existence of these valleys has been known for forty years, but it is only in the last few years that detailed charts such as that shown in Fig. 3 have been available. These show a gently sloping flat bottom 1 to 5 miles wide, bounded by steep sides. The steepness of the sides of these valleys has been much exaggerated by some writers, who have compared them with canyons on land. Such a comparison is misleading, and a belief in it has made them appear more startling than they really are. One school maintains that the soundings show a pattern of converging valleys similar to those produced by streams, and infers that the valleys were, in fact, the result of subaerial denudation at a time when the land stood higher or the sea lower. Such views involve movements of many thousands of feet, and it is difficult to believe that these movements would not have left their mark on the physiography of the continents. Others believe that the valleys have been cut by submarine currents heavily charged with sediments or by landslips precipitated by submarine springs.

THE NATURE OF THE OCEAN FLOOR

Samples of the top few inches of the sediments covering the floor of the oceans may easily be obtained. These show, as might be expected, that far from land fine sediments predominate. The deposits often contain manganese nodules, and show other curious features such as a radium content many times greater than that

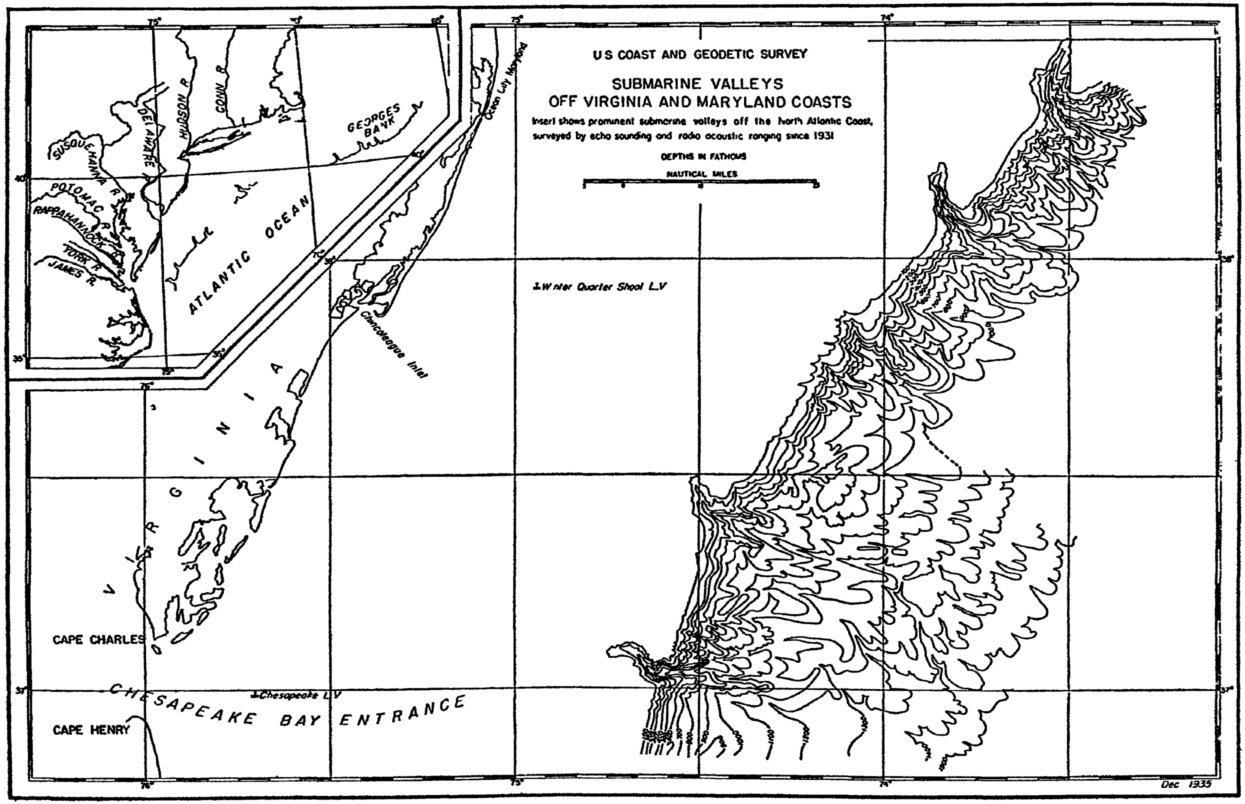

FIG. 2.—General configuration off Virginia-Maryland coasts.

(By permission of the U.S. Coast and Geodetic Survey and of the American Philosophical Society.)

shown by shallow water sediments. These peculiarities are probably to be explained by chemical precipitation from water of varying oxygen content, but much work remains to be done on the subject.

By rapidly lowering a weighted tube to the bottom of the ocean

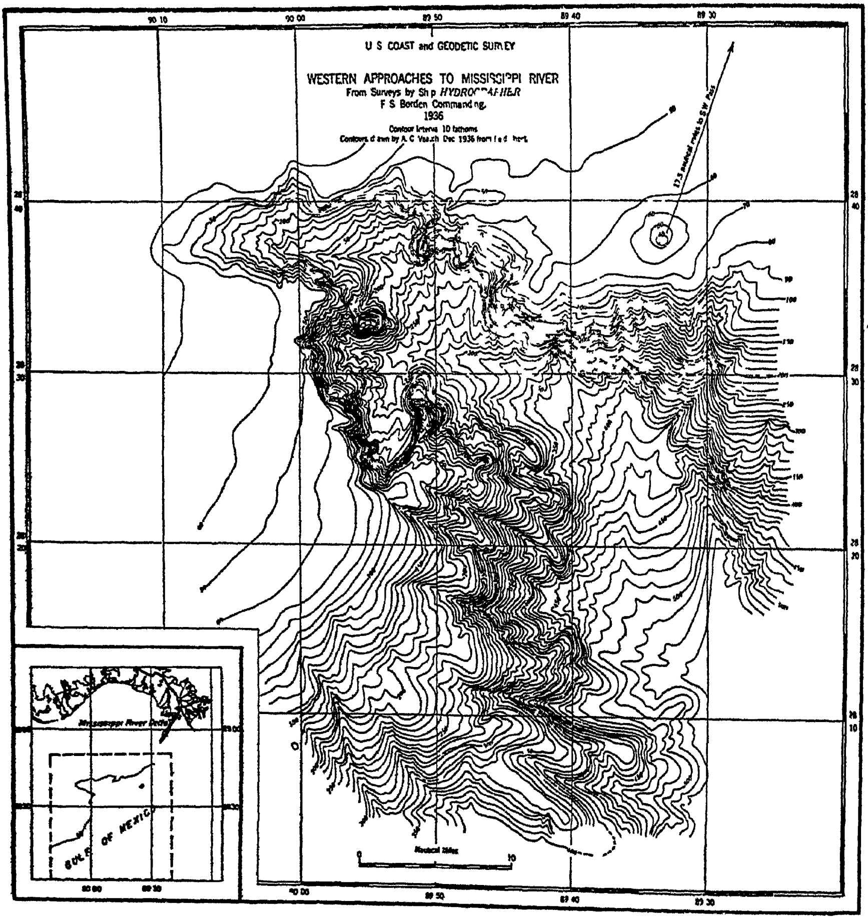
U S COAST and GEODETIC SURVEY
WESTERN APPROACHES TO MISSISSIPPI RIVER
From Surveys by Ship HYDROGRAPHER
F S Borden Commanding
1936
Contour Interval 10 fathoms
Contours drawn by A C Veatch Dec 1936 from field sheet
90 10
90 00
89 50
89 40
89 30
28 40
28 30
28 20
28 10
17.5 nautical miles to S W Pass
Nautical Miles
0
10
Mississippi River Delta
GULF OF MEXICO

FIG. 3.—A submarine region off the Mississippi River Delta.

(By permission of the U.S. Coast and Geodetic Survey and of the American Philosophical Society.)

a core 5 or 6 feet long may be obtained. To obtain longer cores C. S. Piggot has designed a gun which shoots a tube into the sea bottom; with this, cores up to 10 feet long have been obtained. These cores were taken in the North Atlantic, and show a series of glacial and interglacial deposits presumably dropped from floating ice. When these have been fully worked out they should give a more complete picture of quaternary climatic changes than can be obtained from terrestrial deposits, where each succeeding advance of the ice sheet disturbs the earlier deposits. Above the glacial deposits is about a foot of ooze which has clearly been deposited since the close of the Ice Age. The rate of deposition indicated (1 foot in 20,000 years) is surprisingly high, and if it is representative of average conditions through long stretches of geological time would lead to the accumulation of 10,000 feet of post-Palæozoic sediments. Such great thicknesses of deep-sea sediments are quite contrary to the commonly accepted view that the ocean floor has only a thin veneer of sediments, and it would be extremely difficult to find an origin for them, without postulating an altogether unreasonable amount of denudation on the continents.

The technique of obtaining cores can doubtless be improved, but it is unlikely that it will ever be possible to obtain specimens of more than the top 20 or 30 feet by such methods. To go deeper would require the construction of some kind of submarine drilling rig which would be difficult and expensive. Further progress, therefore, depends on indirect methods. Of these the seismic method is the most promising. Charges of explosive are exploded on the sea floor, and the elastic waves produced are recorded by seismographs also on the bottom and connected to a recording apparatus on a ship. This method was used by Ewing to find the thickness of sediments on the Continental Shelf off the east coast of the U.S.A., and has more recently been used for a similar purpose off the west coast of Europe. Attempts to use it in deep water have not so far been successful, but the difficulties are probably not insuperable. The method that has been used up to 100 fathoms is illustrated in Fig. 4. The ship carrying the instruments anchors and lowers a geophone on to the sea floor. A geophone is an instrument for converting movements of the ground into electrical currents and usually consists of a magnet suspended on a spring near a coil of wire in which currents are induced by the relative movement of the coil and the magnet. The geophone is connected to an amplifier and string galvanometer in the ship. A second ship, which need not anchor, lowers a charge on to the sea floor and detonates it electrically. The explosion breaks a wire wrapped round the charge. This wire is connected to a wireless transmitter which signals the instant of explosion to the first ship, where it is recorded alongside the signals from the geophone. The wave travelling through the water is also recorded by a hydrophone, and the time taken by this wave enables the distance to be found. In recent work south of Ireland, charges of up to 160 pounds of blasting gelatine at distances up to 10 miles were employed. In 1938 they instruments were on the surveying ship H.M.S. *Jason*, and the explosive was lowered from the ship's launch. In 1939 a pair of Brixham trawlers, each of 60 tons displacement, were used.

When the explosion takes place, elastic waves spread in all directions through the water and underlying rocks. If the geophone is within a few thousand feet of the explosion the wave that has travelled straight through the sediments (AB in Fig. 4) is the first to arrive, followed after an interval of a few tenths of a second by the wave through the water. After this comes a complicated series of waves, much of which consists of energy reflected from various horizons in the sediments. These reflections are too numerous and complicated to be readily interpreted and little use has been made of them at sea. If the distance from the explosion to the geophone is larger, a wave that has passed down through the sediments and travelled along in the hard underlying rock (AXYB in Fig. 4) may arrive before the direct wave through the sediments. If this wave is observed at a number of distances the depth of the bottom of the sediments and the velocity in the underlying rocks may be found.

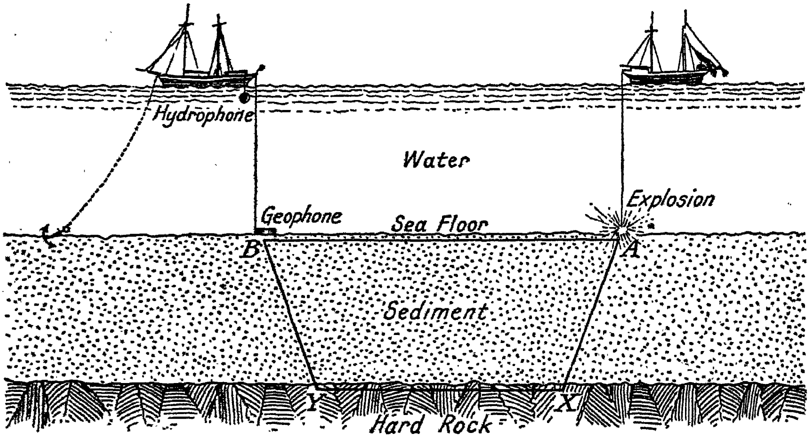

FIG. 4.

In this way it has been shown that the sediments thicken from the land towards the ocean. On a line running west-south-west from the Lizard, for example, there are no sediments near the shore, but a thickness of nearly 8000 feet was reached at the 100-fathom line 180 miles to the west. This wedge of sediment bordering the continent is presumably produced by denudation of the land and probably took ten to a hundred million years to accumulate. There is reason to suppose that no deposition occurs on the Continental Shelf, and that the shelf grows seawards by the continual transport of sediment from the current-swept area shallower than 100 fathoms to the quiet depths beyond it. This kind of sedimentation is somewhat different from the processes that are usually supposed to have produced the great masses of sedimentary rocks found on land.

The study of real earthquakes has contributed less information about the geology of the oceans than might have been expected. If an earthquake occurs in Japan, and the compressional and distortional waves (P and S) from it are observed in Europe and in America, a comparison of the travel times shows the elastic constants and densities over the two paths to be equal within a few tenths of one per cent. The greater part of the paths for these long distances, however, lie far in the interior of the earth, and the similarity of the time-distance relations merely shows that the difference between the continents and the oceans is confined to the outer part of the earth. To get information about the differences between continental and oceanic structure it would be necessary to have at least six seismographs within 100 miles of an oceanic earthquake, a state of affairs which cannot be achieved with the present distribution of stations. The reason for requiring so many near stations is that the times of arrival at any three stations can be fitted exactly by a suitable choice of epicentre and time of occurrence; it is only when there are more than three stations that any information is obtained about travel times.

Although P and S can at present give no information about the differences between the continents and the oceans, the propagation of the surface waves from earthquakes can. These waves are similar to the waves on the surface of the ocean in that the disturbance is confined to within a few wave-lengths of the surface; their velocity, however, is controlled by the elastic constants of the rocks and not by gravity. Since these waves are propagated round the surface of the earth their velocity and dispersion give information about the surface layers of the earth. Stoneley and Gutenburg have shown in this way that the Pacific Ocean differs markedly from the continents, and that the Atlantic has intermediate properties. The petrology of rocks dredged from the ocean bottom and of those occurring on oceanic islands suggests that the difference is probably the absence of granite under the oceans. A more detailed study of the surface waves is very desirable. The method is the only one that gives information about the submarine geology without the necessity of going to sea.

GRAVITY

The acceleration due to gravity varies over the earth's surface by about half of one per cent. The greater part of this variation is due to the ellipticity of the earth and to the change in centrifugal force on going from the equator to the pole. The remaining variation, which is the part of geological interest, is only about one in ten thousand. Thus if measurements of gravity are to be useful they must be made with an accuracy of a few parts in a million. The motions of an ordinary ship produce accelerations which frequently reach 10 per cent. of that due to gravity. The problem of measuring gravity in the presence of such accelerations is probably insoluble; but fortunately the disturbances may be much reduced by working in a submerged submarine. The effects of the remaining accelerations may then be removed by a method devised by Vening Meinesz. This depends on the principle that the relative motion of a pair of pendulums is, to a first approximation, unaffected by horizontal accelerations. Instead of observing the period of a single pendulum, which is the standard method of measuring gravity on land, Meinesz observes the period of a "fictitious pendulum" formed by the difference in the motions of two real pendulums. The effect of the vertical accelerations is eliminated to a first approximation if the pendulums are swung for a time long compared to the period of the waves. In this way the greater part of the small disturbances remaining in a submerged submarine is eliminated. The remaining effect may be allowed for if the roll, pitch and yaw, and the longitudinal transverse and vertical motions of the submarine are measured or are known to be negligible. In all, about a thousand measurements of gravity have been made by this method.

It might be expected that gravity would be less over the oceans than over the continents, as sea water is lighter than rock. The measurements show, however, that this is not so; the difference being, if anything, in the opposite direction. This means that there is, on the average, nearly the same mass under unit area of the surface of the sea as there is under unit area of the continents. That is, the deficiency of density in the sea water is compensated by the rocks under the ocean floor being denser than those under the continents. This is consistent with the suggestion made above that the continents are underlain by a considerable thickness of granite, whilst under the oceans this is replaced by denser basic rocks. If this approximate uniformity of load were not found large stresses would be necessary, extending to a depth comparable with the width of the oceans. Even the small but widespread differences that do exist, such as the excess of 20 parts in a million over the North Atlantic, imply either that the earth possesses a considerable strength even at depths of several thousand kilometres or else that the interior is not in static equilibrium, and is subject to convection currents which maintain density differences in excess of those required for equilibrium. An apparent difference between gravity on the oceans and on the continents might be produced by some unsuspected experimental cause connected with the difference in the methods used on land and on sea. It is therefore desirable that some measurements be made by an entirely independent method. The lowering of a sensitive spring balance to the bottom of the sea in a pressure-tight container is a method the possibilities of which should be examined.

Over the edge of the Continental Shelf Meinesz has found gravity to be deficient by about 40 parts in a million. This deficiency may well be caused by the wedge of sediments found in the seismic investigations. H.M.S. *Narwhal* was to have investigated this point in September 1938, but the work was stopped by the mobilisation of the fleet caused by the Czechoslovakian crisis.

Perhaps the most remarkable result of the gravity measurements that have been made at sea is the discovery of strips of deficient gravity running parallel to the chains of islands in the East and West Indies. The deficiency reaches 200 parts in a million and is too large to be due to superficial irregularities in the density of the rocks. It is probable, as Meinesz suggested, that the whole crust of the earth has been folded into the underlying denser rocks and that the deficiency of gravity is due to the lesser attraction of this mass of relativity light rocks. The presence of such a mass of light rock necessitates stress differences in the crust. The existence of these is confirmed by the occurrence of earthquakes and volcanoes along the line of islands. Visser has pointed out that in the East Indies the epicentres of these earthquakes, many of which have foci hundreds of kilometres deep, show a tendency to lie on planes sloping downwards under the continent. This suggests that they may mark out a thrust plane, motion along which is forcing down the fold postulated by Meinesz.

It has been suggested that these strips of deficient gravity represent the first stage in a mountain building process. The idea is in some ways attractive as the downfolded strip could provide a long narrow basin in which a great thickness of shallow water sediments could be deposited. As the downfolding progressed these sediments would be crumpled and metamorphosed. Later when the horizontal stresses that held it down were relieved, the whole body of sediments together with the downfolded root could rise, producing a mountain system showing all the features of those found on the continents. The principal objection to this view is that the existing strips of deficient gravity are not, in fact, areas of rapid shallow water sedimentation ; on the contrary, they are the sites of the great deeps in which sedimentation is probably exceptionally slow. This may be an accident of present-day geography, or it may mean that another origin must be sought for mountain ranges.

CONCLUSION

From what has been said it will be clear that very little is at present known about submarine geology, A number of methods of obtaining information has been devised, but what has been done cannot be regarded as more than an indication of what lines are most profitable to pursue in the future. Perhaps the most valuable investigations that could be undertaken at present are the detailed survey with the echo sounder of a small area in mid-ocean and the devising of a practical method for deep-sea seismic work. The difficulties lie in organisation and finance rather than in technique. The work holds out no prospect of a direct economic return and requires ships and crews for them. Progress is therefore dependent on making the best use of opportunities offered by navies and yachtsmen and obtaining what results can be got with limited facilities, rather than laying elaborate plans that require several liners and a cable ship for their fulfilment.

THE SUBMICROSCOPIC STRUCTURE OF CELL WALLS[1]

By A. FREY-WYSSLING

Professor of Plant Physiology in the Swiss Federal Institute of Technology, Zürich

Properties of Cell Walls

We shall try to derive the submicroscopic structure of the cell walls from their physical properties. The most striking contrast with the living protoplasm is their extraordinary *anisotropy* ; *i.e.* the properties of the cell wall depend on the direction along which they are examined. The anisotropy is most pronounced in the bast fibres, which we shall choose as an example. If carefully bleached, such fibres represent the purest cellulose material we know, so that we have to do with a fairly well-defined chemical substance.

If we allow a dried fibre to *swell* in water, it becomes 10–20 per cent. broader, but its length will hardly change. A sphere cut out of the dry cell wall will show an ellipsoidal shape after the intake of water, whereas a similar sphere of cytoplasm would swell or shrink in all directions alike. The anisotropy of swelling is the reason why twined cords or ropes shrink when they are brought into contact with water : the circumference of the rope increases, and consequently the pitch of the individual strands becomes less steep.

Even more extraordinary is the anisotropy of tensile strength. In the direction of the fibre axis the tensile strength of flax attains 110 kg. square mm. ; this is about two-thirds that of the best steel ! [2] Perpendicularly to the fibre axis, however, the tensile strength is very small, so that fibres can be separated into *fibrils* without any special effort. These may be easily split further and further down to the limit of microscopic visibility. The possibility of fibrillation is a very characteristic feature of natural fibres.

A third group of anisotropic properties includes the *optical behaviour* of cell walls. In general, stained fibres do not absorb the light along every direction in the same way. If they are observed under the polarising microscope, their colour is greatly different according as the fibres are arranged parallel or perpendicular to the vibration plane of the light: they show double absorption or *dichroism*. Fibres stained with congo red or iodine appear red or black, when viewed in one direction, and colourless in the other.

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[1] Lecture delivered on March 9, 1939, at University College, University of London.

Better known than the anisotropy of absorption is the anisotropy of *refraction*. Ever since polarised light has been used in microscopy, the double refraction of cell walls has been noticed and has surprised the botanists. From this fact, Nägeli concluded that the cell walls must have a crystalline structure, and modern X-ray investigation has proved this hypothesis to be correct.

Besides the anisotropic properties, there are other qualities which indicate that the cell walls are *porous*. The density of cellulose is 1·55 or even higher, but that of the fibre only 1·1 to 1·3 [1]. Consequently there is a discrepancy of density of about 20 per cent. The double refraction of cellulose, *i.e.* the difference of its refractive indices, is 0·068 [2], and that of the fibre calculated from the thickness of its walls 0·061 [3]. Thus we find again a discrepancy of double refraction of about 11 per cent. To these features we have to add the *holopermeability* for salts and sugars, and even the *penetrability* for colloidal dye-stuffs like congo red. Further, we can transform fibres of cellulose into fibres of cellulose esters such as nitrocellulose or cellulose acetate, without dissolving them or changing their form. Such transformations are known as *permutoid or topochemic reactions*. The old statement of chemistry—corpora non agunt nisi fluida—does not therefore hold for our unusual object. All these facts point to an invisible porous structure of the cell wall, and other properties, such as flexibility, cleavage, etc., support this point of view.

Summarising we may state that *anisotropy* and submicroscopic *porosity* are the outstanding features of the cell wall.

METHODS OF INVESTIGATION

The anisotropy can be examined and measured by extremely accurate methods, and this is why we know much more about the submicroscopic structure of cell walls than of cytoplasm, which shows much less pronounced anisotropic effects.

Ambronn [4], who investigated the optical anisotropy of fibrous substances, made a very important discovery. The *double refraction* of such materials does not possess a definite value like that of crystals, but it depends on the liquid in which the fibre is immersed. This peculiar behaviour is a consequence of the fact that the fibre walls are porous; the liquid penetrates and forms, together with the fibre substance, a mixed system. Wiener [5] has given a complete theory of such mixed systems, which he calls " mixed bodies." He has shown that parallel rodlets (Fig. 1A) with a diameter smaller than the wave-length of visible light, cause a double refraction if the refractive index of the penetrating liquid differs from that of the rodlets. The amount of this so-called *rodlet-double refraction* is represented by a hyperbolic curve (Fig. 1B). When there is no difference between the refractive indices of the two components, the mixed body becomes optically homogeneous and the double refraction disappears. By this method it has been shown by imbibition experiments that the cell walls must be mixed bodies with *rodlets* as structural elements. In the case of cellulose fibres circumstances are complicated by the fact that the discovered rodlets have an *intrinsic double refraction* like crystals. As the rodlet double refraction is very small as compared with their intrinsic optical anisotropy, this most important phenomenon was overlooked until 1910, when

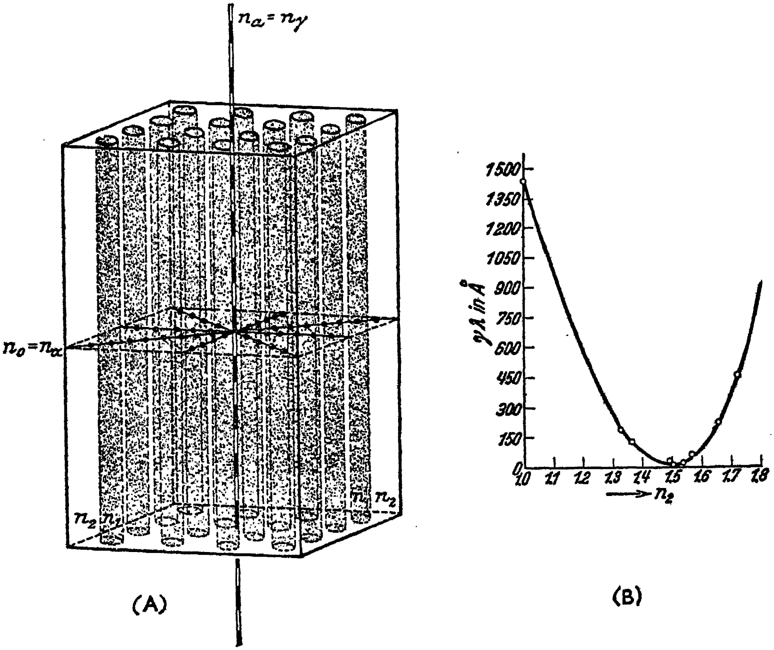


FIG. 1.

(A).—Rodlet mixed system.

n_α = extraordinary, n_o = ordinary refractive index of the system $n_\gamma > n_\alpha$.
n_1 = refractive index of the rodlets, n_2 of the imbibition medium.

(B).—Rodlet double refraction curve of incinerated epidermal hairs from barley awns.

λ = wave length of light, γ = retardation which is proportional to the double refraction.

Ambronn [4] saw its importance and Wiener [5] treated the question theoretically.

Later on the *X-ray analysis* of fibres [6, 8] confirmed the result of these optical studies by proving that the fibres must be built up of crystalline rodlets of cellulose.

An X-ray pattern of cellulose fibres furnishes four different sets of data concerning their crystalline structure.

(1) The arrangement of the diffraction maxima reveals the arrangement of the submicroscopic rodlets. If they are all parallel to the fibre axis, a so-called 4-point or *fibre pattern* appears. If they follow a spiral arrangement, as in cotton, X-ray analysis yields a pattern with sickle-shaped spots, and if there is random distribution, the pattern consists of uniform rings.

(2) The distance between the diffraction spots allows us to calculate the distance between the atoms in the crystalline lattice of cellulose. Investigations of this type yielded the important information that the intervals in the lattice spacing in the direction of the fibre axis correspond to the length of two glucose residues, and it was apparent in this way that these chains are arranged parallel to the fibre axis [7, 8].

(3) The intensity of the diffraction spots permits us to determine which plane of the crystalline lattice corresponds to the plane of the rings. The more atoms lying in a reflecting layer, the darker appears the corresponding diffraction spot on the X-ray pattern. As the plane of the glucose rings contains a good many of the atoms in question, it must furnish the darkest spots.

(4) The width of the diffraction spots is dependent on the number of atom layers by which they are reflected. A large number of such layers in a crystalline lattice produces narrow spots, and a small number broad ones. Consequently there is an inverse relation between the size of the submicroscopic crystallites and the width of their diffraction spots, which allows the calculation of the dimensions of the invisible colloidal particles [9, 10]. This calculation has been applied to cellulose fibre-patterns by Hengstenberg and Mark [11]. As the atom layers running parallel to the fibre axis produce broader diffraction spots than the layers running perpendicular to this axis, the invisible crystalline particles of cellulose must be *rodlike*; and it has been found that the rodlets are about 60 A broad, and at least 600 A long. Thus both the polarising method *and* the X-ray method furnish the same result, and establish, definitely, that the fibre walls are *rodlet mixed systems.*

Micellar Theory

If all the available X-ray data are put together the following picture of the fibre structure may be developed: there must be crystalline rodlets, consisting of bundles of cellulose chains and orientated parallel to the fibre axis. The details may be seen in a scheme by Seifriz [12], which astonishes by its brick-like structure (Fig. 2A). Still more amazing is the fact that this model of the submicroscopic structure of the cell wall is identical with the conception invented by Nägeli [13] some eighty years ago (1858). In his sketches we find isolated crystalline particles which he called *micelles* (from Latin: *mica* = crumb; this term has of course nothing to do with cellula, the origin of the term cell). They are embedded in an intermicellar substance, responsible for such phenomena as swelling, penetrability, permeability, etc. (Fig. 2B). The micellar theory of Nägeli was rejected by his contemporaries. It has only been supported by a few Swiss and German botanists, like Schwendener, Steinbrink, Ambronn and myself (since 1924). Meanwhile, the theory of Nägeli aroused a real interest in colloidal chemistry. His individual particles corresponded to the particles detected in sols by the ultramicroscope of Zsigmondy. The analogy seemed so

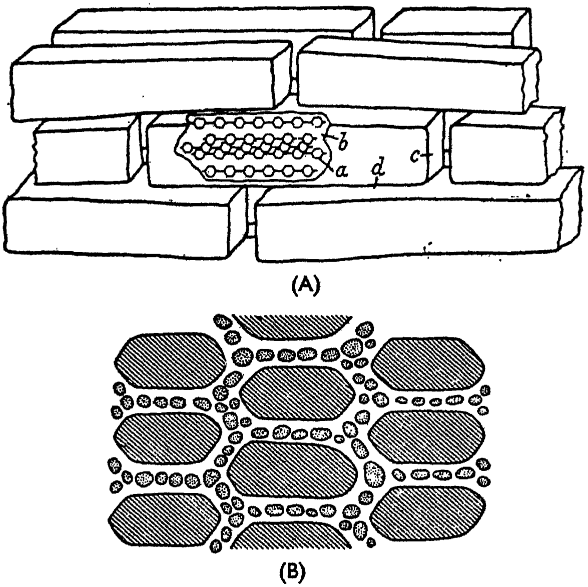


Fig. 2.—Scheme of the submicroscopic structure of cell walls (A) by Seifriz (1929), (B) by Nägeli (1877).

striking that the colloid chemists adopted Nägeli's terminology and called every colloidal particle a *micelle*, although he had created this term especially and exclusively for submicroscopic *crystalline* particles. Later on, when X-ray analysts found that most of the objects investigated by Nägeli have really a crystalline structure, they again adopted his terminology and re-established the *crystalline* micelle. It was indeed a surprising fact in the development of science, when the speculative theory of Nägeli, after having been completely abandoned or ignored by most of the biologists, seemed to be correct, and after the publications of Meyer and Mark in 1930 became generally accepted. Already in 1917 Ambronn [14] had given optical evidence that crystalline rodlets must exist in double refracting biological objects, unfortunately without arousing the interest of the biologists and without earning any credit for his original work; his death in 1927, before the general rehabilitation of the micellar theory, is to be deplored.

Theories have their ups and downs like organisms. So after a short period of general acceptance, criticism arose again. The old question: How can a brick-like structure show such tensile strength as is found in textile fibres, was still unanswered. With such a structure the extraordinary mechanical qualities of the cell walls had to be ascribed to cohesive forces between the individual crystallites (Meyer and Mark) or to an unknown intermicellar substance. Even more intriguing was the viewpoint of Staudinger [15]. With the viscosimetric method elaborated by him he showed that, in solutions of cellulose, the molecular cellulose chains are more than ten times longer than had been indicated by the X-ray method. This had yielded a minimum length of about 600 A, while Staudinger measures cellulose chains of 10,000 to 15,000 A, *i.e.* of 1 to $1\frac{1}{2}\mu$ length. So a controversy arose between this investigator and the X-ray analysts Meyer and Mark, each party maintaining that the method used by the other party was not suitable for measuring the real molecular length of cellulose chains in natural fibres. But as the inconsistency of neither of the two methods could be proved, we proposed in 1935 at the International Botanical Congress in Amsterdam a scheme which satisfies both points of view [16]. In this scheme the cellulose chains are allowed to be several times longer than the crystalline micelles revealed by the X-ray analysis (Fig. 3). There must be regions where these chains crystallise together, and other regions where they are not sufficiently close or regularly parallel to form a crystalline lattice. In this way the former contradiction between the two methods no longer exists, so that *such* a conception has obtained general acceptance to-day. From the standpoint of classical colloidal chemistry the new system is just the inverse of the former brick-like structure : there the cellulose was the dispersed phase and the intermicellar system the dispersion medium, while here the intermicellar spaces appear dispersed within cellulose as a dispersion medium. These spaces may be considered as places where the hydration water of the cellulose molecules is gathered when they crystallise ; their long shape is a consequence of the fibrillar structure of the system. While such a scheme makes full allowance for the extraordinary mechanical properties, especially for the tensile strength of fibres, it cannot explain their permeability and penetrability for the large particles of colloidal dyestuffs. It must be admitted, therefore, that the intermicellar system does not consist of individual holes, but forms a continuous capillary system through which solutions can readily penetrate. In this way we have found by deduction a *reticular system* (Fig. 4). There is no corpuscular dispersed phase, but both the micellar *and* the intermicellar systems are continuous, penetrating each other. Individual micelles do not exist, because they are all connected with one another and grown together, so that we cannot speak any more of micelles as a fundamental structural element ; the micelles of Nägeli correspond merely to crystalline regions within a continuous, porous structure (which in German is called *Lockerstruktur*).

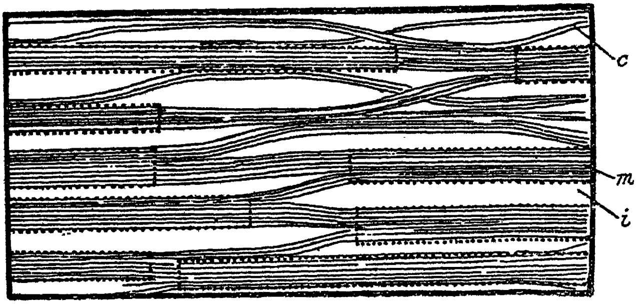


FIG. 3.—Scheme of the submicroscopic structure of a fibre by Frey-Wyssling. c = cellulose chains, m = crystalline cellulose (= micelles *sensu* Nägeli), i = intermicellar spacings.

Such a scheme permits us to understand all the extraordinary qualities of the vegetable fibres mentioned above. These are very long, slender, practically " endless," cellulose chains. They form a micellar framework with crystalline regions (= micelles of Nägeli), causing the remarkable intrinsic double refraction. The unexpected tensile strength and the other mechanical properties are a consequence of the very long molecular chains, because primary valency bonds must be broken when a fibre is torn into two pieces. The anisotropic swelling is caused by the uptake of water into the intermicellar system and the unusual penetrability of this system is explained by its continuity, which permits rapid diffusion in all directions. The discrepancies of density and double refraction are caused by the empty spaces in the micellar system.

While X-ray analysis has given a complete account of the submicroscopic structure of the crystalline regions in the cell wall, so that we know their shape, size and molecular arrangement, we have got but little information on the intermicellar system. From a biological viewpoint, however, this system is more important than the micellar system, which is of primary importance to technical interests because it concerns the mechanical properties of textile fibres, wood, etc. In very young, so-called primary cell walls, the intermicellar system may contain living protoplasm, and in full-grown membranes it is responsible for the permeability and repre-

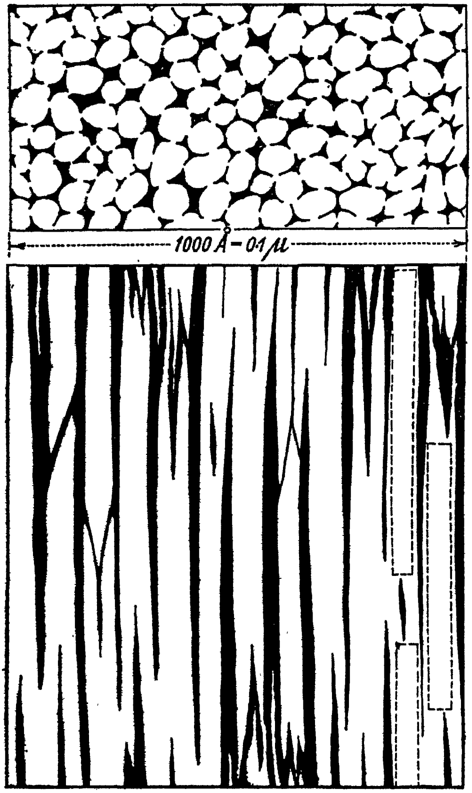


FIG. 4.—Scheme showing the continuity of the intermicellar system; above: cross-section; below: longitudinal section.

sents the place where the so-called incrusting materials (lignin, suberin, cutin, mineral substances, etc.) are deposited.

In order to obtain information about the shape and size of the intermicellar spaces, we have to fill them with a substance that shows both optical *and* X-ray effects. For this purpose we have deposited colloidal gold and silver in the cell wall, by reducing gold chloride and silver nitrate. In this way we get a coloration which shows a beautiful dichroism in the polarising microscope [17]. If the light vibrates in the direction parallel to the axis of a fibre stained in this way, light of short wave-length is transmitted selectively, while light of longer wave-length is absorbed, so that the fibre appears to be green with gold or blue with silver. If the light vibrates perpendicularly to the fibre axis the inverse is the case and the fibres will exhibit a red (gold) or yellow (silver) colour. With the aid of Wiener's theory of "*mixed bodies*" it can be shown that this dichroism is caused by a rod-like arrangement of the submicroscopic particles of the precious metals [18].

X-ray analysis, on the other hand, does not reveal any orientation of the embedded particles. It shows, in addition to the cellulose pattern, *rings* of crystalline gold or silver; and as we have heard, diffraction rings are characteristic of random arrangement. These rings allow us, however, to calculate the width of the spaces in which the metal particles are lodged, since there exists the above-mentioned relation between the width of diffraction lines (measured with a complicated photometer) and the size of the crystalline particles. We have made this investigation and found rather curious figures, indicating that the gold and silver particles in cellulose fibres have diameters of about 100 A; *i.e.* they are twice as broad as the crystalline cellulose strands between which they are embedded [19]. This unexpected result demands remarkably large submicroscopic spaces within the fibres, but of course their number cannot be considerable. They must correspond to the same channels that allow us to stain textile fibres with colloidal dye-stuffs.

As gold and silver particles of 100 A diameter are detectable in the ultramicroscope, our fibres charged with these metals have been investigated also in this direction [20]. They show the typical Tyndall light and exhibit a pronounced azimuth effect; *i.e.* if they are illuminated perpendicularly to the fibre axis they are very bright, but if the lateral illumination is directed parallel to the fibre axis they appear to be dark. (To obtain the optimum silver content for clear ultramicroscopic effects, the fibres have been stained to a series of different depths.) The ultramicroscopic behaviour is a second proof of the existence of rod-like particles embedded in the fibre.

The combination of the optical X-ray and ultramicroscopic analyses allows the following conclusion: there are cubical gold or silver crystallites of about 100 A diameter (Fig. 5) which are associated in much bigger rod-like particles (so-called secondary particles). That this conception is correct could be objectively demonstrated by allowing the rod-like particles to grow until they became visible in the immersion microscope.

Fibres must therefore contain rather coarse capillaries of about 100 A diameter, in which the so-called incrusting materials are lodged and which make it possible to stain the cell wall with colloidal dye-stuffs. On the other hand there must be much smaller spaces of the order of about 10 A, where the water and molecular solutions penetrate, causing swelling or permutoid reactions. The coarse and the fine spaces communicate and are probably of the same nature from the chemical standpoint. But morphologically we have found in this way the important fact that the intermicellar system of cell walls is *heterocapillary* (Fig. 6).

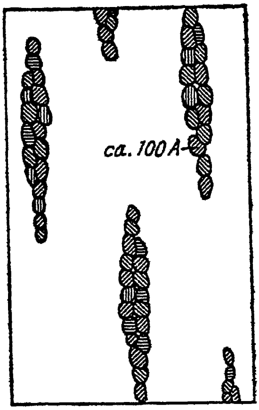


Fig. 5.—Scheme showing rodlets composed of primary gold particles.

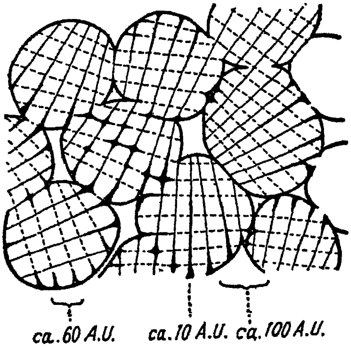


Fig. 6.—Scheme showing the heterocapillary character of the intermicellar system (cross-section); micellar strands 60 A.U.

TYPES OF MEMBRANE STRUCTURES AND THE GROWTH OF CELL WALLS

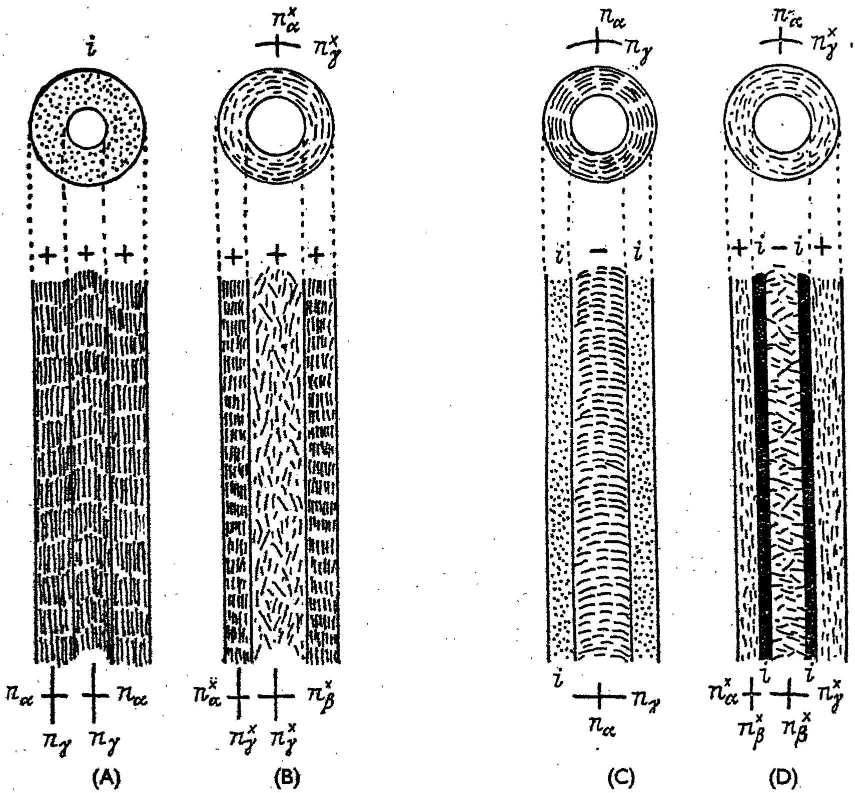
The majority of the above-discussed theoretical conceptions of the submicroscopic structure of cell walls have been gathered from fibres, because their wall structure is simple as compared with other cells. In ideal fibres the submicroscopic micellar strands are all parallel to the fibre axis. But actually this ideal is never completely realised. In general, these strands form a small angle with the fibre axis, causing in this way a *spiral structure*. This is the reason why the X-ray diffraction points of natural cellulose fibres are always elliptical spots, the long axis of which allows us to calculate the angle of deviation from the axial direction. The cotton hairs are the best-known example of cells with spiral structure where that angle amounts to about 30°.

The polarising microscope permits us to recognise other structures as well in which the micellar strands are not regularly parallel to one another, as in fibre and spiral structures, but in which they appear to be arranged at random. This random arrangement can be caused by a certain or even complete disorder of the cellulose strands, or by a superposition of numerous submicroscopic layers with a differently orientated micellar structure. Which of these two possibilities actually occurs cannot be decided, as the polarising microscope reveals only a summarised statistical effect.

If we allow the micellar strands of the fibre structure (Fig. 7 (A)) to scatter, we get a *fibre-like* (Fig. 7 (B) structure [21], which is realised in sclerenchyma fibres, pericyclic fibres, etc. If the scattering increases more and more, we shall get random orientation and the membrane will finally become statistically isotropic, as is known of artificial films produced by drying. This kind of arrangement has been called *folium structure* ; it is realised with isodiametric parenchyma cells, *e.g.* in the pith of elder.

While in fibres the orientation of the micellar structure is axial, in the ring vessels we find a strictly tangential orientation of the cellulose strands, which is called *ring structure* (Fig. 7 (C)). If we start from this structure and allow the micellar strands to scatter, we obtain an arrangement that we have called *tube structure* (Fig. 7 (D)). It displays very interesting optical features. While in a tangential section the cellulose strands of a cell with tube structure show a certain tendency to run tangentially; on the radial section their projections run parallel to the axial direction. In consequence of this fact, the tangential section appears in the polarising microscope to be optically positive ; and between the positive and the negative fields the cell exhibits two statistically isotropic strokes without double refraction. This *tube structure* is realised in sieve tubes, reticular vessels, latex tubes, etc. It seems to be of peculiar importance, as the meristematic primary walls capable of considerable elongation are characterised by this same structure. The cell walls we have dealt with so far are so-called secondary walls, *i.e.* they are definite wall layers that are laid-down against the meristematic wall by apposition, after the cell is fully expanded. During the elongation process, however, the cell is invested by a very thin elastic membrane, which does not show cellulose reactions when microchemical methods are applied. In spite of this fact, cellulose is present, as has been shown by X-ray analysis, but it is masked by membrane waxes, which incrust the young cell wall.

As meristematic cells elongate and reach a length of several times their original size, it might be expected that the slightly tangential orientation of the micellar strands (Fig. 8A) would be changed into a fibre-like orientation (Fig. 8B). But this is not the case. Even in *Avena* coleoptiles [22] and grass filaments [23], where a cell stretching of several hundred per cent. occurs, the orientation of the micellar framework does not change. This unexpected result can be explained in the following way: the scattered cellulose strands are connected with one another by heteropolar points of attachment. If the primary cell wall is stretched artificially, the orientation of the


i
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i n_γ n_α
n_α^x i n_β^x i n_β^x n_γ^x
(A)
(B)
(C)
(D)

FIG. 7.—Scheme of (A) fibre structure, (B) fibre-like structure, (C) ring structure, (D) tube structure.

n_γ = higher, n_β = medium, n_α = lower refractive index, + = optically positive, − = negative, i = isotropic.

submicroscopic structure *does* change, as the points of attachment behave like hinges. But while the cell wall is actively growing this is not the case (Fig. 8c). The points of attachment are loosened and the micellar strands are free to move and change their points of attachment, without changing their direction. It is known that the growing cell wall has a considerably greater plasticity than the resting wall [24, 25], and that this greater plasticity is caused by the action of auxins. We must therefore suppose that the auxin loosens the points of attachment between the cellulose strands by indirect means, thus enabling the wall to be stretched. The mechanism of loosening of the micellar joints is not yet quite clear, although it has been proved that the points of attachment in question must be of a heteropolar character ; because the effect of auxin is strikingly influenced by the addition of dilute salt solutions [26]. As the micellar strands have become more widely separated during the growth process, new cellulose strands must be interwoven by the often discussed phenomenon of *intussusception.*

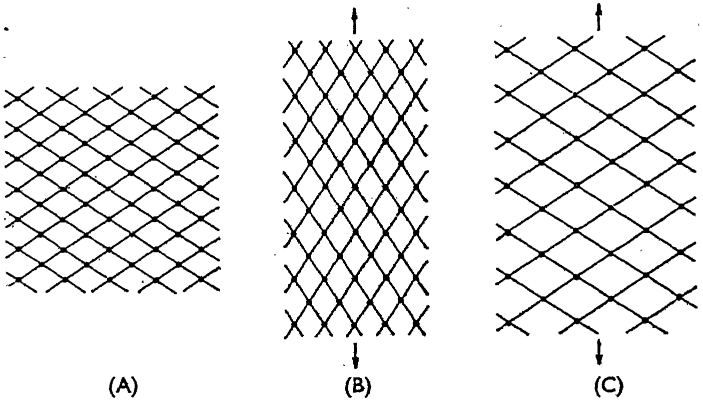

FIG. 8 (A).—Scheme showing the submicroscopic reticular system of a meristematic cell wall ; • = points of attachment.
(B).—After artificial stretching.
(C).—After natural extension.

To sum up, we can say that the application of modern methods of investigation has not only allowed us to establish the submicroscopic morphology of the cell walls, but that it has also furnished a substantial contribution to the solution of the important problem of *growth.*

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AN EXPERIMENTAL PHILOSOPHY OF PAINTINGS

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The title of this article has been chosen with deliberation. Before any progress can be made it will be necessary to submit reasons for believing that the subject exists, and if that much is granted that it may be a profitable occupation to study it. By the time that such a stage has been reached, the reader will doubtless have observed that a fair proportion of the material might have been assembled and used somewhat differently, or at least that the stresses might have been otherwise directed. In other words, the sequel is *an* experimental philosophy rather than *the* experimental philosophy. The author puts forward some tenets which a scientist may accept about art (paintings especially) with a due sense of responsibility. They may be shown to be false, but up to the present they do not contradict experience, which state of affairs an experimental philosophy, however immature, obviously demands. The system, such as it is, derives from the conviction that the conflict—if that is not too strong a word—between the student of art and his scientific brother is as essentially out of date as it is sterile.

We are not concerned with artists in the sense of helping them in their craft; what does concern us is the things which they have created, and perhaps, above all, why they created them.

An enquiry of this kind is no light task; paintings are the "percepta" of our investigations; in some cases they may be exceedingly precious, but in any event we have lost the battle should our experiments do them violence. It is interesting to reflect whether the great masters who painted them prized them as highly as we do; an uncomfortable feeling that our own regard for consummate pictures may be compounded of a lack of creativeness in ourselves, and other less worthy motives, is perhaps a reminder of how far we have to travel. However, if touch them we must, let it be with reverent fingers. Yet, if our data can be obtained otherwise, so much the better.

A magnificent collection of paintings is impressive, and may be productive of pleasure. A test of a sense of values arises here. The scientific mind will want to know, at least in principle, two things. Firstly what exactly the construction of a picture involves, and secondly what in fact it means, a matter for philosophers and psychologists. As a by-product will arise the question of the relationship of the particular painting to the other normal activities of the period (theological, philosophical, social and so forth), while above all, the painter's art can never be divorced from other contemporaneous creative work, such as architecture and sculpture. These are subjects which may well provide problems of great interest for the art-historian.

To answer all these questions involves experiment in some degree in every case, but essentially in the first. Common ground is covered by such investigations to this extent—they demand evidence for everything and yet more evidence. Again, as in jurisprudence, evidence to be of any avail must be the " best evidence," that is, if better is obtainable no judgment can be given until it is forthcoming. Admittedly, there is little place for æsthetics in all this; yet nobody wishes to deny the beautiful; on the contrary it is almost axiomatic.

A quantitative theory of æsthetics has in fact been evolved by Professor Pope. He realises that appreciation of what is permanent and valuable in art differs largely with education and training, and further that inferior work is associated with elements which are essentially discordant. It is conceivable that on this basis, in the limit we should be constrained to admire complete monotony. This is one of the considerations which very naturally present themselves in any attempt to hold the balance between art as a phenomenon—the existence of objects of art—and the motives underlying their creation. A quantitative aspect has also been introduced by Professor Birkhoff in his " æsthetic formula."

The queries already indicated seem to the writer reasonable grounds for supposing that an experimental philosophy of paintings may exist. The study of such a subject would have as its prime object the elucidation of such questions as have been outlined. This would scarcely be worth while if any and every picture were a law unto itself, but from what has been said, broader generalisations may be attempted and thus the rudiments of a characteristic (if only temporary) *Weltanschauung* established. It would, of necessity, be compounded of psychological and physical elements.

This, then, is the position which, as mentioned at the very beginning, must be reached before any detailed progress can be made.

Having regard to the rigorous and perhaps excessive departmentalism of present-day knowledge, it appears possible that some benefit might accrue from an enquiry such as this. Naturally, much trespassing upon fields of thought not commonly associated will be involved. But on the view that there is in fact no trespass without wilful damage rests the hope that occupiers of such property will be kind. For gentle indeed must these excursions be if they are to open up some sort of a path which art-student and scientist may care to tread together.

THE MAKING OF A PAINTING

In the course of a comparatively few years, a new and fascinating subject of study has arisen; the technics of painting. This concerns not only the re-birth of interest in materials, but the wider problem of endeavouring to understand the technique of the great masters, or, for that matter, of any painter. In a word, " How did they do it ? " All the resources of physics, chemistry, and even to some extent those of engineering, have been called upon to supply what answers they can to this question. The present writer has attempted on various occasions to contribute to this process, and he does not propose to burden this discussion with experimental detail. It is perhaps enough to stress the importance of a thoroughly objective approach to these problems ; experience has demonstrated that it is possible to design or adapt instruments of precision for the special needs of a museum or gallery laboratory. Further—and particularly in the field of radiological investigations—it is becoming a little more feasible to interpret empirical results scientifically.

A painting of any description and executed in any medium consists of a series of layers ; it is a stratified structure. These layers consist of the support, the ground, the paint-film and the varnish-film. The support is the wood, canvas, stone, composition-material, which holds the design, the ground is the priming, or gesso which overlays the support, making it suitable for the application of the design itself. The paint-film (" the picture ") is of composite and complex construction ; it consists essentially of pigment plus medium. Finally, the varnish-film affords a protective coating to the whole, and, as a matter of fact, the ideal substance for such a coating has yet to be discovered. If this stratified concept were sufficient as it stands, the task of picture-restorers and others would be greatly simplified. But these layers are not independent of each other, not even when the whole structure is in equilibrium (which it seldom is). The interdependence appears to be of at least two kinds, (*a*) chemical, (*b*) physical.

(*a*) *The chemical interdependence* concerns mostly the paint and varnish films. In fact, owing partly to the similarity of the substances present, it is not always possible to say exactly where one ends and the other begins. They are separate much more on account of their functions (and order of application) than they are by reason of their chemical nature. Again, the presence of "glazes," of subtle importance for artists, complicates the issue a stage further. Such "glazes" are usually pigment-medium phases exceedingly rich in medium, extending to the case when the pigment content is vanishingly small. They depend for their effectiveness upon their high optical transmissivity, and, naturally, upon their tendency to suppress diffuse reflection. But most important of all, there seems to be rarely any such thing, so far as the study of the techniques of the great masters are concerned, as pure oil medium, or pure tempera medium and so on, considered as a definite entity. All the controversy which has surrounded the famous "medium of the van Eycks" is wrapped up with this question, and we are compelled to admit that we simply do not know what their medium was. It is possible, luckily, to estimate some of its constituents by chemical and other means, and to appreciate the part with which emulsions must have played in their studio. And so, a host of paintings are classified as being executed in a "mixed medium." It is even possible that such a mixed medium and pure tempera may exist in close juxtaposition in a certain Italian composition of the late fifteenth century. From this, it will be seen that failure is likely to result from a conservative quest for the secret of the old masters, so far as medium goes. It is just for this reason that the structural classification of paint films recently advocated by Stout is so important. We know that paint films in the hands of van Eyck and van Gogh are totally dissimilar and, as he remarks, they are both oil paintings. Our experimental philosophy therefore will probably be wise to reject chemical categories as a promising field of research and turn towards more physical concepts.

(*b*) *The physical relationships* of the layers to each other is complicated enough, but less intractable. The most interesting property is the mechanical one, which manifests itself in flaking or blistering; terms which are probably obvious enough to pass without further definition. A picture in inferior condition sometimes reveals that the paint film is coming away from the ground, or that both paint film and ground are becoming detached from the support. It is usually assumed that excessive relative motion is responsible for phenomena of this kind, that is the flexure of the support, expansion and contraction and so forth. Doubtless this is true to a great extent, but a more intimate cause may exist in the binding between the layers themselves above the support. This tends to occur when the states of aggregation upon adjacent sides of a common boundary are very discrepant, for example a pigment-rich mixture on one side, and a mere pigmented film or stain upon the other. In these circumstances the lock or "bite" is likely to be insufficient and a loosening action readily sets in. Such a state of affairs offers a refined opportunity for the microscopist, and, given the requisite skill and apparatus, he will be able to demonstrate this, very likely with great practical advantage for remedial measures, or preventive ones, if the trouble is taken in time. It is worth noting that a great deal of information about various techniques can be culled from a study of ailing pictures, much in the same way that knowledge of normal anatomy has progressed by means of pathological researches. It is not inconceivable that investigations of this nature, if sufficiently carefully documented, will be found to have considerable bearing upon our recognition of the "gentle art of faking." An abnormal state may always provide that element of exaggeration which progress requires.

The existence of "crackle," the complicated and sometimes very beautiful network of cracks observable in paintings, represents another interesting and significant physical effect. There may be several such systems of "crackle"; one characteristic kind is due to the warping, or other movement, of the support. Another is superficial, and resides in the varnish-film, usually an indication of over-rapid application, which can be recognised readily with the microscope. Yet another series of fissures may have their seat in miniature volcanoes; that is they are the result of combustion or chemical transitions (with increase of volume and evolution of minute traces of gas) in one of the layers below. Their similarity to geological contortions and faults on a microscopic scale is remarkable.

Attempts have even been made to infer something about the authenticity of a picture from a study of its "crackle." It would be extravagant to claim that this goal has been reached; nevertheless, observation of a large number of samples (duly attested on other grounds) has convinced the writer that the method is by no means futile.

In concluding this section dealing with technique, a brief account of some physical properties of paint itself should perhaps find a place. As a substance, paint exhibits the characteristic feature of thixotropy, that is, its coherence is disturbed by some external impressed force, *e.g.* by stirring or shaking, but when left to itself re-aggregation occurs. In the language of colloid physics, it may be said that thixotropy is a reciprocal sol-gel transition, with the particular feature that the action is isothermal and mechanical. The importance of this for our purpose is that the viscosity is influenced by thixotropy and naturally the elastic constants as well, so that the configuration and general geometrical properties of a brush-stroke became subject to it. A number of reasonable but somewhat idealised assumptions lead to the result that the depth of a brush-stroke should be approximately proportional to the square of its width and the yield value of the paint, and inversely proportional to the surface tension. It is interesting to note what a comparatively " open-work " substance paint is ; the distance between pigment particles (on the basis of electro-potential measurements) may be as much as 10^3A.

THE SIGNIFICANCE OF PAINTINGS

In this section the second of the two main questions will be discussed in some detail.

What does a picture mean ? That an object such as a picture is significant in the sense that (if of any importance at all) it claims attention, is obvious. What is connoted here, however, is that a " fact " such as a painting has a definite meaning and that this meaning appertains to the painting in question and to no other. This is not to assert that all the facts (*i.e.* pictures) and all their corresponding meanings comprise nothing but a system of special cases having only accidental and secondary relations between them. On the contrary, it is to regard the functional property [fact-meaning] as sufficiently definite—and recognisable—to make it profitable to draw broad—if tentative—conclusions. For clearly, to take the mathematical counterpart, it is pointless to enquire about the property of (say) a family of curves, taken as a whole, unless it is feasible to express each in functional form with the least possible number of arbitrary constants. But as soon as the family can be seen *as* a family, significance follows.

This work, so far as art is concerned, needs the establishment of a " methodik." What should be the exact purpose of such a discipline ?

Firstly, to reduce the [fact-meaning] property of a painting to its simplest possible terms and then by comparison to see what is common to analogous paintings, with a view to the elimination of needless co-efficients.

A practical consideration obtrudes itself here. A plentiful supply of photographs of the works in question, taken with scrupulous care, is essential. By " photographs " is meant any reproduction of the original (or part of it) by electro-magnetic radiation such as X-rays, ultra-violet light, infra-red rays, photomicrography, or even perhaps electronic diffraction. All these processes contribute something to the [fact-meaning] quest, by exaggeration or suppression as the case may be. But the experimental conditions must be definite, known and reproducible. A recent thesis dealing with the exegesis of paintings from X-ray data illustrates this, or rather, the absence of it. For whereas considerable skill is demonstrated in interpretation of X-ray shadowgraphs (an exceedingly difficult task) the conditions under which they were obtained—kilovoltage, current, exposures, working distance from focal point of tube and so forth—are not divulged. This goes far to vitiate the nature of the work, since it is common experience to those who make a special study of such tests, that almost any result can be obtained on a X-ray film by varying the physical factors. Such lapses are doubly unfortunate. Not only are they wasteful of much good labour in unravelling X-ray photographs, but their consequences have gone far to " cause the enemy to blaspheme " and to maintain that such experiments are worthless. Criticism of this sort is by no means without justification, but the reader of X-ray photographs is not really exonerated from all responsibility if he goes merrily ahead (as he usually does) without asking what the physical conditions were, the results of which he has before him. And the same is true, in a lesser degree perhaps, of other " photographs " with which the art-historian is accustomed to work.

The criticism that this habit of working with reproductions (photographic or otherwise) sterilises the investigator's sensibility and blinds his " eye for a picture " is for the moment neither here nor there. There are circumstances no doubt in which any attempts to make progress without constant reference to the original are bound to fail. But for the experimental philosopher they are relatively rare. For him, the " methodik " consists largely in exaggeration, first of one property, then of another, enabling him to learn by a process akin to the use of the microscope. Mathematically it somewhat resembles " Iteration " ; the goal is reached by staggering, in which each lurch is of smaller amplitude than its predecessor.

Our discussion of the significance of a picture, or in an abbreviated form, our consideration of the [fact-meaning] relationship, cannot be expected to develop until we attempt to find some place for the creations of great artists among the " documents " of psychology.

This, sounds, perhaps little more than yet another effort to fit everything into a psychological framework. Yet it is impossible to resist the conclusion, reached already upon other grounds, that supreme works were fashioned—if not always, at least very often—when the artist was in an abnormal state. He gives his work to the world in a sense unconsciously, but at the same time well aware throughout of a set purpose, calling out from him a definite (and great) amount of labour. These conditions come very near those underlying the " Gestalt-psychologie " associated with the names of Köhler, Wertheimer and others. It is almost impossible to render *Gestalt* satisfactorily into English: it means " configuration," " shape," " whole " or " form," with an underlying insistence upon an integrative property, and by such an integrative property is suggested that the *Gestalt* is meaningless, apart from the inclusion of the " parts " within the " whole." Yet these " parts " bear to the " whole " (the *Gestalt*) a very peculiar relationship. Their co-operation is essential, and yet the end-result differs from them not only in degree but in kind. A chemical illustration might be the union of hydrogen and oxygen to form water. The properties of those two gases are well known, but water, the stable compound formed by them, resembles neither. This, of course, is a fact known to many generations of chemists, and to identify water with a certain *Gestalt* helps nobody. What does assist enormously however—in the writer's opinion—is the recognition that twentieth-century theoretical physics with all its exact analysis of the energy levels involved still comes independently to the conclusion that, given a set of elementary states, combined in such a way that the residual energy is least (*i.e.* the energy expanded in the process is greatest), then the integrative property is demonstrated in the stability of the final compound, and by its divergence in characteristics from any or all of its components. This illustration gives a hint of a fundamental law of significance, fundamental for processes outside physics, as well as for those encountered in a study of inanimate nature. Such is the law of Prägnanz, enunciated in this form by Wertheimer and others. Thus:

" Psychological organisation will always be as good as the prevailing conditions allow." " Good " means " regular," " symmetrical," " simple," " satisfying," and so forth.

" Prägnanz " corresponds closely, but not quite, to our " significance." Further, it is inherent in the integrative property to which reference has been made that the end-result, or *Gestalt*, is not fortuitous, but eventuates from the purposeful edification, or building-up of the components. Again, the *Gestalt* concept includes what mathematicians call a maximum-minimum relation; that is, there is a connection of some kind between the completed *Gestalt*, the energy needed to produce it, and the available, or free-energy, which it has when produced. Those who have experienced the sublime nature of the science of thermodynamics will recognise a correlative idea with entropy in this. Take over this "methodik" bodily, and apply it to the activity which we are discussing now, creative art, and see how illuminating it becomes.

A finished painting then is a *Gestalt*. It will be a great painting in so far (for our immediate purpose) as the law of Prägnanz is fulfilled in it, that is, that the organisation is as "good" as possible.

Again, it will be a great painting in so far that the artist has given of his best in creating it, that is, that the maximum-minimum property is secured. Yet again, it will "satisfy" in so far that the free—or available—energy which it has is the least possible, for this carries with it (whether we realise it or not) our conviction that the artist has spent himself in producing it, *i.e.* the maximal requirement which is theoretically infinite. Finally, our sense of peace, of repose, which the painting calls forth, is functionally related (it is impossible as yet to say exactly how) to the low available energy (the minimal condition) which it possesses. This minimum in the limit is zero, and naturally a mathematical equation for the path of approach is as yet far beyond us. But one thing can be guessed already, and that is that the free energy curve rises steeply, since our senses are not slow to comprehend when a painting fails to grant us rest.

We might pause for a moment to pluck one wild flower by the way. Man kills the thing he loves (and probably pictures have been the victims of murder more than most objects), and on our reading of *Gestalt*, he does this because of the helplessness of a great work. Its free energy is so small, its self-defence so slender, compared with the energy of the man who created it, and—grim thought but necessary—compared also with the energy of other men who are tempted by a knowledge of the magnitude of that creative urge, which they themselves do not possess.

So far, our thought has been with the dynamics of *Gestalt*-forming. But a moment's meditation upon the static aspect is desirable. The desire to begin a *Gestalt* may well be intuitive, the flash of genius, and the process then goes its appointed way with due regard to order and phase at every step. Naturally, this pre-condition throws over the classical outlook of Hume to a considerable extent. We are discussing an experimental philosophy which cannot exist in a watertight bulkhead, impermeable by contemporary thought in other fields. And there is a strong revolt against a pure positivistic creed, of which the art critic may perhaps take note.

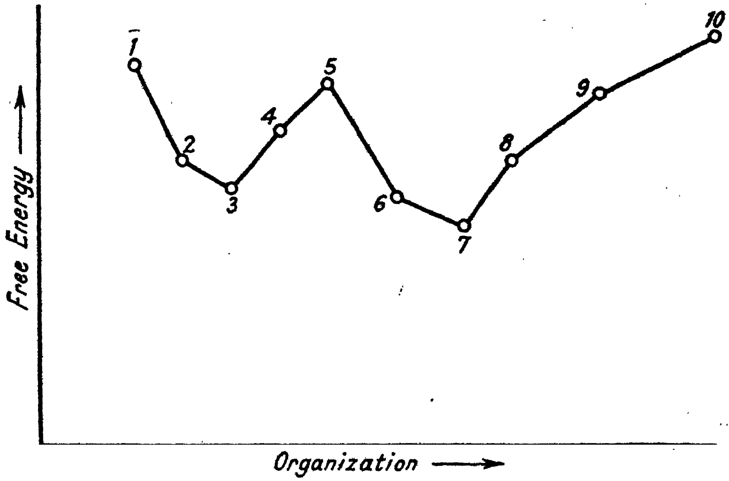
This section was headed "The Significance of Paintings," and we now attempt to sum up the contribution which our *Gestalt* treatment of the subject may have been able to make to the solution of this problem, or rather that particular aspect of it implied in our quest for a [fact-meaning] relationship. Our facts must be obedient to the law of Prägnanz, that is, that the psychological organisation depends upon them, or is functionally related to them. Generally, carefully designed experiments have shown that closed, or nearly closed areas are satisfying (akin to the "stationary states" of physics), whereas unclosed ones are not so. This fact is, more or less, acceptable to picture-lovers, whether knowingly appreciated or not. And apparently it is not lost even in the bold adventures of modern art. Again, the fact that so much can be learnt from black-and-white reproductions demonstrates the paramount place occupied by such properties as grouping and balance.

Next, as to meaning, taken by itself. From the experimental point of view—which is all that concerns this paper—the observer is there solely to register (in himself as it happens) the message or interpretation which the artist intended. He is not there to invoke his own personality at all or any of his (the observer's) reactions. From the scientific aspect, the beholder should be a "lamp-and-scale" recorder, responding promptly to the artist's energy flux, without addition or subtraction. This is only a formal way of stating that great artists certainly knew what they meant to say, and further, that they did not think it necessary to ask observers of their works to interpret for them.

The final object to be achieved in this section is to couple the last two considerations together, that is to achieve the synthesis essential for our [fact-meaning] relationship.

To restate; our facts must be amenable, in general, to the law of Prägnanz, that is, our facts must be "good" facts. Our meaning is the artist's meaning, and not our particular rendering of it. Thus the bond we seek is, in a sense, unique, and "invariant" with respect to a change of observer.

It can only be the energy-difference between the sum-total of the individual urges of the artist's creative instinct (including his zeal to transmit his message without ambiguity) and the (minimal) free energy of the finished work. This "modulus," which is necessarily positive for any great picture, is—if we could ever measure it—akin to an experimental quantity, a philosophical or symbolic measure of the amount of self-annihilation represented by the product on stone, wood, or canvas. This is the outcome of the use of a particular "methodik," *i.e.* the *Gestalt*-concept, to reduce the [fact-meaning] relationship, or property, of a picture to its simplest possible terms. "Picture-fact" we have discussed, "picture-meaning" we have discussed, and a significance of some sort has been given to their co-existence, a significance independent, in theory, of the observer. Like many another reduction to simple terms, the result is not immediately applicable to any particular example selected at random; akin once more to the formulæ of mathematics concrete instances produce prodigious difficulties of manipulation. What value this [fact-meaning] synthesis may have —and indeed the writer submits it with little more than the hope that it may induce others to think about it, or something like it —consists, as was implied earlier on, in its freedom from arbitrary coefficients depending upon such qualities as period, style, school or technique. To compare two pictures then, on this criterion, is to ask, What is the energy-modulus for this one, and what is it for that one? It should be noticed that we do not need to know either the initial energy, or the final energy; our valuation of the merit of the work in question depends solely upon their difference.



The accompanying graph represents quite generally the concept advanced in this paper.

The co-ordinates are Free Energy and "Organisation." The latter contains an implicit geometrical factor, since it is concerned with such qualities as form, balance, grouping, etc. Thus, in abstract, the graph has some correspondence with the Potential curves of physics. Points 1, 2, . . . n are supposed (ideally) to represent successive attempts by the artist to reach a "good" composition within the meaning of the law of Prägnanz. For example, point 3 has considerable merit, but he decides to "organise" more fully, thus attaining point 7 (the absolute minimum), which is assumed to correspond with his supreme effort. Beyond this (points 8, 9 . . . *n*) he merely elaborates without producing a fresh minimum, *i.e.* without " creating " anything more valuable.

It should be noticed that " organisation " increasing towards the right, in the usual way, only indicates an attribute of the composition. The sole " effective organisations " are those associated with points 3 and 7. Points 1 and 2, on the other hand, represent a state of affairs sometimes revealed by X-rays in which the artist has made several attempts before obtaining satisfaction: in other words, his efforts to organise more completely were leading him towards an energy minimum rather than away from it. Once such a minimum has been passed it may be inferred that the energy curve will rise rapidly, since it is usually obvious both to artist and critic that further work, or alteration, would spell ruin to the effect. On the " low organisation " side of a minimum, however, the curve may well be gradual.

Finally, it should be clearly noticed that the energy discussed here is the available, or free energy of a composition, which is necessarily very low—on the *Gestalt* hypothesis—for a great work. This makes no reference whatever—one way or the other—to the " intrinsic " energy which a painting may possess, and which, for certain æsthetic purposes, might be claimed to be large. The present view has no immediate bearing upon this issue, except perhaps to suggest that the position of the observer is subordinate, and that what æsthetic reactions there may be are his affair.

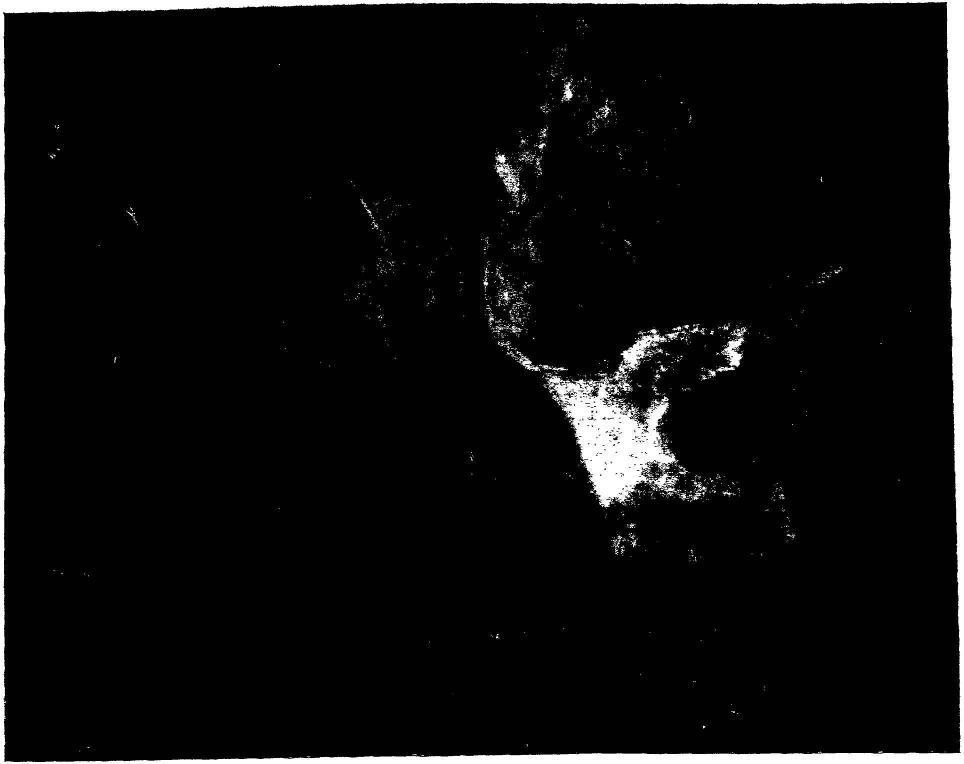
An example of an artist's successive attempts to obtain a " good " organisation is afforded by the illustration of a portrait by Bronzino (1503–1572). The entire picture is shown in Plate I and an X-ray shadowgraph of the left hand in Plate II. The first position was a little lower, the second depicted the hand upon the hip, and the third (and last) entailed an alteration of the fingers and thumb. There are a number of other modifications in this subject, revealed by X-rays, all of which suggest a striving for greater elegance and refinement. Apparently the original effect was one of pent-up energy, whereas the final result is characterised by stability and a diminution of nervousness.

THE RELATION OF PAINTINGS TO OTHER ACTIVITIES

There remains now this question to be discussed. Clearly it is one that many people have asked, and some of the answers obtained are illuminating: it is the author's view, however, that the rigours of scientific method might well be applied, in the hopes of obtaining

Portrait by Bronzino.
(Courtesy of the Metropolitan Museum, New York.)

PLATE II



Portrait by Bronzino. X-ray shadowgraph of the left hand.

(*Courtesy of the Metropolitan Museum, New York.*)

at least a more systematic conspectus of the whole matter. One has only to study the backgrounds of a number of paintings of the best Italian schools to see what an important part architecture plays in their composition; and the same is probably true, on a domestic scale, of many Dutch and Flemish pictures. The point is that in all these instances, the architectural features appear to be conceived in great detail, and rendered with infinite care and fidelity; they are not " extras." Again, it is significant that many of the great names of Southern European pictorial art are famous not only as painters but as sculptors, architects, goldsmiths and in various other activities. In a word, specialisation, in the bad sense, had not then set in, and their works show a generosity of treatment which demands breadth of understanding (and observation) for due appreciation. Thus, we forfeit a great deal in our habit of studying pictures too exclusively, apart from the kindred arts.

Another aspect of this same problem—the inter-relation of the arts—is our academic attitude towards it. On the assumption that paintings are " documents " at all (which it is very difficulty to deny), it seems to follow that they should provide very precious information for the historian, the theologian and the psychologist. If their subjects are matters of live interest at the Universities, which they undoubtedly are, it appears curious that so little use is made of paintings as evidence for the mental background of the age which produced them. It is not stretching our material too far to maintain that a satisfactory experimental philosophy of paintings must include all these studies, fearlessly pursued, independently of existing departmentalism. The " percepta " of such investigations invite the establishment of a school of " symbolic " researches. It is one of the few consolations for scholars which the past few years have produced that the Warburg Institute has settled in our midst, and is bending its great mental energies to interpreting and synthesising classical symbolism in its relation to the humanities. Christian art and pagan art are at one in this, that they express man's progress as a thinking creature.

This article is little more than a throwing-together of meditations which are probably common enough amongst people whose interest is knowledge rather than information. The most that can be expected of it is that it may stimulate curiosity about certain aspects of human endeavour usually somewhat ignored or obliterated in an age which rewards mechanical achievement as highly as ours does. This is not to say that the technical triumphs of our time are not in their way sublime (art-lovers might well be more generous than they usually are towards the intrinsic beauty of modern plastics, reinforced concrete, roller-bearings and the rest) but to recollect that the natural sciences do not fill up the whole volume available to mankind's needs and urges, and that there are reservoirs of creativeness untapped by them. This way of looking at things is the *Weltanschauung* which an epistemological approach to paintings tends to bring about. A writer of last century found in his thoughts the "companions of his solitude" and worked out the idea at some length in a book. These pages cannot aspire to that, but they represent what the writer believes to be a first—and very imperfect—attempt to establish an experimental philosophy of paintings.

THE NATURE AND ORIGIN OF FLINT

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THERE are few things in the realm of geology which seem to arouse the curiosity of the layman more than the oddly-shaped flint, and it frequently falls to the lot of the geologist to answer questions from his friends about this literally common or garden stone. Often these questions are difficult to answer. In the first place they usually concern matters, such as the mode of origin of flint, about which geologists are of more than one mind. Moreover, many of the problems concerning flint lie more within the province of the physical chemist than within that of the geologist.

It is thought, therefore, that a brief review of some of the ideas about flint which have been expressed by geochemists and others during recent years may serve a useful purpose.

From meaning any hard, rock-like mineral, the Old English term *flint* has come in modern times to denote a form of nodular silica, with granular structure and perfect conchoidal fracture, occurring in limestone—more especially in the European Chalk. Some geologists restrict the term entirely to siliceous nodules and nodular layers occurring in the Chalk, and use the term *chert* for nodular silica in all other formations, whether it differs from the Cretaceous variety or not. Others have attempted lithological definition of these two terms, and propose that homogeneous, fine-textured, black varieties of nodular or layered silica in limestones should be termed flint, whatever their horizon. In the present paper, however, consideration is given only to nodular silica in the Chalk, which is flint *par excellence*.

Flint occurs in the Chalk in the form of nodules, either scattered or in bands, and in tabular masses of nodular formation. In this country flint is found mainly in the Upper Chalk, occurrences in the Middle Chalk being sporadic, and in the Lower Chalk negligible.

Freshly broken flint is typically black and sub-lustrous in appearance, although in some localities it presents a grey or brownish colour. Weathered pieces of flint, however, show a considerable range of coloration: they may be blue, white, yellow, red, brown or purplish-black. Such coloration is due to a chemical alteration of the surface known as patination, and when flints so altered are broken open, a black, grey or brownish-grey interior is generally revealed.

The blackness of typical flint is not entirely due to pigmentation. Such flint is usually slightly pigmented by iron compounds and traces of organic matter, but in thin section it appears almost colourless or pale straw-coloured. Its dead black appearance in the mass is mainly an optical effect. The particles composing flint are so minute and densely packed that in thick pieces the material tends to absorb all the light falling on it. Nevertheless, quite apart from surface alteration which may counteract this tendency, flint may acquire a uniform body-colour. In order to appreciate the significance of such variations in the appearance and colour of flint it is necessary to have some understanding of its composition and structure.

Flint is a mosaic-like aggregate of colloidal silica (opal) and crypto-crystalline silica (the form of quartz known as chalcedony) in variable proportions. Opaline silica has the structure of a gel and tends to absorb soluble colouring matter. Thus, fresh flint containing a high proportion of this component may become uniformly brown through the absorption of iron compounds which counteract the tendency to complete light absorption. In some cases the absorption of iron salts may have taken place while the flint was wholly gelatinous, as it would have been during its formation. Diffusion of iron salts under these conditions probably explains the broad colour-banding of the so-called " marbled " flints in the Bull-head Bed of North Kent.

The uniform or patchy greyness of some fresh flint is more difficult to account for. It has been suggested that it is due to the presence of finely divided calcium carbonate within the substance of the flint. While this may be the explanation of the greyness of some flint it does not satisfactorily account for all cases. Black flint becomes a greyish-white when calcined, apparently owing to dehydration of the opaline component. This suggests that in originally grey flint chalcedonic silica may preponderate over the colloidal form. Further research on the structure of flint is needed before such problems can be cleared up. X-ray powder photographs of flint are of little use in this connection owing to the masking effect of the quartz lines thrown by the chalcedony.

By patination is meant a chemical alteration of the surface of flint, generally leading to some colour change. This process is quite distinct from the assumption of body-colour, or interior staining referred to above. The formation of patina on a flint is due to the superficial leaching out of the more soluble ingredient (*i.e.* the opaline silica), leaving a porous skin consisting of a meshwork of crystalline fibres or grains (see Gehrke, 1935). Flint may be patinated artificially by boiling in a strong alkaline solution, such as a solution of sodium hydroxide. In nature, however, the chief patinating agent is undoubtedly carbonic acid in the ionised state. When the patina is sufficiently thick it appears white, since the open meshwork of crypto-crystalline silica offers an infinite number of minute reflecting facets which tend to scatter light instead of absorbing it—as happens when the crystal fibres are buried in opaline silica. When the patina is merely incipient, the shorter light waves only are reflected, and the flint presents a bluish appearance. Unless the flint has acquired a gloss of secondary silica, as often happens in the soil, it is possible, in the incipient stage of patination, to restore the dark appearance of the flint by immersing it in oil; this fills the spaces vacated by the opaline silica. If patination proceeds far enough a piece of flint may become white and porous throughout its thickness.

On account of its porous nature, patina absorbs iron salts and other staining agencies much more readily than most fresh flint does. Thus, while weathered flints in dry soils generally have a white patina, those in river gravels have almost invariably acquired a range of ochreous hues owing to ferruginous staining of the patina.

We may now pass to the more fundamental question of the way in which flint in the Chalk originated. Three problems are involved: the origin of the silica; the mode of its precipitation; and the time of its precipitation. The first two problems really hinge on the third.

With regard to the time-relation between chalk and flint there are three possibilities: that flint is (1) syngenetic, that is deposited in gelatinous form on the floor of the Chalk Sea; (2) penecontemporaneous, that is formed by the segregation of colloidal silica within the calcareous mud before its consolidation as chalk; or (3) epigenetic, that is formed through siliceous replacement of the chalk after its consolidation.

For many years geologists in this country accepted flints as being either epigenetic or penecontemporaneous concretions of silica derived from the solution of siliceous sponge remains (see Sollas, 1905). However, with the spread of ideas about the chemistry of colloids there has been a growing tendency amongst geologists to regard flint and chert in many formations as having originated syngenetically through the direct precipitation of inorganic silica from sea-water in the form of a colloidal gel. Professor W. A. Tarr of Missouri has attempted to explain the origin of Cretaceous flint in this way (Tarr, 1926).

It is an established fact that rivers introduce enormous quantities of colloidal silica into the sea. The amount of colloidally suspended silica in river water averages about 15 parts per million (Moore and Maynard, 1929), and it is estimated that the rivers of the world discharge more than three million metric tons of silica into the oceans each year. Yet in sea-water the content of colloidal silica is rarely more than 1 part per million. Undoubtedly much of the silica introduced into the sea is removed by organisms such as diatoms, radiolarians and sponges which require it for their skeletons. Some is probably precipitated with muddy sediments, because the salts in sea-water, acting as electrolytes, not only cause flocculation of mud, but also induce coagulation of colloidally suspended silica. Silica-gel separating from sea-water in this way, however, is not normally deposited in large masses, but tends to be disseminated uniformly through the sediment. To account for the syngenetic development of flint or chert the assumption has to be made that at certain times conditions prevailed locally which prevented the immediate precipitation of silica by electrolytes, so that the bottom waters eventually became supersaturated with it. There is no evidence that such conditions are prevailing in any sea at the present day, but it is known that the precipitation of colloidal silica by electrolytes can be inhibited by the presence of certain substances, for example, organic colloids. The supersaturation of bottom waters with silica-sol, in the way suggested, would be followed by the separation of silica-gel in treacly or jelly-like masses which, with burial and slow loss of water, would harden and become crypto-crystalline.

It is probable that some cherts have originated in the way outlined above, for instance, the Middle Cambrian cherts of Alabama in which jelly-fish have been preserved (Walcott, 1898), but there are a number of considerations which stand against the application of this theory to Cretaceous flint. They may be enumerated as follows:

(1) There is no trace of derived flint in the intraformational conglomerates occurring at five horizons in the Chalk around Mons in Belgium, although in each case flint occurs in profusion in the adjacent beds (Cornet, 1910).

(2) Nodules of flint occasionally occur along joint and fracture planes in the Chalk. Vertical lines of flint nodules may be observed

—to quote one of many examples—in the Chalk exposed above the undercliff walk, east of Black Rock, near Brighton. Such flints appear in no way different from those occurring in horizontal bands. Tarr's explanation of these vertical stringers as being due to the silica-gel flowing down fissures in the sea-floor is untenable by those with intimate knowledge of our Chalk; the structures in question clearly originated after the consolidation of the deposit.

(3) The occurrence of cylindrical nodules (paramoudras) in vertical position one above the other, as in the Chalk at Horstead in Norfolk, would be difficult to account for except by some theory of epigenetic replacement involving vertical percolation.

(4) Flints in the Chalk commonly enclose complete skeletons of sponges. If flint had originated as a direct precipitate of treacly silica-gel on the sea-floor one would have expected that, occasionally at least, it would have embalmed complete skeletons of Tetractinellid and Monactinellid sponges whose spicules are united in life, but fall apart with decay (Burton, 1931). Yet the only sponges preserved intact in flint are forms in which the spicules are fused or interlocked, such as Hexactinellids, certain Tetractinellids (particularly Lithistids) and Calcarea. Sponges of those types in which the spicules became scattered after death are represented in flint, as in chalk where their solution has been prevented, by isolated spicules only. It might be argued that it was the actual decomposition of sponges with liberation of ammonia which induced the gelation of silica. Yet thick bands of flint occur at horizons in the Chalk where sponge remains are not greatly in evidence, while at other levels sponge remains are abundant, but no flint occurs.

(5) The remains of siliceous sponges are abundant at many levels in the Chalk, but they are mostly preserved in calcite or iron oxide. There has thus been widespread solution of silica in the Chalk subsequent to consolidation, and it would not be unreasonable to suppose that the dissolved silica migrated downwards and then regelated as flint.

In addition to the complete sponge skeletons in the Chalk which have undergone desilicification, loose spicules were originally disseminated in great abundance throughout much of the formation, and these too have largely disappeared through solution. The white powder which commonly occurs at the centre of a hollow flint nodule may be taken to represent a sample of the Chalk before epigenetic changes took place. This "flint meal" is found to consist of finely divided calcium carbonate, together with quantities of siliceous sponge spicules. The late Dr. Hinde identified the spicules of over forty species in the "meal" from the interior of a single nodule taken from the Upper Chalk of Horstead in Norfolk. Yet siliceous spicules are invariably missing or represented by casts in the chalk surrounding such nodules.

(6) The late Dr. Volkmar Wroost of Frankfurt has shown that if one assumes that flint in the Chalk originated epigenetically it is possible to explain almost all the features of its occurrence by the application of physico-chemical principles (Wroost, 1936). Previously the difficulty of the epigenetic theory had been to account for the migration of colloidal silica through chalk, followed locally by simultaneous removal of chalk in solution and gelation of the silica. Wroost has provided what seems to be an adequate explanation of such a process.

Most of the objections which apply to the theory of the syngenetic origin of Cretaceous flint also apply to the theory of penecontemporaneous origin. It so happens that there is evidence of penecontemporaneous segregation of colloidal silica in the Lower Chalk of Wiltshire (Jukes-Browne and Hill, 1889), but it did not result primarily in the formation of flint nodules, only of minute globules of opal which became profusely dispersed through the calcareous ground-mass.

Wetzel (1933) and Deflandre (1936) have called attention to the remarkable preservation of chitinous Protista, including Dinoflagellates, in Cretaceous flint. The preservation of such delicate structures might seem to be a strong argument in favour of the syngenetic origin of flint. However, the recent discovery of chitinous microzoa in certain Silurian limestones from the Baltic region (Eisenack, 1938) shows that delicate material of this nature may, under certain conditions, escape destruction in calcareous muds. While only a few such chitinous bodies have survived to this day in normal chalk, it is reasonable to suppose that even the most delicate ones would have survived long enough to be permanently preserved by any siliceous replacement which took place shortly after the chalk had been consolidated (see below).

It must be noted that there is evidence of more than one generation of flint in the Chalk—probably two. There are records of nodules which have been dislocated and recemented by secondary flint; it also is possible to find nodules in the Chalk which show secondary thickening or tubular hollows secondarily filled with minutely banded flint. This seems to leave the possibility that some flint is syngenetic and some epigenetic. However, the accretionary generation is a comparatively rare phenomenon, and would only account for a minute fraction of the flint in the Chalk as a whole. The "primary" generation forms the bulk of the flint in the Chalk, and it is to this generation, therefore, that most of the objections raised against the syngenetic theory apply.

The time of the main generation of flint in the Chalk was most probably shortly after consolidation, when the formation was being uplifted at the end of Cretaceous times. Its formation may have taken place within a comparatively short time. The evidence in the Belgian Chalk indicates that most of it is post-Maestrichtian—some of it is even post-Montian—while the green-coated flints in the Bull-head Bed at the base of the Lower London Tertiaries are proof of its pre-Landenian origin.

We may now follow the mode of formation of flint in the Chalk as envisaged by Wroost. He considered that as the Chalk was being uplifted, atmospheric waters charged with carbonic acid percolated downwards. The carbonic acid would have been in an ionised state; the free CO_3 ions would have attacked the chalk and so produced a solution of calcium bicarbonate. The amount of calcium bicarbonate formed in this way would be limited by the solubility of normal chalk under the pressure operating at any particular level and by the number of free CO_3 ions in the water. A certain number of free CO_3 ions would continue to exist after the solubility limit of the chalk had been passed. Silica occurring in the chalk as sponge spicules would be in the condition of opal. Each component particle of opaline silica, which is a colloidal gel, has an envelope of adsorbed water molecules; in the presence of a weak acid (*e.g.* carbonic acid) such particles would have adsorbed any free CO_3 ions left over after the maximum amount of calcium bicarbonate possible for the chalk in question had been formed. This would render the silica in the state of a colloidal sol, and therefore free to travel downwards with the percolating water. The mobilisation of the silica through removal of free CO_3 ions from the water would have rendered the bicarbonate content of the water less stable, and there would consequently have been a tendency for calcium carbonate to separate out with liberation of more free CO_3 ions. This would account for the common occurrence in the chalk of siliceous sponges replaced by calcite.

Eventually a state of equilibrium would be reached, and there would be no change in the relative quantities of calcium bicarbonate and silica-sol in the percolating water *so long as the solubility of the chalk through which it was passing remained the same.* However, on reaching a level at which there was a sudden increase in the solubility of the chalk, the silica-sol would lose its adsorbed CO_3 ions to the calcium carbonate, whose claim to it would then be the stronger. The more soluble chalk would thus go into solution as calcium bicarbonate, and the silica, robbed of its mobilising CO_3 ions, would pass into the state of a gel, eventually becoming flint through dehydration and development of micelle-structure (*i.e.* the chalcedony meshwork of flint). It will readily be seen that in this way chalk would be replaced by silica, particle by particle.

Wroost has shown that by giving consideration to the different ways in which the solubility of chalk may be increased, it is possible to account for almost all the modes of occurrence of flint in the Chalk. Two of its most striking modes of occurrence are as internal casts of sea-urchin tests, and in rhythmically spaced tabular layers or bands of nodules.

The occurrence of discrete internal casts in flint of echinoid tests has been difficult to explain on the basis of any of the earlier theories of the origin of flint. Wroost points out, however, that the chalky mud filling or partially filling an echinoid test would have been protected by the calcite corona from the close compaction to which chalk outside was subjected. The uncompacted chalky mud would present percolating water with a greater total surface area of calcium carbonate particles than normal chalk, and would, therefore, in Wroost's opinion, be more readily soluble. If water holding the maximum bicarbonate content for normal chalk, and at the same time bearing silica-sol, percolated through one of these mud-filled tests, it is believed that replacement by silica-gel would immediately take place. For this reason: the calcium bicarbonate content of the water would be below the maximum for the more soluble form of chalk within the test. In consequence CO_3 ions would become detached from the silica particles; the chalky mud would thus pass into solution as calcium bicarbonate *pari passu* with the separation of the silica from the water as a gel.

In support of the idea that differential solubility was the controlling factor in the formation of these discrete flint casts, it may be noted that normally the calcite test itself is not replaced by flint. It is a fairly general rule that calcite shells (*e.g.* *Inoceramus*) embedded in flint are found to have resisted silicification, whereas aragonite skeletons (*e.g.* the coral *Parasmilia*) are in the same circumstances invariably silicified. Calcite is the least soluble form of calcium carbonate, whereas aragonite is one of the most soluble forms.

The occurrence of flint in rhythmically spaced bands appears to be neatly explicable according to the same general principles. The solubility of calcium carbonate rises with pressure, so that as atmospheric water passed downwards through the chalk the amount of calcium bicarbonate which it would have held would have increased with depth. The bicarbonate content would not have risen uniformly owing to the bondage of CO_3 ions ; but as the water bearing silica-sol and calcium bicarbonate passed certain critical pressure-surfaces, the forces binding CO_3 ions to the particles of hydrated silica would have been overcome ; chalk would then have passed into solution as calcium bicarbonate, while silica would have separated as gel. Gelation of silica, once started, would have continued beyond the limits imposed by the increased solubility of the chalk, and in this way an excess of calcium bicarbonate would result. As the water continued to percolate downwards, free CO_3 ions would have been released from the excess bicarbonate, causing the separation of calcite and the mobilisation of more colloidal silica. On reaching the next critical pressure-surface the whole process would be repeated.

The spacing of the bands of flint formed in this way, and also their individual continuity and thickness, would depend on such variable factors as the amount of mobile silica available in the percolating water when it reached a critical pressure-surface ; the vertical distribution of siliceous sponges in the chalk ; the frequency of marly bands (where solubility would be reduced) ; and the rate of percolation of the water.

Given the necessary physico-chemical conditions for the separation of silica-gel at the expense of chalk, there would be a natural tendency for the process to commence wherever gelatinised silica was already present. It is presumably for this reason that siliceous sponges which escaped initial dissolution tended to act as nuclei for the formation of flint. A siliceous or silicified sponge is very commonly found forming the core of a flint nodule.

Once the gelatinous silica had developed micelle-structure, as we may call the chalcedony meshwork, subsequent percolation of carbonated water through the chalk would have had only the effect of removing the opaline matrix, thus producing the white cortex (a coarse patina) invariably exhibited by flint nodules in the Chalk.

Wroost's theory of the mechanism of epigenetic replacement demands an abundant supply of carbonic acid in the percolating ground-water. It is perhaps debatable whether the carbonic acid in rainwater would have been sufficient for the purpose, but bacterial activity in soils formed on the surface of the uplifted chalk would have been an additional source of carbon dioxide. Another unsolved problem is the reason for the practical restriction of flint to the upper part of the Chalk in this country. One suggestion which requires examination is that the primary generation of flint was limited by the level to which the water-table sank during the initial uplift.

In spite of these unsolved problems, there is no doubt that from the geological point of view Wroost's theory goes much further in the direction of providing a complete explanation of Cretaceous flint than any previous one. One thing the theory does emphasise is the delicacy of the balance of electro-chemical forces involved in siliceous replacement of limestone. However, the final verdict regarding the workability of the suggested physico-chemical mechanism must rest with physical chemists. It is hoped, therefore, that this brief exposition of recent views on flint, however inadequate, may at any rate serve to focus attention on Wroost's work, and stimulate geologists and physical chemists to give it critical consideration.

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THE DISINTEGRATION OF CELLULOSE IN THE ALIMENTARY CANAL OF HERBIVORA

BY FRANK BAKER

INTRODUCTION

EVER since the investigations of Haubener in 1855 [1] it has been known that a disintegration of cellulose takes place during the passage of the food through the alimentary canal of herbivora—a circumstance which, in addition to its economic importance, is of considerable biological interest. In ruminants the major focus of this process is generally allocated to the rumen, in non-ruminants to the cæcum. The actual amount of cellulose digested is variable and is influenced by a number of factors including the accessory constituents of the diet. In view of the fact that all efforts to demonstrate the presence of an enzymatic secretion have failed, the changes produced have by common consent been attributed to the action of micro-organisms. This conclusion receives support from the fact, first demonstrated by Tappeiner in 1888 [2], that a disintegration of cellulose can be induced *in vitro* by the inoculation of appropriate media either with fæces or with gastric or intestinal contents. Indeed, from that time onwards, a number of micro-organisms have been isolated in pure culture and their claims to a primary role in the cytoclastic process successively advanced. We may recall in this connection the isolation of an Aspergillus by Ellenberger [3], of anærobic spore-formers by Choukewitch [4], of *B. cellulosæ dissolvens* by Khouvine [5], of *Plectridium cellulolyticum* by Pochon [6], of thermophilic species by Woodman and Stewart [7], and of several aerobic species by Arnaudi [8]. But it cannot be said that these claims carry ultimate conviction; since severally each can possess no more than a quite provisional substantiation whilst collectively they are irreconcilable. Indeed, by the exclusive application of pure-cultural methods, it must always remain difficult or impossible effectively to distinguish real inhabitants from the casual passengers through the gastro-intestinal tract. The food of herbivora is always contaminated with cytoclastic species from the soil or elsewhere; and any one of these species may acquire a prominence on artificial media altogether disproportionate to its functional significance in the viscera of the animal. For these reasons, as in the parallel connection of soil microbiology, a direct microscopical observation of the micro-organisms present and of the changes occurring under natural conditions is pre-requisite both to the initial prosecution and to the final elucidation of pure cultural enquiries.

Observations of this kind, however, have in the past been largely confined to the indigenous protozoa almost to the neglect of other species. The work of W. Henneberg [9], it is true, published in 1922, constitutes a notable exception; but does not appear to have been sufficiently followed up. Henneberg showed that the disintegration of cell-wall substances can be demonstrated microscopically owing to the formation, by the responsible micro-organisms, of zones of erosion upon the affected vegetable structures. Fæces, material from the gastro-intestinal tract, from dunghills and from composts, were all investigated; but no consistent attempt was made directly to interpret the results obtained. Moreover, the mere observation of zones of erosion on the affected structures and the use of simple micro-chemical tests does not in all cases suffice adequately to characterise the nature of the substances attacked.

Subsequent to the period of Henneberg's researches, however, the importance of polarised light as an instrument of microstructural investigation has achieved increasing recognition both in purely academic and in industrial connections. Through the observation of changes in double refraction, the organisation of micellar structures can be studied in great detail and with an accuracy that is unobtainable by the traditional histological technique. Amongst such micellar structures the birefringent polysaccharides of the cell wall, of which in the vegetable kingdom cellulose itself affords the prototype, yield particularly favourable material for investigation. Thus, by the conjoint application of polariscopic and microchemical methods, the disintegration of cellulose is rendered accessible to direct microscopical observation; and the original procedure of Henneberg acquires a precision it does not otherwise possess. A technique incorporating these features was therefore developed by the author and has systematically been applied to the study of the cytoclastic process in herbivora. Before proceeding, however, to summarise the results of the investigation it is necessary clearly to envisage the criteria of disintegration adopted.

Criteria of Disintegration

(1) *Loss of Double Refraction in Polarised Light.*—The cellulose and hemicellulose components of the wall are bi-refringent. Disintegration, consequently, is accompanied by loss of double refraction. Under crossed nicols the affected regions appear as sharply outlined black patches, the intact substratum affording a luminous white background.

(2) *Change in Microchemical Reaction.*—The presence of cellulose and/or hemicellulose is indicated both by a blue reaction with chlorzinciodine and by a strong affinity for tetrazonium dyes. Within the zones of erosion these reactions entirely fail.

(3) *Dichroic Intensification in Polarised Light.*—Under a single nicol, after treatment with either of the above reagents, the cellulosic components of plant structures exhibit dichroic intensification. The maxima fall parallel to the longitudinal alignment of the micellar chains [10]. By this means, therefore, (*a*) the specificity of the staining and micro-chemical reactions can be controlled; (*b*) the persistence or disorganisation of the micellar substratum within a given region may be determined with accuracy.

Practical details of these procedures have elsewhere been described at length. The investigation of the changes taking place can still further be extended by the use of additional and familiar microchemical tests and by examination before and after extraction with appropriate solvents.

These methods have been applied to material from the stomach and cæcum of the sheep [15], and from the cæcum of the guinea-pig [11, 12, 14], rabbit [13] and horse [16]. The results obtained appear to be definitive and enable certain conclusions to be drawn regarding the biological factors concerned in the disintegration of cellulose in ruminant and non-ruminant herbivora. In this connection we propose to consider (1) the cytoclastic process, *viz.* the nature and extent of the changes observed in the cellulosic substrate; (2) the cytoclastic agents, *viz.* the characteristics of the responsible micro-organisms; (3) the functional significance of the iodophile microflora; (4) the general characteristics and functional significance of the indigenous protozoa. In the course of our discussion some reference to the extensive and controversial literature which has accumulated will be inevitable; though no attempt at an historical survey will be made.

The Cytoclastic Process

Essentially similar features attend the disintegration of cellulosic structures both in the cæcum of non-ruminant and in the stomach of ruminant herbivora. All the criteria of disintegration advanced above are uniformly satisfied. The process is primarily evidenced by the formation of zones of erosion around the responsible micro-organisms, which are attached to the disintegrating material. These zones will be referred to as lacunæ. The contours of the lacunæ correspond throughout to the outlines of the micro-organisms. Loss of double refraction in the affected regions is entire and coincides precisely with loss of, or change in, microchemical reaction.

Such features have been observed in epidermal cells, stomatal guard-cells, epidermal hairs, isodiametric parenchyma and in the sclerenchymatous prosenchyma of bast and xylem. By extracting with 4 per cent. and 17·5 per cent. alkali it can be shown that, in addition to hemicelluloses, the more resistant forms of cellulose are also attacked. The cytoclastic process is vectorial and bears a definite relationship to the micellar configuration of the substrate. The directional maxima of resistance run parallel to the main

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EXPLANATION OF PHOTOMICROGRAPHS

1. Disintegration of cellulose. Surface of two contiguous cell walls. Polarised light. Crossed Nicols. The disintegration results in the formation of zones of erosion in which the double-refraction of the cellulose entirely ceases. Cæcum of Horse. × 500 (about).
2. Ditto. Surface of cell wall. Stained Congo-Red and Iodine. Transmitted light. The causative agents are iodophile micro-organisms (black). Around each chain of "coccoids" (see text) can be seen a clearly defined zone of erosion. Cæcum of Horse. × 1400 (about).

3 *and* 4. Surface of contiguous cell-walls.
   (3) Polarised light. Crossed Nicols. Formation of zones of erosion with loss of double-refraction.
   (4) Same preparation. Nicols uncrossed. + Iodine. Distribution of iodophile micro-organisms corresponds precisely to margin of zone of erosion in (3). Rumen of Sheep. × 400 (about).

5 *and* 6. Surfaces of cell walls. Polarised light. Crossed Nicols. Extensive formation of zones of erosion. Early (5) and advanced (6) stages of disintegration. Rumen of Sheep. × 400.

7. Surface of cell wall. Transmitted light. Stained Congo-Red and Iodine. Iodophile micro-organisms (coccoids) in zones of erosion. cf. (2). Rumen of sheep. × 800 (about).
8. Surface of cell wall. Transmitted light. Stained chlorzinciodine. Iodophile micro-organisms at margin of disintegrating wall. Cæcum of Horse. × 800 (about).

9 *and* 10. Surfaces of contiguous walls.
   (9) Polarised light. Crossed Nicols.
   (10) Nicols uncrossed. + Iodine.
   The distribution of iodophile micro-organisms in (10) corresponds precisely to the marginal loss of double-refraction in (9). cf. (3 and 4). Cæcum of Horse. × 500 (about).

The photomicrographs show that the disintegration of cellulose follows a similar course in the stomach of ruminant and in the cæcum of non-ruminant herbivora. In both cases disintegration is accompanied by loss of double-refraction, and of staining or microchemical reaction, in the affected regions. The causative agents are iodophile micro-organisms whose activities result in the formation of the zones of erosion observed.

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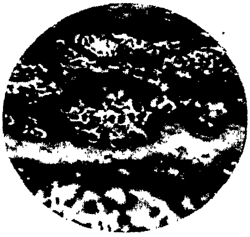
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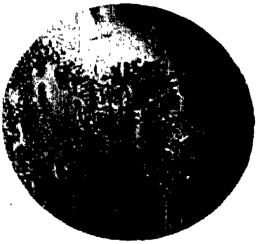
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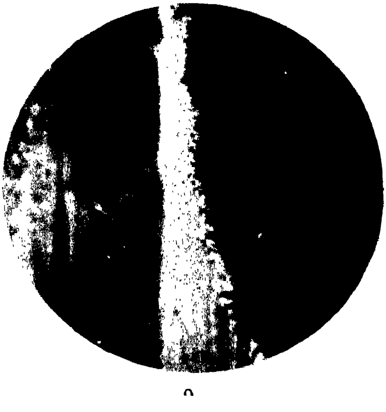
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valance linkages. Conversely, the directional minima correspond, *inter alia*, to the distribution of water in the inter-micellar spaces [14, 16]. Disintegration may be markedly influenced by impregnation of the cellulosic substratum with encrusting substances. It appears, for instance, to be unaffected by the presence of pectic constituents. Cutin and suberin, on the contrary, are not attacked, and confer immunity on structures in which they are incorporated. Hemicelluloses appear in some instances to accelerate disintegration. Slightly lignified but heavily thickened sclerenchyma, for example, may be rapidly disorganised. The hemicellulose content of such tissues, moreover, is known to be high. Heavily lignified tissues, meanwhile, such as the larger spiral, annular or scalariform vessels, remain intact. It may therefore be supposed, that, at least in certain cases, the ease or difficulty with which sclerenchymatous structures are attacked is related to the lignin-hemicellulose ratio of their constituents ; lignin inhibiting and hemicellulose favouring the cytoclastic process. But this hypothesis must be advanced with some reserve ; especially since it is necessary to discriminate between one hemicellulose and another, and between lignin itself and the phenolic or aldehydic derivatives by which it may be accompanied.

The Cytoclastic Agents

The proximate cause of disintegration is the activity of micro-organisms exercised *in situ* upon the surfaces and within the interstices of the vegetable fragments. These micro-organisms present similar characteristics both in the stomach of ruminant and in the cæcum of non-ruminant herbivora [15]. Vibrionic and coccoid forms are encountered which appear to be stages in the life history of some single polyphasic species [*op. cit.*, 14, 15, 16]. Small granules appear in the vibrios which progressively increase in size. Thus the vibrionic or spiriloid forms give place to chains of coccoids of variable length and diameter. Between coccoids of extreme dimensions every transition can be observed. An outstanding feature of the micro-organisms is their blue-mauve reaction to iodine which enables them readily to be detected *in situ* ; but which is far stronger in the final and coccoid than in the initial and vibrionic phase.

It is clear from these observations that the features which attend disintegration in ruminants and non-ruminants are essentially similar both in regard to the effects produced and the causes operative in their production. Nevertheless, they are differently localised in the respective instances. Thus whereas in the cæcum of non-ruminants the disintegration of cellulose is actively in progress, in the cæcum of ruminants the changes taking place in the rumen have long since come to a standstill. The vegetable fragments consequently are in this case observed to be in a very advanced stage of disintegration; but the iodophile micro-organisms so conspicuous upon them in the stomach are for the most part absent from the still visible lacunæ. Here, therefore, as in many other respects, the cæcum and the rumen appear to discharge analogous functions and to stand in a compensatory relation.

THE FUNCTIONAL SIGNIFICANCE OF THE IODOPHILE MICROFLORA

Due emphasis having been laid upon these differences of localisation, we may now consider some of the more general features of the pattern of microbiological relationships in which the cytoclastic process is included. Amongst such features the prevalence of an iodophile habit in the indigenous micro-organisms is especially conspicuous. In the cæcum of the guinea-pig, for instance, an iodine reaction may be observed not only in the cytoclastic species already mentioned but also in the enterococci proper, in the giant spore-forming bacillus, *B. caviæ* of Schussnig [17], in a large spore-forming organism of uncertain identity figured by the author [12], and in the colourless oscillarian, *Oscillospira Guillermondi*, first noted by Chaton and Perard [18] and subsequently described in detail by H. Simons [19]. Similarly, in the cæcum of the horse no less than seven iodophile species have been indicated by the author, of which four remain taxonomically unidentified. Now the mere presence of an unspecified iodine reaction already indicated, as Zopf [20], Boas [21] and Henneberg [22] had independently observed, the sucrophile tendency of a micro-organism. But, in addition, it is noticeable that in the micro-organisms of the paunch or cæcum it is polysaccharides of the blue reacting or amylodextrin type rather than of the red reacting or glycogenic type that tend specifically to predominate. In this connection the example of Oscillospira acquires reinforced significance since, according to Fritsch and West [23], in the majority of free-living species fat or glycogen alone constitute the normal storage products. Nor can the observed deviation in Oscillospira be attributed solely to its adoption of a saprophytic existence; since, in the otherwise similar oscillariæ of the human mouth, described by Reiner Muller and Fellinger [24], it is the red-reacting polysaccharide that is once more encountered. Finally it may be emphasised that the iodine reaction so conspicuous in the cytoclastic micro-organisms of the paunch and cæcum is inconspicuous or absent in the cytoclastic species of the soil; whether aerobic such as *S. cytophaga*, or anaerobic, such as the bacilli of Omelianski. On these and other grounds, therefore, it is at least legitimate to entertain the opinion that the prevalence of a particular pattern of polysaccharide condensations in these micro-organisms may be stimulated by the secretions and subordinated to the metabolic requirements of their host. More precisely, the hypothesis may be advanced [13, 14, 16] that the micro-organisms here specifically function as acceptors of soluble carbohydrates; and so protect from further degradation the sugars liberated in the cytoclastic process. On this assumption, however, the store of carbohydrate initially deposited must at some subsequent and appropriate period be set free. Now it is at least in provisional agreement with this requirement that, in the fæces of all the animals so far examined, the iodophile species are either conspicuously reduced in numbers or have else entirely disappeared. This reduction is, as we have seen, effected in ruminants before, but in non-ruminants only after, the food has passed the cæcum. We may now proceed to examine the agencies by which it is brought about, amongst which the activities of protozoa may play an important part.

THE GENERAL CHARACTERISTICS AND FUNCTIONAL SIGNIFICANCE OF THE INDIGENOUS PROTOZOA

(*a*) *In the Stomach of Ruminants.*—The presence of protozoa both in the paunch of ruminant and in the cæcum of non-ruminant herbivora was known already to Gruby and Delafond [25] in 1843. A few additional types were included by Colin in his Traité [26] of 1854, whilst by 1870 Stein, in a classical memoir, had described several species and genera of the Ophryoscolecidæ. Since that date the structural features and taxonomic relationship of this and other families of the Entodiniomorpha have become the subject of an extensive literature; in which connection the earlier investigations of Schuberg [27], Braune [28], and in particular the magnificent monograph of Dogiel [29], may especially be recalled. The great size of the representative species, their peculiar morphology, structural elaboration and hypertrophy, have often been remarked; and lend to them many of the characteristics which phylogenists assign to climax types. Particularly noteworthy is the frequent presence of organised internal deposits, the so-called skeletal substances, which contain a bi-refringent polysaccharide showing some of the reactions of cellulose. In these and other respects the protozoa of the paunch stand in marked contrast to those of the soil, in which only the simpler levels of structural and functional organisation are commonly attained. No less remarkable than their morphological characteristics is their numerical ascendancy; reaching in the sheep, according to the estimate of Ferber, a figure of 10^6 per c.c. [30]. Nineteen species of the genera Diplodinium, Entodinium, Oxytricha and Isotricha, respectively, have been recorded in this animal [31]. Other and comparable estimates are those of Winogradowa-Fedorowa [32], Dogiel [33] and Becker [34]. In face of these facts, interest has naturally been focussed upon the functional status of the organisms and their relation to the metabolic process of their host. Thus from an early period an abundant ingestion of vegetable particles has been remarked. The digestion, moreover, of starch, glycogen, glucose and lactose, appears definitively to have been established by Trier [35] and is confirmed by the later investigations of Westphal [36]. Mangold [37] records that milk globules are dissolved; so that, since these possess an envelope of casein, an active proteolysis may be assumed. The breakdown of chloroplasts witnessed by Trier supports the same conclusion. Becker and Mowry, moreover, have determined that the numbers of protozoa fall conspicuously when the animals are fed on dry hay which, though rich in cellulose, is deficient in proteins [38]. It may be, therefore, that both the proteins and the growth-promoting substances of green foodstuffs are essential to their welfare.

A far more controversial issue is the capacity, with which the protozoa have from time to time been credited, effectively to digest and utilise the cellulose of the plant structures ingested. This is affirmed by some authorities amongst whom Eberlein [39], Liebetanz [40], Schuberg [41], Weineck [42] may be numbered, and no less emphatically denied by others such as Bundle [43], Scheunert [44], Usuelli [45], Dogiel and Federowa [46], and Westphal [36].

Countenance has been lent to this assumption through the demonstration by Cleveland of the role of Hypermastigotes in the digestion of cellulose in lignophagous termites and roaches [47, 48]. But the analogy is by no means a complete one since the Hypermastigotes lack entirely the capacity to digest starch possessed by the Entodiniomorpha.

Now in attempting justly to estimate the validity of these rival standpoints the following facts appear of importance, though they have very commonly been ignored. In the first place it is conspicuous that the size of the larger proportion of vegetable fragments present is initially such as absolutely to prohibit ingestion by the protozoa. It is in point of fact only the smaller particles that are ingested. But such fragments have usually been excised by the action of the cytoclastic species described above, and by which in many cases they are still accompanied. It seems probable, therefore, to the point of certainty that the disorganisation witnessed has been effected prior rather than subsequent to the ingestion of the particles by the protozoa. To these organisms, then, if any, only a secondary role can directly be assigned in the disintegration of cell-wall substances in the rumen [16].

This is by no means to infer, however, that in an indirect and subordinate capacity they may not play a significant part in the functional organisation of the cytoclastic process. On the contrary, it is evident from the above facts that they conspicuously operate as agents in the removal of the iodophile micro-organisms. In advance of further considerations, therefore, it is certain that they must contribute in a definitive manner to the maintenance of statistical normality between the members of a balanced population. But this is not the only role with which they can legitimately be credited.

The maximum development of the protozoa, for instance, is witnessed in the rumen and reticulum ; whilst in the omasum their rate of division can commonly be observed to fall. In the first two localities they are rich in paraglycogen, their skeletal rods remain intact and their meganuclei are prominent and deeply stained. These features are in marked contrast to those observed in the abomasum. Here the alkaline secretion of the rumen and omasum is replaced by a true gastric secretion which is strongly acid and contains a peptic enzyme. Concurrently, phenomena of advanced disorganisation are witnessed in the protozoa. The cytoplasm shrinks, the meganucleus becomes deformed and the component particles of the skeletal rods dissociate. These processes culminate in the entire digestion of the cell contents, only the resistant cuticular membrane still persisting as an empty case. Within such eviscerated cuticles both ingested starch grains and iodophile micro-organisms have, in the majority of cases, entirely disappeared, though plant fragments, particularly those rich in non-cellulosic residues, may be observed *in situ*. Facts of this kind make it clear, in the first place, that the reserve of polysaccharide initially deposited in the iodophile species has been assimilated by the protozoa ; and, in the second place, that, through the eventual digestion of the protozoa themselves, the totality of these carbohydrates may in due course become available to the metabolic process of the host organism. On this assumption, therefore, whereas the activities of the iodophile micro-organism are primarily organised with reference to the *storage*, the activities of the protozoa contribute especially to the effective *translocation* of the soluble carbohydrate liberated in the cytoclastic process.

(*b*) *In the Cæcum of Non-ruminants.*—With regard to the number and activity of the protozoa indigenous to their cæcum, the non-ruminants so far examined appear to form a graded series. At the one extreme, as in the horse, we encounter associations which approximate in character to those witnessed in the paunch. Amongst the prevalent organisms, for instance, are typical Entodiniomorpha of which, however, the Cycloposthidæ here replace the Ophryoscolecidæ of ruminants. In *Cycloposthium bipalmatum*, the commonest of this family, the ingestion and digestion of both starch grains and micro-organisms may ubiquitously be observed [16]. An ingestion of cell-wall structures can also be established though it occurs more rarely. In the genera of other families such as Blepharoconus, Didesmis, and Paraisotricha no ingestion of vegetable fragments could be observed at all; though both iodine-reacting bacteria and starch grains were present in large numbers. Finally, in the genus Blepharocorys, even starch grains appear to be excluded and the visible diet exclusively confined to bacteria. As in the case of the rumen, therefore, it appears very unlikely that the indigenous protozoa of the cæcum are directly concerned in the disintegration of cellulose. Similar conclusions, in the meantime, to those advanced above are still valid in regard to their role in the preservation of statistical normality and in the translocation of carbohydrates. These conclusions, however, cannot be extended to the other extreme of the series as represented by the rabbit. For here, even in animals obtained from widely separated localities, both spirochætes and protozoa may, in the experience of the author, be entirely absent. The reduction of the iodophile microflora still witnessed in the fæces must in this instance be occasioned by a different agency, and the translocation of carbohydrate effected along some alternative route. The guinea-pig is intermediate between these extremes. As in the horse, both spirochætes and protozoa are present in large numbers. The protozoa, however, are confined for the most part to small species such as Trichomonas and Hexamitus [14]. Although an ingestion of iodophile micro-organisms can be observed, it appears quite insufficient to account for the reduction observed in the fæces. Again, the Entodiniomorpha encountered in the horse are lacking; so that a direct comparison with the microfauna of the paunch can no longer be sustained.

CONCLUSION

Even from a cursory examination of these data, it will be evident that many details of the cytoclastic process in herbivora stand urgently in need of further investigation; not only by biochemical and pure cultural methods but, also, by the direct microscopic examination of fresh or preserved materials.

The influence of changes of diet both upon the characteristics of microflora and fauna and upon the amount of cellulose digested is a case in point, and acquires a special importance where, as in the instance of domesticated animals, the enhancement in amount and marketable value of their products is secured by intensive feeding. Again, the variation in numbers and relative proportions of the indigenous species at different phases of the digestive process requires to be more accurately determined and the results evaluated by an appropriate statistical analysis. The resolution, however, of both this and the previous problem devolves to an important extent upon the development of a satisfactory method for the continuous collection of fresh samples from the living animal. Precisely in this connection the combination of the methods of direct microscopical observation described above and the admirable fistular technique elaborated in recent years by Quin [49], suggest a number of profitable and hitherto unexplored avenues of approach.

A problem which is far more difficult of resolution and which is perhaps in the nature of the case insoluble concerns the actual origin of the micro-biological associations established. It is impossible, for instance, to state, in advance of further investigation, whether or not these specifically cytoclastic species are imported anew from without at each generation; or whether they are continuously transmitted from one animal to another. In the case of many of the protozoa, however, it is evident that the second alternative is the one exclusively adopted. The entire order of Entodiniomorpha, for example, show no close affinity to any free living species and, as already stated, are sharply distinguished in their morphological features from the common protozoa of the soil. A parallel instance is afforded by the Hypermastigotes of termites and roaches. Cleveland [48] has shown that similarities in the microfauna are attendant upon the taxonomic affinities of their respective hosts; and may, if only conjecturally, be in principle referred to a common phylogenesis. But identical considerations can scarcely be valid for the Entodiniomorpha; since, whereas, as we have seen, these are present in the paunch of ruminants and in the cæcum of the horse they are absent from the cæcum of the guinea-pig and the rabbit but recur, nevertheless, according to the observations of Von Reichenow [50] and others, in the large intestine of such taxonomically distant species as the fruit-eating apes. We are confronted, therefore, by the apparent paradox that whilst, on the one hand, the peculiarities of structure and function so conspicuous in this order are not acquired *de novo* in the course of a single generation, they cannot, on the other hand, have originated by phylogenetic transmission from a single ancestral type. In short, we are compelled to postulate a polyphyletic origin for features whose genesis in any case, on any of the traditional evolutionary hypotheses, remains essentially obscure.

Despite these obscurities, however, there are still a number of conclusions and hypotheses which the existing data make it necessary to consider. Thus, in the first place, it is clear, from the features of the cytoclastic organisms themselves and from the general characteristics of the iodine reacting organisms, that an essential unity underlies the superficial diversity of the cytoclastic process in different species of herbivora. We receive the impression, throughout, of a genuine extension to the effective range of the digestive process accomplished by the incorporation of a microbiological association organised with reference to the metabolic requirements of its host [51]. This association, however, whose existence is related in general to the " fitness " [52] of the inner environment and in particular to the prevalence of cellulose in the diet, attains different grades of elaboration and complexity in various species. It seems evident, for example, that the associations of the paunch of ruminants stands at a higher level of organisation than those of the cæcum of non-ruminant herbivora, an advance that may be integral to the major structural and functional displacements attendant upon the adoption of the ruminant habit.

For, as all the existing evidence emphatically goes to show, the cytoclastic process is inherently dependent upon an interlocking of activities, and upon an adjustment of growth rates between the members of a mixed population. The preservation of such a state of dynamic equilibrium, however, cannot be uniquely referred to the operation of some single biological factor. The ingestion of particles, for example, by the protozoa is limited in the immediate instance by their respective dimensions. But the size of the particles in turn devolves upon the degree of trituration to which they have been subject; and so ultimately both upon the type of dentition and the nature and duration of the process of mastication. Again, the ease of difficulty with which the cellulose itself is attacked by the cytoclastic micro-organisms is, as we have seen, influenced profoundly by the presence of encrusting substances; components which vary in kind and amount not only from one plant species to another but in the same plant at different stages of maturity and under different conditions of climate and cultivation. Now these differences in fine structural organisation are reflected, as such terms as woody, herbaceous, thorny and succulent, sufficiently indicate, in the macroscopic features of texture and habit; qualities that determine to a greater or less degree the capacity of plants to attract the attention and provoke the appetites of animals. Not only the dentition of an herbivorous species, therefore, and the relative development of the stomach and cæcum, but also its instinctive behaviour and ecological context, may intervene effectively to modify the pattern of microbiological relationship established. Indeed, it will be now apparent, that the cytoclastic process is a single and indivisible operation, organised with reference to the attainment of some final state, and determined throughout its trajectory, from the ingestion of the food to the expulsion of the fæces, by a " complex relationship of internal states to external factors " [53]. But, notwithstanding the essential continuity of the operation and precisely in consequence of the number and diversity of the factors involved, different stability maxima may at each level of organisation characterise the microbiological associations of various herbivorous species. In this connection, a point remains to be considered that has so far escaped attention.

The increase in microbiological complexity in the associations of ruminants is primarily manifest in regard to the number and characteristics of the protozoan population. Now, in addition to the secondary relations of these organisms to the cytoclastic process conjectured above, a primary role has sometimes been attributed to them as a direct and additional source of nitrogen to their host. The validity of this assumption rests in part upon the sources of nitrogen utilised by the protozoa themselves. Thus it has been suggested by Karl Schwarz [54] that the bacteria, demonstrably ingested in large numbers, are capable of utilising a lower grade of nitrogenous compounds than either the protozoa or their common host. In this case there would be occasioned by the synthesis of microbial protoplasm, a regradation of nitrogenous substances which, in consequence of the eventual digestion of the protozoa, would in due course become accessible to the ruminant animal. If, furthermore, the reality of this process of " biological ennoblement," as the Germans term it, were substantiated, it would evidently become necessary to discover the lower limit and complexity of the compounds utilised. Indeed, the possibility of an anaerobic fixation of nitrogen on the part of the cytoclastic micro-organisms themselves could not then entirely be ignored. But, here, as elsewhere, the final elucidation of the biological factors concerned presupposes not only a systematic and comparative investigation of the cytoclastic process in herbivora, but also far more accurate information than at present exists upon the influence of domestication on the microbiological patterns and stability maxima of wild species. In the absence consequently of further data on this subject a more lengthy discussion would be premature.

It remains, therefore, to notice in conclusion that in the cytoclastic process a variable proportion of the cellulose present in the food may and normally does escape digestion, whether on account of the presence of encrusting substances or for reasons that are still obscure. Under the polarising microscope it can readily be demonstrated that these residues, subsequent to their expulsion with the fæces, no longer remain intact but, after a short time, enter upon a further and rapid phase of disintegration. In the production of these changes, however, an entirely new system of microbiological relationships is established in which the activities of fungi and actinomycetes often play a predominant role [55]. It is clear then that the cytoclastic process does not culminate in the bowel but that the disintegration of cellulose is completed outside the animal by micro-organisms of an altogether different kind. The initiation of this second process must be due to many factors amongst which the by-products of digestion in the form of substances stimulating to fungi—a fact already evident in the case of the coprophilous species —may exercise a determinant influence. Thus the point of arrival of the changes taking place in the intestine affords a point of departure for those occurring in dung, in farmyard manure and in the soil.

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SOME ASPECTS OF TIN RESEARCH—PART II

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In a previous communication (Science Progress, 1938, **33**, 66) a number of the publications of the International Tin Research and Development Council were summarised and reviewed, and since that date a large amount of further work has been published dealing with various aspects connected with the use of metallic tin.

A subject of particular interest at the present time is dealt with in No. 85, which reports on the examination of some tinned food of historic interest and describes how a number of tins of meats and vegetables which had reposed in the Royal United Services Museum and the National Maritime Museum, London, were opened and submitted to chemical and bacteriological analysis. Two of the tins had accompanied Sir Edward Parry, the Arctic Explorer, on his famous expeditions in search of the North-West Passage in 1824, and when opened they proved to be in " what one could fairly call perfect condition." The first part of this report recounts briefly the discovery of the principles of the canning process by Nicholas Appert in France and their adaptation by Bryan Donkin, a Fellow of the Royal Society, so that robust canisters of tinplate could be used instead of Appert's fragile glass jars. So successful was Donkin that within seven years his tinned food factory at Bermondsey, the first in the world, was supplying tens of thousands of large tins to the Navy for Australian and Arctic Expeditions and later as " medical comforts " for crews of ships on ordinary voyages.

Tinplates stored in a damp place sometimes become covered with droplets of liquid, a defect known in some works as " sweating," and this defect is reported upon in No. 80. It is shown that inclusions of zinc chloride trapped under the tin coating are responsible. It is stated that every pit and fissure which may be produced during the pickling operation which precedes the tinning of the steel may retain by capillarity, a small quantity of flux which becomes an inclusion. By suitably regulating the pickling operation, this form of corrosion of the steel can be completely eliminated and the amount of flux inclusions considerably reduced with a corresponding lessening of the sweating of the tinplates.

A new process known as " stannising " for coating brass, copper, iron, zinc and other metals with tin and which is applicable to articles of the most complicated shapes is described in No. 76. This process consists in coating the articles with tin from the vapour phase and is carried out by suspending them for a few minutes in an atmosphere of hydrogen and the vapour of stannous chloride at a temperature of 500–600° C. In contact with the metal surface the hydrogen reduces the chloride to metallic tin which then alloys with the metal and the thickness and composition of the alloy formed depend upon the regulation of the temperature and time of exposure and on the proper circulation of the vapour. By passing the gases from the stannising chamber through molten tin they are regenerated for recirculation. Coatings can be applied to rough, threaded or irregular surfaces and machined parts can be built up to size after first machining to undersize. From the porosity point of view the stannised coatings on sheet steel strips are found to be better than those on ordinary tinplate.

The electro-deposition of tin is possible from either alkaline or acid baths and two publications deal with these electrolytes. No. 82 deals with alkaline solutions and shows that stannite baths have to be used with care owing to their liability to oxidation, whereas stannate baths can be used over a wide range of plating conditions. Metallic tin anodes are satisfactory if the current density is controlled so that an oxide film is formed and maintained over them. The effects of addition agents and chlorides are considered and methods for the analytical control of the plating solutions are described.

The production, maintenance and nature of the film on the tin anode mentioned above is the subject of a separate report, No. 91, in which it is shown that this film consists principally of hydrated stannic oxide containing some stannous oxide.

Acid sulphate electrolytes, which also formed the subject of an earlier publication (No. 56), receive further attention in No. 83, particularly as regards the effects produced by three addition agents, all of which have been found to be necessary, namely a sulphonated aromatic compound, *e.g.* cresol sulphonic acid, a protective colloid such as gelatin and a hydroxy compound such as naphthol or resorcinol. Different combinations of addition agents in baths of the same basic composition have been worked for nearly six months, and the effects of several types of protective colloid have been examined, and it has been found that the bath giving a consistently good deposit over the longest period contained 30 g. of tin (as stannous sulphate), 60 g. of free sulphuric acid, 100 g. of cresol sulphonic acid, 1 g. of lysalbic acid or 2 g. of gelatin and 1 g. of β-naphthol per litre.

Tinplate, after being plated with nickel or chromium, owing to its high surface lustre and the fact that it can be stored without rusting, offers advantages over sheet steel as a basis material for fabrication into articles, and the factors affecting the adhesion of the electro-deposited metals to the tinplate are the subject of No. 79. "Charcoal" grades of tinplate (4 lb. quality) are preferable to the less thickly coated "coke" grades (1·5 lb. quality) owing to the reduced likelihood of rusting at breaks and pores in the plated coatings and because a better finish is more easily obtained and adhesion is improved. The tinplate must be coated with copper before nickel plating, but a chromium coating can be applied directly without nickel and copper undercoats, although without these it is more difficult to obtain a good finish.

The uses of tin and its alloys in dentistry are reviewed in No. 89. In the introduction it is mentioned that tin has been used for fillings at least since 1783. Fillings commonly contain 25 per cent. of tin amalgamated with silver, as this composition, suitably treated, has the requisite extremely small expansion during setting so that the fillings do not drop out and at the same time do not expand sufficiently to give pain. Compared with gold and silver, tin has a low conductivity for heat and therefore can be used closer to heat-sensitive pulp.

The mechanical polishing of soft metal specimens such as of tin for microscopical examination has proved practically impossible owing to the fact that the abrasion necessary may produce a false structure to a certain depth from the surface. No. 90 describes the method of electrolytic polishing of tin in which the specimen with a cast or roughly sawn surface is made the anode in a concentrated mixture of perchloric acid and acetic anhydride or acetic acid; a piece of tinfoil is used as the cathode with a current density of 9–15 amps. per sq. cm., obtained from a direct current supply at 25–40 volts. With a cast surface a high polish can be obtained in from three to four minutes, but a roughly sawn surface may require forty minutes for polishing. Selective electrolytic etching of the specimen may be obtained with a lower current density than that given above.

The effects of small additions of tellurium on the mechanical properties of pure tin are dealt with in No. 81, in which additions up to 0·1 per cent. are reported upon. These small additions slightly improve the Brinell hardness number and the tensile strength of tin, but heat treatment does not produce permanently improved properties. Tellurium considerably improves the creep strength of pure tin, and grain size of the specimens is found to have a marked effect on the creep strength.

The working properties of plumbers' solder having a frostly surface is dealt with in No. 73. It has been known that frostiness and also bad working properties may be produced by traces of impurities and for this reason frostiness has been considered an almost infallible indication of inferiority. Frostiness which is now shown to be produced on good solder when this is cast into an overheated mould is due in this case to the growth of large crystals and has no detrimental effects on the soldering operation. Some interesting results of work carried out by two selected plumbers are given, which illustrate the psychological reaction of even experienced workmen to preconceived ideas as to what the appearance of materials worked with should be.

The influence of fluxes on the spreading power of tin solders on copper is the subject of No. 88, and this spreading power under a layer of resin or zinc chloride flux has been evaluated from measurements of the contact angle between the solder and the flux. The fact that solders spread better than pure tin in either flux is ascribed to their lower surface tensions ; at the same time their adhesion is not found to be inferior.

The factors that may determine the service life of tin-base bearing metals are dealt with in No. 75. It is pointed out that white bearing metals, rich in tin, have already been used for many years for conditions of severe service in a wide variety of engines and machines. In recent years, in certain types of internal combustion engines in which the loading on the bearings is very severe, trouble has arisen owing to the cracking and breaking up of the bearing metal and the study of these failures is reported. Tests carried out showed the degree of stress in white metal linings on thin sheet shells due to expansion and contraction, and also to what extent the stress could be relieved by creep. The fatigue strength of bearing metals is dealt with, and it is pointed out that although tensile stresses are not directly applied during the use of bearings, they nevertheless may be present, and the most probable cause is the tension set up as a result of the considerable differences between the co-efficients of thermal expansion of the white metal and the steel shell upon which it is cast. As a result of experimental work it is concluded that in the case of tin-base white metal containing 7·0 per cent. antimony and 3·5 per cent. copper, less than 2 per cent. of cadmium and no lead should be present.

RECENT ADVANCES IN SCIENCE

ASTRONOMY. By A. HUNTER, Ph.D., F.R.A.S., Royal Observatory, Greenwich.

A NEW KIND OF STELLAR SYSTEM.—The bricks of which the universe is built are the stars. It is a very interesting fact—and a challenge to the theorists—that there seems to be an element of economy in creation which puts certain limits to the kinds of bricks used: for instance, there are apparently no extremely heavy stars, though the range of sizes is enormous; again, there are no middleweights amongst the cool stars—they are either relatively heavy or relatively light. This element of economy persists when we consider the buildings of the universe, the way in which the stars are associated together: here, too, many conceivable cosmic systems seem to find no place in the evolution of the universe. There are single stars, such as our own sun (we do not know, nor can we make more than tentative guesses, whether there are other planetary systems, but in any case planets are very small fry, cosmically speaking). There are binary systems, in which two stars are sufficiently close to one another for their mutual gravitation to cause them to revolve in observable orbits; these extend from wide pairs, whose components creep round each other in such long periods that astronomers have not yet seen more than a fraction of the orbit, through pairs so close that only the spectroscope can tell them from single stars, to pairs whose members are so cramped together that they are almost in contact and raise enormous tides in one another. There are multiple stars (considerably more rare, these), such as Castor of the Twins, which is a visual binary of period exceeding 300 years, accompanied by a faint companion, more than a minute of arc away, which revolves about the bright pair perhaps once every 15,000 years; each of the three components being itself a short-period spectroscopic binary, so that the light reaching the earth comes from six stars in all.

Such multiple systems, rare though they are, prevent a marked hiatus between binary stars and the open clusters, systems containing a few dozen to a few thousand stars which are separated by distances so great that orbital motion is not recognisable, but which move through space much as a flock of birds does, with common proper motions towards a common convergent point. If such systems are small and at a great distance, they may be classed as a single star (θ Orionis is an example which is hardly distinguished from a multiple star); if they are somewhat nearer they will appear as a hazy spot of light (such as the Præsepe cluster in Cancer); if they are close, the separate stars are resolvable to the naked eye (*e.g.* the Pleiades). If such a system is extremely close, the members are scattered over a large area of the sky, and the cluster (which is then a typical "moving cluster") is recognisable only from the community of proper motion amongst its components. An extreme case of this is the Ursa Major cluster, which includes all the bright stars of the Plough except the ones at each end, and also Sirius and many other bright stars in totally different parts of the sky. The sun is evidently actually within this cluster, though not a member of it. The shape of these open clusters is usually quite irregular; it may vary from the more or less spherical form taken by the Pleiades to the flattened grouping shown by the Ursa Major cluster.

But after the open cluster the inventiveness of nature seems to falter. The next building unit of the universe is the globular cluster, which is totally different from the open cluster in size, in compactness, in the total number of stars it contains, and in rarity of occurrence. The globular cluster consists of hundreds of thousands of stars packed densely into a close spheroidal swarm highly condensed towards the centre. Even the closest is so far away that it is only just visible to the naked eye as a diffuse spot; it is twice as distant as the farthest known open cluster. With these globular clusters, of which we know only about 100, the number of known stellar systems in our own universe comes to an end: the galaxy is merely an agglomeration of single stars, binaries, multiple stars, moving and open clusters, and globular clusters, together with a little stray matter in the form of clouds of atoms and dust, this latter being by mass quite negligible, though its presence leads to spectacular bright nebulæ and patches of irregular obscuration. And as far as we know, other galaxies, the so-called spiral nebulæ, also consist of these same constituents, possibly in much the same proportions.

It may be, however, that a recent discovery announced from Harvard (*Nature*, **142,** 715, Oct. 15, 1938) has added another type of stellar system to our list. Shapley reports that long-exposure plates taken at the South African out-station of the observatory show the presence of two faint, widely-extended stellar systems, in

Sculptor and Fornax respectively, which are apparently neither open nor globular clusters. On first inspection of the Sculptor cluster, the first to be discovered, it was thought to be an extended cluster of external galaxies, but subsequent examination of photographs taken with the 60-inch reflector showed that the individual members are stars, not spheroidal galaxies. The objects are about a degree in angular diameter (they therefore cover four times the area of the full moon), and are roughly circular in outline, with smooth but not conspicuous symmetrical condensation towards the centre. Their outstanding feature is the extreme faintness of even the brightest individual stars, which are of apparent magnitude only 17·8. Brighter stars appear on the plates, of course, but these are recognised as foreground stars by star-counts in neighbouring regions of the sky. Eighteenth-magnitude stars are abundant, and altogether down to about magnitude 19·5, there are 10,000 stars in each system, though there can be little doubt that many more exist beyond the plate limit. It is evidently the low surface brightness of the objects which has hitherto prevented discovery: long-focus telescopes will not show such systems except with exposures long enough to bring out the separate star-images, whilst short-focus cameras, although not reaching within four magnitudes of the individual stars, will record the integrated clusters as hazy patches which on such small-scale plates might be almost anything, and would probably be put down to faults in the emulsion. There is, of course, no question of visibility to the naked eye; the integrated magnitude of the Sculptor cluster is only about 9·0, sixteen times fainter than the faintest naked-eye stars, while the Fornax system is fainter still.

Inspection of the photographs shows that the new objects have some properties in common with globular clusters, and some with such close external galaxies as the Magellanic Clouds. In uniformity and radial symmetry they resemble globular clusters; but by comparison with say ω Centauri, the nearest of these clusters, they are ten times bigger in apparent area, but send us less than one-hundredth of the amount of light; and, moreover, their central condensation is far less. Since all known globular clusters are extraordinarily alike, both in size and in other characteristics, astronomers will be reluctant to class the new clusters with these. In complete resolution, though not in total number of stars, they resemble the Magellanic Clouds, but they are too regular in outline, too devoid of supergiant stars, open clusters, and diffuse nebulæ, luminous or dark, for this comparison to be given much weight.

If this new kind of cluster is to be regarded as a member of our galaxy, then even the brightest of the individual stars must be dwarfs, and the linear dimensions of the whole assembly those of a giant open cluster. If the brightest stars are ordinary giants, the clusters will be close external galaxies of size approximately that of the Small Magellanic Cloud. If the brightest stars are supergiants, as in our own and most external galaxies, then the clusters are gigantic systems like the Andromeda nebula, but spheroidal in shape instead of spiral, sparsely populated instead of densely, and completely resolved instead of nucleated.

Evidently the crucial test is the determination of their distances. With such faint stars as components, it is of course quite incredible that ordinary trigonometric observations of parallax will give measurable results. We are therefore driven back to the standard method for determining great astronomical distances, *viz.* the recognition of objects of known luminosity, and subsequent comparison of the absolute magnitudes of these objects with their apparent magnitudes. Obviously the apparent magnitudes used must be correct; in particular, they must not be affected by absorption of light in space, otherwise the dimming due to this cause will be attributed to the inverse-square effect, and an overestimate of distance will result. Luckily, both the new clusters lie in the south galactic polar cap, where absorption within our galaxy is known to be negligible [1]; and where confusion with foreground stars is at a minimum; further, there can be no appreciable absorption within the clusters themselves, since very distant spiral nebulæ can be seen in as great numbers through their central regions as in neighbouring parts of the sky. Our apparent magnitudes, then, are reliable, except of course insofar as photometric measurements on such faint stars at the plate limit are difficult anyway. The absolute magnitude determinations, however, are another matter. If the spectra were known, luminosities could be assigned to the various stars with reasonable certainty, but for such faint objects spectra are not yet available, nor indeed are they likely to be until some major advance in technique can be recorded. The remaining hope is in the finding of Cepheid variables in the clusters. An empirical relation between period and luminosity is now well-established for these stars, so that the problem reduces to finding the period of light-variation of a cluster variable in the new objects.

Progress in this direction has been very recently reported from Mt. Wilson (*Publications of the Astronomical Society of the Pacific*, **51**, 40, Feb. 1939). At this observatory Baade and Hubble have the advantage of the use of the 100-inch reflector, of nearly three times the light-gathering power of the Bloemfontein instrument; but on the other hand, objects so far south as the new clusters culminate at rather low altitudes, so that observations are limited to a couple of hours a night through a great air-mass. At this stage, then, only tentative results are available, and although in the Sculptor system some 40 variables have been identified, no periods have been definitely determined. The observational work involved is of course very arduous, and establishing periodicities which may from star to star range from less than a day to more than two months is not a quick job. However, of the 40 variables, two are very bright, being at maximum as bright (17·8 mag.) as the brightest stars of the cluster, and have periods of the order of a week and amplitudes of the order of 1 mag. These are typical Cepheid variables, for which the period-luminosity curve predicts a median absolute magnitude of − 1·8. Since the median apparent magnitude is 18·3, the distance[1] is just over 100,000 parsecs. The remainder of the variables are much fainter (19·6 mag.) and are apparently typical cluster variables, *i.e.* short-period Cepheids resembling those found in globular clusters, with periods of about half a day. These stars are of absolute magnitude about 0, giving a distance of 84,000 parsecs. Thus it seems fairly certain that the Sculptor cluster is at a distance of about 300,000 light years, and is some three or four thousand light years in diameter. We are dealing here, then, with no globular cluster—these are mostly considerably nearer and some 50 times less in diameter; still less with a galactic open cluster—these are only a few thousand light years away and a few score of light years across. The Sculptor cluster is evidently an external galaxy, farther away than the Magellanic Clouds, and somewhat smaller than these, but not so far as the Andromeda nebula. Perhaps the most noteworthy feature of the system, on this view, is the absence of supergiants: the brightest members are only − 2·3 absolute magnitude. For this reason no hint of the presence of the system would be obtained on a photographic plate until the exposure reaches a critical value, beyond which the giants of the system will appear somewhat abruptly in great numbers. This feature had already been noted by Shapley before the Mt. Wilson results were available.

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[1] The absorbing matter in our galaxy is almost confined to a thin layer whose trace on the celestial sphere is within 5° or 10° of the centre of the Milky Way.

Study of the Fornax cluster shows that it is similar in essentials

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[1] The relation between apparent magnitude m, absolute magnitude M, and distance d is

$$m - M = 5(\log_{10} d - 1),$$

where d is expressed in parsecs. 1 parsec = 3·26 light years.

to the one in Sculptor; it is somewhat fainter overall, and its brightest members, too, are fainter, but these characteristics could be produced simply by greater distance. Once again the outstanding feature is the uniform distribution of very faint stars over a wide area of sky, with no nuclei and no diffuse nebulosity. The problem of its distance is, however, simplified by the fact that it contains at least two globular clusters. The uniformity amongst globular clusters has been remarked above; it is noticeable both in our own galaxy and in neighbouring external galaxies, so that we are justified in assuming that the brighter stars of these globular clusters in the Fornax system are of much the same luminosity as those of similar clusters elsewhere in space. If this is so, the Fornax system is placed at about 600,000 light years, twice as far away as that in Sculptor, and is of about twice the diameter, say 7000 light years.

To sum up, then, we are dealing here with two examples of a new stellar organisation whose features, though not unusual when considered one by one, have never previously been observed in combination; a transition system which may equally well be regarded as a super-cluster of the globular type but of galactic dimensions, as a close spheroidal galaxy, highly resolved but lacking in supergiants and of abnormally low surface brightness, or as a symmetrical Magellanic Cloud devoid of its open clusters and luminous nebulæ. These are all different ways of saying the same thing—in fact, of confessing the unique nature of the objects. Their discovery is the more important in that it is entirely possible that such objects are of common occurrence, their peculiar luminosity characteristics making it fatally easy to underestimate their frequency. The initial discovery of the Sculptor cluster was due to a freak combination of an abnormally sensitive emulsion and abnormally transparent observing conditions. It is in any case certain that if the clusters were four or five times farther away than they are, they would be awaiting discovery yet; are there others, then, beyond our present reach, and if so, how many? The distances of the clusters put them well within the "local group" of galaxies—a cluster of galaxies crammed into a volume a million light years in diameter ("crammed" because this is much smaller than the average distance between nebulæ). This local group now contains 11 members: our own galaxy, the Andromeda nebula and its two elliptical companions, both Magellanic Clouds, the spiral M33, two irregular nebulæ, and now these Sculptor-Fornax clusters. The importance of these new members lies in the fact that they are dwarfs; their discovery has equalised the number of dwarf and giant galaxies in our immediate neighbourhood. If our corner of space is typical, then, such dwarfs should be present in other clusters of galaxies. It may be that certain objects of low surface brightness in the Virgo super-galaxy are clusters of the Sculptor-Fornax type at such a great distance as to be unresolved. If so, we may yet again be presented with the exhilarating spectacle of a chance discovery on a photographic plate starting a train of investigation which ends by revolutionising current ideas of the evolution of the universe.

PHYSICS. By F. A. VICK, Ph.D., University College, London.

GAS TEMPERATURE MEASUREMENT.—It is well known that the measurement of the temperature of a gas, even of the air in a room, is decidedly not a simple matter, and yet it has to be attempted, often with little success, by physicists, chemists, physiologists, engineers and others in the course of their daily work. It is therefore worth while considering the excellent Symposium on Gas Temperature Measurement arranged by the Institute of Fuel, together with some of the other recent papers on the subject.

Let us first enquire, with J. G. Bennett and M. Pirani (*J. Inst. Fuel*, **12**, S.1, Mar. 1939) what is meant by "the temperature of a gas." This question is easy to answer when an ideal gas is in thermal equilibrium, *i.e.* when the average energy of each molecular degree of freedom is the same and proportional to the absolute temperature. It is then immaterial whether the temperature is measured by one energy "carrier" rather than another, and a variety of methods is available:

(*a*) Use of properties of the gas (*e.g.* pressure, refractive index, velocity of sound) which vary in a known manner with the mean energy of the molecules.

(*b*) Observation of the radiation emitted by the gas, the characteristics of such radiation being a function of the energy of the individual molecules.

(*c*) The behaviour of solids or liquids immersed in the gas, since, by the principle of equi-partition of energy, the atoms and molecules of the solid or liquid will acquire the same mean energy per degree of freedom as those in the gas.

Unfortunately, in many practical cases thermal equilibrium is not reached. For example, a discharge tube containing neon at 1 mm. pressure and with current density of 50 milliamps per sq. cm. gives a temperature measured in terms of electron mean energy of 15,000° C. The energy of excitation of the gas atoms corresponds to a temperature of about 10,000° C., and that of ionisation to about

12,000° C. On the other hand, the temperature increase of the unexcited atoms due to change in their translational energy is very small and a thermometer in the gas would indicate about 50° C. Thus we have in one gas four different "temperatures," each of which has a physical significance and technical importance. A second example, quoted by Bennett and Pirani, concerns combustion gases, composed principally of nitrogen, water vapour and carbon dioxide. The latter two gases absorb and emit infra-red radiation. Consequently, when the mixed gases pass through a water-cooled chamber, the water vapour and carbon dioxide are cooled more than the nitrogen, especially if the velocity of flow is large. The gases will tend to separate out into parallel streams, and thermal equilibrium, reached by diffusion and collision, will be slow in attainment. In these cases, and many others, the temperatures recorded by the different methods (*a*), (*b*) and (*c*) will be different. To distinguish between the various forms of energy and their corresponding temperatures, the term "Energy Flow Trap" was introduced many years ago (H. Kohn, *Ann. Phys.*, **44**, 749, 1914), and defined as follows: "In the absence of thermal equilibrium, one or more of the various energy carriers may be isolated or partially isolated from the others, with the result that the equipartition of energy between the different degrees of freedom will be prevented or delayed. In these cases, each class of energy carrier is called an Energy Flow Trap (E.F.T.)."

The E.F.T. systems are, in general:

(1) Kinetic energy of translation.
(2) Energy of excited states of atoms, molecules, or free radicals.
(3) Energy of rotation of molecules.
(4) Energy of vibration of the nuclei.
(5) Potential energy of modifications, such as those due to ionisation or dissociation.

With no thermal equilibrium, we are led to different values of temperature ("pseudo temperatures") by measuring the specific properties of the various energy flow traps. For example, in a sodium vapour street lamp, the pseudo temperature corresponding to the energy of translation of the unexcited atoms is about 100°–300° C., while that corresponding to the excited atoms is about 1000°–3000° C. (M. Pirani and A. Rüttenauer, *Das Licht*, **5**, 94, 1935.) The first is associated with the temperature of the glass walls, and the second with the intrinsic brilliance of the gas.

The above considerations are perfectly general. We will now consider the experimental methods of gas temperature measurement, and it is convenient to begin with the thermocouple. A. Schulze

(*Chem. Zeit.*, **62**, 308 and 485, 1938; *J. Inst. Fuel*, **12**, S.41, Mar. 1939) gives a survey of the materials used, beginning with the noble metals. The Le Chatelier couple, consisting of platinum and 90 per cent. platinum + 10 per cent. rhodium, is still the best between 300° C. and 1600° C. One very great advantage of this couple is its reproducibility, since the materials can be obtained with negligible impurities, and the alloy can be made accurately. It should be noted, however, that if kept for long periods above 1500° C., the volatility of the rhodium gradually changes the composition of the alloy. The high cost of the couple has led to a search for a suitable substitute. Pure palladium cannot be used, because of its absorption of hydrogen, which causes considerable variations in E.M.F., but addition of other noble metals to the palladium reduces this absorption. Two such alloys, known as "32" and "40," described in detail by O. Feussner (*Elektrotechn. Zeit.*, **48**, 535, 1927), when used together give much higher E.M.F.s than the Le Chatelier couple, and the price is about half, but they can be used only to about 1200° C. Like all noble metals, they must be protected by porcelain or other suitable sheaths against attack by hot vapours or gases containing carbon or silicon. The use of thermocouples above 1500° C. is beset with difficulties. Schulze (*loc. cit.*) gives a summary of a whole series of investigations of platinum alloys, some of which can be used up to 1900° C., but they all have the disadvantage of very low E.M.F.s. Feussner (*Elektrotechn. Zeit.*, **54**, 155, 1933) describes an iridium-rhodium alloy which, with pure iridium as the other wire, can be used up to 2000° C. At these high temperatures, magnesia, beryllia or thoria sheaths are used. Great care must be taken when inserting platinum thermocouples into hot gases containing hydrogen, since catalytic action may take place at the metal surface, raising the temperature of the latter.

For temperatures up to 600° C., constantan/Cu, constantan/Fe and constantan/Ag couples are still used, but have to be calibrated individually owing to the variation in composition of the constantan. For temperatures between 600° and 1100° C., various new base metal couples have found wide application, particularly alloys of chromium and nickel, which best resist oxidation at the higher temperatures. Schulze gives tables and curves for a number of these. Roeser and Dahl have published recently reference tables for constantan/Fe and constantan/Cu couples (*Bur. Stand. J. Res.*, **20**, 337, Mar. 1938).

The precautions which must be taken when measuring the E.M.F. of a thermocouple, such as attention to insulation, absence of induced currents and constancy of cold-junction temperature, are well known and need not detain us here. In view of the common industrial practice of using a millivoltmeter, it should be emphasised that considerable errors may result from changing resistance of couple and leads due to corrosion, and temperature variations of the leads. Also, the reading of the millivoltmeter will approximate to the E.M.F. only if the resistance of the instrument is high. If E is the E.M.F. of the couple, V the measured voltage, R_c the resistance of the couple and leads, R_v the resistance of the millivoltmeter, then

$$V = E\left[1 - \frac{R_c}{R_c + R_v}\right]$$

For all but rough work, it is better to use a null method. Even if a potentiometer is not desired, the couple, in series with a low resistance galvanometer, can be connected across a standard low resistance through which the current is varied until the galvanometer shows no deflection. The E.M.F. is then computed from an ammeter reading.

We come now to the special precautions necessary when using a thermocouple for gas temperature measurement. The indication of a simple thermocouple inserted in a hot gas depends on the temperatures of all the surfaces " seen " by the thermocouple, the diathermancy of the gas, the emissivity of the couple, the thermal conductivity of the leads, etc. Thus the temperature given by the couple cannot be taken to be the temperature of the gas surrounding it. If the gas temperature is fluctuating we have, in addition, a lag in thermocouple reading. If the gas is not in thermal equilibrium, the temperature computed when all these corrections have been taken into account is still a pseudo-temperature, which may or may not be the one required. Much the same considerations apply to a mercury or resistance thermometer inserted in the gas.

M. Fishenden and O. A. Saunders (*J. Inst. Fuel*, **12**, S.5, Mar. 1939) give curves and tables, based on experimental results, from which it is possible to estimate the errors due to radiation between thermocouple and walls. (There is a very useful table of emissivities.) They show that the errors are least for thin wires of low emissivity and when the gas velocities are high. Even then, the errors can amount to as much as 200° C. in 1500° C. The radiation errors can be reduced by surrounding the couple with a shield thermally insulated from it. If the gas is flowing past the couple, the shield is a tube coaxial with the direction of flow. Fishenden and Saunders deal, also, with estimates of rapidity of response of couples to fluctuating gas temperatures, and with the errors due to thermal conductivity along the leads, useful curves being provided for this purpose. Conduction along the leads is more important when the couple is sheathed. C. H. Bosanquet (*J. Inst. Fuel*, **12**, S.14, Mar. 1939) deals with similar problems, using a more theoretical method and with particular reference to gases flowing in tubes. Both papers leave us with the conclusion that above 500° C. the radiation from the couple to cool walls becomes so serious that no form of screening is of much avail in moderate pressure gases at ordinary rates of flow. It is then necessary to place the thermocouple (or thermometer) in an open-ended tubular shield through which the gas is aspirated to give a greater rate of flow. The theory and application of such "suction pyrometers" is given by A. Schack (*J. Inst. Fuel*, **12**, S.30, Mar. 1939). One very interesting and accurate method is due to Wenzl and Schulze. The thermocouple, in its sheath, is surrounded by a protective tube which can be heated electrically to or above the gas temperature. When equilibrium has been attained, the gas to be measured is drawn through the annular space between the thermocouple sheath and heated tube. If the couple temperature rises, the heating has been insufficient, while if it falls the heating has been excessive. The true gas temperature is found by interpolation, and can be used to calibrate other instruments. The electrically heated instrument is perhaps the most accurate form of thermocouple, but it is not suited to measuring gas temperatures below that of the surroundings, it cannot be used above 1150° C. owing to the breakdown of insulating materials, etc., and the necessity of interpolation makes automatic recording difficult. The use of simpler types of suction instruments is discussed by Schack. Another interesting variation is described by B. M. Larsen, G. Siddall, and K. Heindihofer (*J. Inst. Fuel*, **12**, S.55, Mar. 1939), in which a refractory brick is used as a radiation shield, with small channels for the flow of gas. The brick has small conductivity, a low thermal capacity, and, owing to the cellular structure, is equivalent to a very large number of thin-walled concentric radiation shields.

Sufficient has been said to indicate the precautions necessary to obtain an approximation to the true temperature of a gas by means of a thermocouple. Many of the above considerations apply equally well to other types of thermometer. Some of these thermometers are discussed in other papers in the symposium. There is also a particularly valuable paper on the measurement of temperatures of flames by G. Ribaud, Y. Laure and H. Gaudry (*J. Inst. Fuel*, **12**, S.18, Mar. 1939).

METEOROLOGY. By E. V. Newnham, B.Sc., Meteorological Office, London.

TEMPERATURE IN RELATION TO PLANT GROWTH AND SURVIVAL.—The G. J. Symons Memorial Lecture was given by Professor E. J. Salisbury on April 19, 1939, and is published in the *Quarterly Journal of the Royal Meteorological Society* for July 1939 under the title "Ecological Aspects of Meteorology." The lecturer considers first the differing way in which even closely allied species are affected by meteorological conditions, and in particular by extremes of temperature, according to differences in their life histories. These considerations show how little can be expected from such methods as those of De Candolle, and others who have followed him, for dealing with the factor of temperature by summation above an arbitrary minimum—of which the "accumulated temperature" of agricultural meteorology is an example. The complications of the problem are enhanced by the fact that each physiological process essential to plant life has its own relationship to temperature even in a single species. In general the temperature best suited to simple vegetative growth is lower than that which best suits the reproductive processes of flowering and fruiting, but exceptions are common among plants that flower in the spring, snowdrops and crocuses furnishing examples when grown in a warm room, for vegetative growth is then vigorous yet flowers often fail to develop.

Another difficulty encountered when dealing with the influence of temperature is that particular physiological processes are apt to be influenced only by the temperature at a particular stage of the life history of the plant, for example the buds of the garden hyacinth are laid down in August and the optimum temperature is about 78° F., while the buds of the daffodil are formed in May and require a much lower temperature. The temperature in these months, therefore, has to be taken into account as well as that of the month of actual flowering when considering the influence of temperature. For a species to survive it is generally necessary to have a suitable temperature for the growth of the pollen tubes to the ovules, and the interval between pollination and fertilisation may be so long that seed formation requires that temperature shall reach a certain minimum level in two consecutive years at particular seasons. In the case of the Scots pine, pollination takes place in May of one year and fertilisation in June of the following year. The northern limit of this species is therefore set by the growing infrequency of occurrence of two consecutive years with the minimum temperature requirement in early summer for seed production, and not by the mean summer temperature. Even if seeds are produced, they may not germinate owing to the absence of the required temperature conditions, and these are not always confined to the occurrence of a long enough period of sufficient warmth at the appropriate season for germination; some seeds will not germinate unless temperature fluctuates sufficiently.

Coming now to the important problem of food production, the gain by the plant will be greatest at that temperature at which the rate of food production by photosynthesis is greatest relative to the loss by respiration. At low temperatures the loss by respiration may be the greater of the two and at very high temperatures the rate of respiration rises more rapidly with increase of temperature than does the rate of food production. The optimum temperature will be at some intermediate point, but will be different for different plants. For the common blackberry it is about 61° F., for the wood anemone about 59° F. and for the bean about 81° F. In this connection it is essential to take into account the temperature of the air immediately surrounding the actual plant, and not just the temperatures principally recorded by meteorologists, *i.e.* those registered in a louvred screen 4 feet above the ground and over short grass in an open position; that is to say we are concerned with the microclimate of the plant. Without this precaution one is confronted with apparent anomalies such as the development of abundant foliage in woodland plants such as dog's mercury, and the lesser celandine by the beginning of February and in the wild hyacinth and wood anemone by the end of the month, in spite of the fact that the optimum temperatures for food production are about 30 degrees above the average daily *maximum* of temperature for that month in the case of these plants. The explanation of the apparent anomaly lies in the peculiarly favourable conditions prevailing at that season close to the ground in deciduous woodland. The trees afford shelter from the wind, yet admit sunshine; the loose litter on the ground is a bad conductor of heat and does not permit the heat which it absorbs so readily because of its dark colour from being conducted quickly to the cold soil beneath, with the result that food production may be taking place under optimum temperature conditions when temperature as ordinarily measured in a screen is very much lower. Firbas found a difference of temperature of nearly 80° F. between the litter at 2 cm. depth in oak woods and the air at a height of 1 metre. A somewhat similar state of affairs is found on the landward side of the sand dunes of the Norfolk coast. In April the dunes of medium age are carpeted with winter annuals at their most active stage of growth, and the sand surface is then found to be generally at least 5° F. warmer than the air 2 feet above it, and sometimes reaches over 80° F. In the older dunes, the sand of which is darker on account of the greater humus content, temperatures of 100–120° F. have been observed on the sand surface.

Yet another illustration of the connection between the seasonal distribution of the physiological processes of a species and its climatic tolerances is furnished by two closely allied woodland species of wild arum or cuckoo-pint, *Arum maculatum*, and the much rarer *Arum neglectum*. The first winters 9 inches below the litter-covered surface as a corm, safe from all but the most prolonged frosts. The new leaves, which are formed from the food reserves in the corm, emerge above the ground about the middle of February and usually wither away late in June. *Arum neglectum*, on the other hand, produces leaves early in December, and these do not wither until the following September. It has therefore a photosynthetic period of about nine months, during more than half of which the deciduous trees and shrubs overhead are not in leaf, and therefore do not obstruct the light. This advantage is gained at the cost of greater susceptibility to frost, and a map of the distribution of the two plants shows clearly this weakness, for *Arum maculatum* is found throughout England, Wales and Ireland, in southern Scotland and even in Caithness, but *Arum neglectum* hugs the southern or south-western coastal regions of England and Wales. An analogous case is furnished by the restricted distribution of *Scilla autumnalis* compared with that of *scilla verna*, which may be attributed at least in part to the greater winter exposure of the leaves of the former, the leaves of *S. verna* appearing only in spring.

Some summer annuals are safe from frost because the seeds do not germinate until the Spring, but are limited in their northward extension by a lack of sufficient autumn warmth for the ripening if not for the formation of seeds, whereas the winter annuals mature earlier and, in consequence of this, and of their germinating in the autumn, are susceptible to winter frost and not to lack of autumn warmth. Some summer annuals show a very good correspondence between their distribution and the area over which the mean daily maximum in September exceeds some minimum value. Southern Continental species of annuals have yet other temperature requirements, being able to survive severe frost, but needing high average daily maximum temperature over the three months August–October. The north-western limit of the mean of 62° is a fair approximation to the north-western limit of distribution of such species considered in the mass.

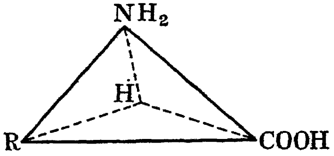
The persistence of some species, notably the rare storks-bill, *Erodium moschatum*, which is a native of south-west Europe, is conditioned largely by the inability of the plant to survive more than a certain severity of frost. For such species the mean minimum temperature is of no importance, but only the extreme minimum, which need occur only once to have lethal effects, because all the seeds germinate at the same time. When germination tends to occur in batches from autumn to spring, there is a less simple reaction to temperature.

The conclusion to be drawn is that temperature variations must be considered in detail, more attention being paid to extremes than to means, and in close relation to the life processes of individual species of plant, before the significance of temperature for plant life can be properly assessed. As the lecturer remarked, " the relations of temperature and vegetation are in fact more individual than general, though it is true that certain biological types sometimes prevail in specialised climatic conditions."

BIOCHEMISTRY. By W. O. Kermack, D.Sc., LL.D., F.R.S.E., Research Laboratory, Royal College of Physicians, Edinburgh.

The Amino Acids of Protein from Cancerous Tissue.—The problem of cancer is one aspect of the more general problem of organic growth. In the wild uncontrolled multiplication of tumour cells, something is missing which is required to co-ordinate and restrain the growth and bring it into harmony with its surroundings. Investigation of the factors which regulate normal development is clearly a line of approach to the problem of cancer. It is therefore not surprising that Kögl, famous for his brilliant researches on auxin, the compound which plays so important a role in plant growth, and of biotin, required in extraordinarily minute amounts for the growth of yeasts and of various bacteria, should have turned his mind to the problem of abnormal growth such as is presented by cancer. The first detailed communication by Kögl and Erxleben on this subject claims to demonstrate a simple but fundamental chemical distinction between the cancer cell and all normal plant and animal tissue. If this claim is substantiated it would seem that a new era in cancer investigation will have been opened up.

The new discovery is in a perhaps rather unexpected field, that of optical activity. The amino acids which are obtained by the hydrolysis of proteins contain an asymmetric carbon atom, with the exception of glycine, the simplest of them all, the molecule of which has a plane of symmetry and is therefore optically inactive. The other amino acids are theoretically capable of existing in two forms which are mirror images of each other and which have opposite effects on a beam of plain-polarised light transmitted through them. One rotates the plane of polarisation to the right, the other to the left. Now it is found that all the naturally occurring amino acids belong to the *l*-series. This does not mean that they all rotate the plane to the left. To appreciate what it does mean we must think of the molecule of an amino acid as a carbon atom with four different groups attached to it in tetrahedra fashion, a hydrogen atom, an amino group, a carboxyl group and another group R characteristic of the acid in question.



To say that all the acids belong to the same series means that they are all obtained from each other by altering this fourth group R, the positions of the other three remaining unchanged. The effect of any such change on the optical activity is not readily predictable. Particular acids belonging to a series of this kind may rotate the plane of polarisation to the right or to the left, but because of their fundamental affinity with each other it has been found convenient to describe them all as belonging to the *d*- or *l*-series, the initial denoting the actual direction of rotation (dextro- or lævo-) of a key compound (*l*-glyceric aldehyde), to which it has been agreed to relate the whole series.

Kögl and Erxleben quote two exceptions to the generalisation that all naturally occurring amino acids belong to the *l*-series. One of the products of hydrolysis of the alkaloid ergotamine is proline, and this has been shown by Jacobs and Craig (*J. Biol. Chem.*, 1935, **110**, 522) to belong to the *d*-series. The second known exception was brought to light by Ivanovics and Bruchner (*Naturw.*, 1937, **25**, 250 ; *Z. Immunitätsforschung*, 1937, **90**, 304), who isolated from the protective capsule of the cholera bacillus a polypeptide-like substance consisting largely of *d*-glutamic acid.

Quite a large number of polypeptides containing amino acids of the *d*-series have been prepared synthetically. These have been found to differ from the ordinary naturally occurring amino acids in one important respect—they are resistant to the action of the polypeptide-splitting enzymes.

In view of the great specificity of enzymes this is not surprising, but clearly it implies that though the ordinary chemical properties of optical isomers are identical, their biochemical properties may be vastly different. The existence of a *d*-amino acid in a protecting covering of a bacterial cell may confer on that layer an immunity to the action of degradative enzymes which might otherwise penetrate the defences.

The essential fact revealed by the experiments of Kögl and Erxleben is that cancer tissue protein contains a considerable proportion of its constituent amino acids in the form of the *d*-stereoisomers. Some of the results are summarised in Table I. It will be seen that whereas normal tissue yields only acids of the *l*-series, appreciable quantities of certain amino acids isolated from tumour protein belong to the " unnatural " *d*-series. In the case of glutamic acid, the proportion may amount to as much as 40 to 45 per cent. The amino acids arginine, histidine, *iso*-leucine, tyrosine, oxyproline and aspartic acid were isolated from various tumours but were found to be present wholly or almost wholly (over 98·5 per cent.) in the *l*-form.

TABLE I

Amino Acids.	Per cent. of *d*-acid in:							
						Human.		
	Normal Calf-lung.	Various Normal Rabbit Tissue.	Normal Ovaries (Human).	Rabbit Net-tumour.	Mouse Tumour.	Ovarian Tumour. I.	Ovarian Tumour. II.	Mammary Carcinoma.
Glutamic Acid . .	—	0	0	44·5	—	42·7	15·6	31·7
Leucine . .	0	—	—	4·5	4·2	7·2	—	—
Lysine . .	0	—	—	4·5	4·5	3·8	—	—
Oxyglutamic Acid . .	0	—	—	—	3·1	3·1	—	—
Phenylalanine	0	—	—	0	0	1·1	—	—
Valine . .	0	—	—	2·3	3·1	3·3	—	—

One of the possibilities which has to be guarded against in work of this kind is that the chemical methods employed for the hydrolysis of the proteins and isolation of the amino-acids may be too drastic and may partially convert the natural *l*-acids into the *d*-stereoisomers. In these circumstances the results would of course have no significance, the " unnatural " acid detected being simply an artefact. Obviously the best method of controlling this possible fallacy is to apply exactly the same method of analysis to normal as to cancerous tissue. It was, in fact, found that in the case of the three amino acids, alanine, serine and proline, some optical inversion occurred during the hydrolysis and subsequent chemical treatment. These acids have therefore been omitted from the table. The fact that in the case of the other acids the same methods applied to normal tissues give only the *l*-stereoisomers, together with the acknowledged experimental skill and scientific ability of Professor Kögl and Fräulein Erxleben, discounts any such simple explanation of the new results. At the same time attention should be drawn to a communication by Chibnall, Rees, Tristram, Williams and Boyland (*Nature*, 1939, **144**, 71) who have isolated glutamic acid from various tumour tissues but in no case found any appreciable admixture of the " unnatural " *d*-form. Kögl and Erxleben suggest (*Nature*, 1939, **144**, 111 ; *Naturw.*, 1939, **27**, 486) that the failure to confirm their results may be due to the fact that Chibnall and co-workers isolated glutamic acid by a method likely to lead to their overlooking the *d*-component.

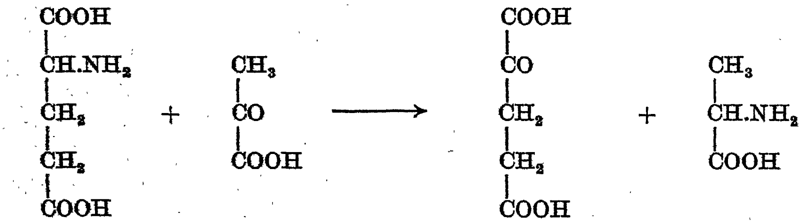
It is interesting to observe that the idea that the proteins of cancer tissue might possess unnatural stereochemical configuration is not altogether new, for as long ago as 1907 Dr. John Beard, an Edinburgh Zoologist, suggested that " the albumins of cancer and of malignant tumours in general must be dextro-bodies " (cf. *Medical Record*, 1913, March 29). However, this suggestion does not seem to have been taken very seriously, and Kögl does not appear to have been aware of it.

If we assume that the results of Kögl and Erxleben are essentially correct, it seems clear that a most important attack has been initiated on the cancer problem. If the proteins of tumour tissue contain in their polypeptide chains amino acids of the " unnatural " series, then from the work referred to above, it follows that these proteins will be largely immune from attack by the ordinary protein-splitting enzymes of the body. It is not surprising that cells so constituted should be free from control by neighbouring tissue and should be able to grow without restraint. Many of the interesting points which the new discovery raises are discussed in the original paper.

ROLE OF GLUTAMIC ACID IN AMINO ACID METABOLISM.—It will be observed that of all the amino acids it is glutamic acid which is present to the largest extent in the " unnatural " form. Now it happens that within the past few years several quite distinct lines of work have brought to light the importance of glutamic acid in the general metabolism of amino acids and proteins. A brief account of some of this work may be of interest, especially as a knowledge of it is required in order to appreciate the significance of Kögl and Erxleben's results.

First of all we may refer to the contribution recently made by Schoenheimer and co-workers. This is an interesting application of the use of isotopes in the investigation of biochemical problems. It is now possible to prepare compounds such as ammonia and amino acids containing ^{15}N (*i.e.* with atomic weight 15 instead of 14) in excess of the small proportion (0·3 per cent.) present in ordinary atmospheric nitrogen. Such compounds containing an excess of ^{15}N are for all ordinary chemical and biochemical purposes indistinguishable from similar compounds containing ordinary nitrogen, but the excess, as it were, labels the nitrogen and allows it to be identified and the course of its metabolism to be followed. For instance (Foster, Schoenheimer and Rittenberg, *J. Biol. Chem.*, 1939, **127**, 319), two growing rats on a low protein diet to which a large amount of labelled ammonia (as citrate) was added were killed after some time and various amino acids isolated from their tissues. All with the exception of lysine contained some labelled nitrogen, but the highest amount was found in glutamic acid, and the second highest in the other dicarboxylic acid, aspartic acid. This shows that the ammonia administered was utilised for amino acid synthesis, and that the dicarboxylic acids and especially glutamic acid were chiefly concerned in such synthetical activity. Somewhat similar results were obtained in another experiment in which tyrosine containing labelled nitrogen was administered in the diet (Schoenheimer, Ratner, Rittenheimer, *J. Biol. Chem.*, 1939, **127**, 333).

Another series of experiments, this time by Braunstein and Kritsman (*Nature*, 1937, **140**, 503; *Enzymologia*, 1937, **2**, 129 and 147), shed light on the details of the manner in which glutamic acid may function. The authors have discovered a special type of exchange reaction which takes place in presence of minced muscle and other tissues. They find, for example, that glutamic acid and pyruvic acid form α-ketoglutaric acid and alanine according to the equation



$$\begin{array}{c}\text{COOH}\\|\\\text{CH.NH}_2\\|\\\text{CH}_2\\|\\\text{CH}_2\\|\\\text{COOH}\end{array} + \begin{array}{c}\text{CH}_3\\|\\\text{CO}\\|\\\text{COOH}\end{array} \longrightarrow \begin{array}{c}\text{COOH}\\|\\\text{CO}\\|\\\text{CH}_2\\|\\\text{CH}_2\\|\\\text{COOH}\end{array} + \begin{array}{c}\text{CH}_3\\|\\\text{CH.NH}_2\\|\\\text{COOH}\end{array}$$

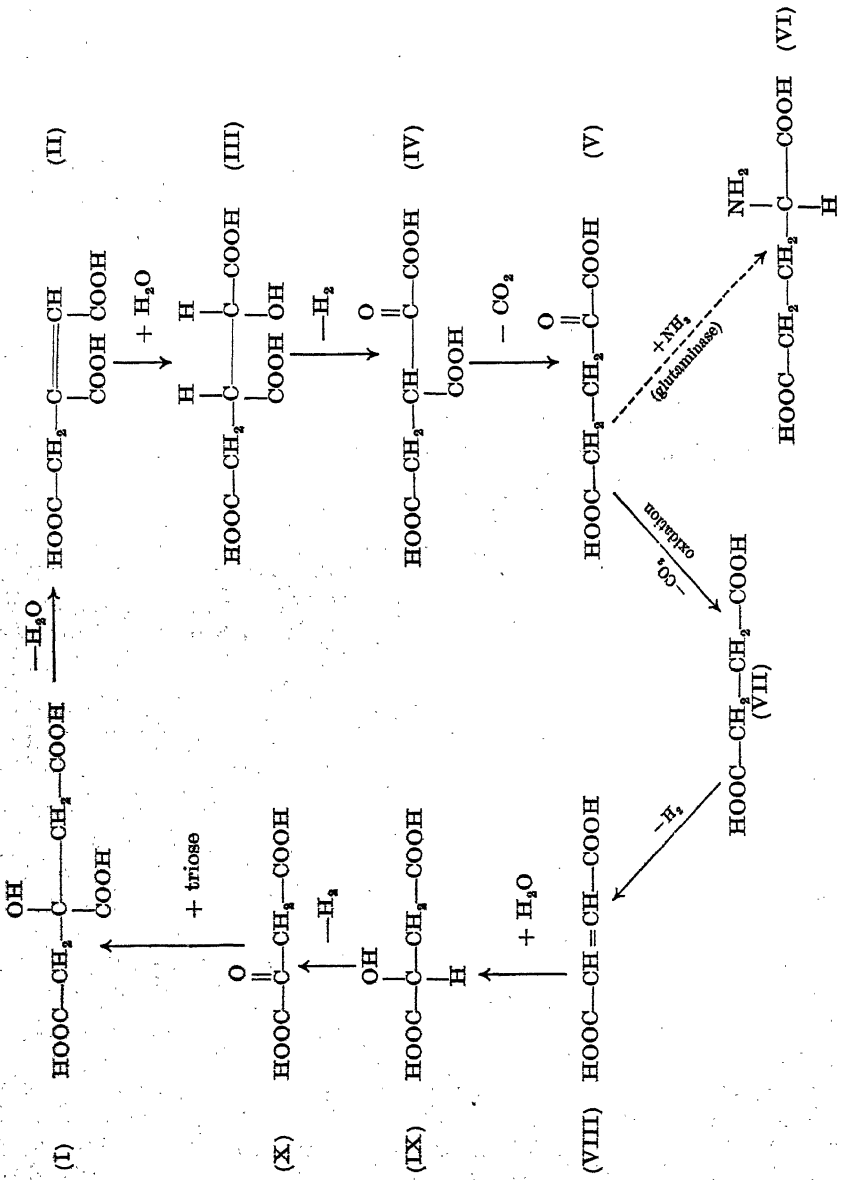
Glutamic Acid Pyruvic Acid α-ketoglutaric Acid Alanine

The reaction involves the transfer of ammonia from one compound to the other accompanied by a simultaneous oxidation-reduction. The reaction would seem to be one of considerable generality, a wide variety of α-keto-acids reacting with *l*-glutamic acid with the formation of the corresponding amino acid, and indeed the place of *l*-glutamic acid itself may be taken by the other naturally occurring amino acid, *l*-aspartic acid. Furthermore, the reaction is a reversible one so that glutamic acid acts as a nitrogen carrier, exercising a very important function in the synthesis and degradation of amino acids in general. It at once becomes clear why so much of the labelled nitrogen in the ingested tyrosine in the experiments of Schoenheimer found its way into the glutamic and aspartic acid molecules, and also why the high percentage of " unnatural " glutamic acid in cancer tissue may very profoundly alter the behaviour and metabolism of the cancer cell.

But glutamic and α-ketoglutaric acids play an important role not only in the biochemistry of amino acids and proteins ; it seems that they also form an important link with the carbohydrates and so occupy a key position in cell metabolism. A third series of investigations, which we owe largely to Krebs and his colleagues, has opened up this aspect of the subject. In order to obtain some notion of this work we must first concentrate our attention on citric acid.

Citric Acid Cycle and Cell-Oxidation.—At first sight citric acid would seem to be related neither to carbohydrates nor to amino acids. Its importance in cell metabolism, however, was shown by Krebs and Johnson (*Enzymologia*, 1937, IV, 148), who found that oxidation by muscle tissue was catalysed by the addition of citrate as well as by other compounds such as oxaloacetate and -succinate. The importance of certain dibasic acids containing four carbon atoms had already been demonstrated by Szent Györgyi and his school (*Z. physiol. Chem.*, 1937, **247**, I), whilst Martius and Knoop (*Z. physiol. Chem.*, 1937, **246**, I, and **247**, 104) had shown that in presence of the appropriate liver enzyme citric acid is converted into α-ketoglutaric acid. The details of all this work are complicated and highly specialised, but the general conclusion is as follows.

Citric acid (I) is converted in the cell through *cis*aconitic acid (II) into *iso*citric acid (III) which is oxidised to oxalosuccinic acid (IV). By the loss of carbon dioxide this readily forms α-ketoglutaric acid (V), which may either react with ammonia to give *l*-glutamic acid (VI)—a reaction which proceeds in presence of the enzyme glutaminase investigated by Euler, Adler, Günther and Das (*Z. physiol. Chem.*, 1938, **254**, 61)—or be oxidised to succinic acid (VII). The cell is known to contain an enzyme which promotes the oxidation of succinic acid to fumaric acid (VIII), which by the addition of water yields malic acid (IX), a compound which on further oxidation is converted into oxalacetic acid (X). Krebs



CITRIC ACID CYCLE

suggests that this oxalacetic acid now combines with a carbohydrate derivative probably containing three carbon atoms, and for convenience referred to as "triose," and that after further oxidation a new molecule of citric acid is formed, ready to pass through all these reactions once more and so continue the cycle.

This citric acid cycle is fed by carbohydrate in the form of "triose," and its products are carbon dioxide and water. In other words, it is a route for the oxidation of carbohydrate and may, in fact, prove to be the chief one utilised in the cell. (For objections to the citric acid cycle see Breusch, *Z. physiol. Chem.*, 1937, **250**, 262.)

A paper by Krebs, Salvin and Johnson (*Bioch. J.*, 1938, **32**, 113) provides evidence that the reactions involved in the citric acid cycle do actually occur in the intact animal body. Extending some observations of Orten and Smith (*J. Biol. Chem.*, 1937, **117**, 555), they show that if succinic, *l*(-)malic, fumaric or oxaloacetic acid be administered to rats or rabbits, citric acid appears in the urine. On the other hand, after the administration of citric acid the urine is found to contain α-ketoglutaric and succinic acids. These observations are readily understood if we assume that the citric acid cycle is actively functioning in the animal body.

Amongst the many observations made by Krebs and his co-workers on the citric acid cycle, one is perhaps of special significance. Krebs and Eggleston (*Bioch. J.*, 1938, **32**, 913) have shown that the oxygen uptake by minced pigeon breast muscle, suitably diluted, and containing an adequate supply of muscle co-enzymes and of compounds of the citric acid—oxalacetic acid cycle, is materially promoted by the addition of insulin. It would seem that here we have a direct demonstration of the action of insulin in vitro. The importance of this result may be gauged from the fact that in spite of very many investigations the only action of insulin in vitro so far demonstrated has been an inhibition of gluconeogenesis and of urea formation from amino acids in liver (Bach and Holmes, *Bioch. J.*, 1937, **31**, 89). This effect, however, would seem to be a secondary one, and does not reveal the fundamental action of insulin in the organism. Krebs and Eggleston adduce evidence that in their experiments the effect of insulin is on the citric acid cycle. There is strong reason for believing that this cycle provides the main route for the oxidation of carbohydrate, and so it is possible that these experiments of Krebs and Eggleston may lead to a solution of that so far very mysterious problem, the primary function of insulin in tissue metabolism.

During the last ten years very important advances have been made in our knowledge of the anaerobic breakdown of carbohydrate in muscles, thanks largely to the work of Embden, Meyerhof, Lohmann and associated workers (*cf.* SCIENCE PROGRESS, 1934, **28**, 682). Normal muscle metabolism is, however, aerobic, and interesting though the anaerobic mechanism may be, it is of the utmost importance to obtain some detailed insight into the mechanism of oxidative degradation in the animal cell. It has always been realised that part of the chemical system may be common to both aerobic and anaerobic routes, and indeed it is a possibility that lactic acid or pyruvic acid, the end-products of the anaerobic breakdown of carbohydrate, might be first formed and these then oxidised to carbon dioxide and water. The Krebs citric acid theory suggests that it is only the first part of the already known Embden-Meyerhof scheme that is applicable to the aerobic breakdown, namely the phosphylation of the carbohydrate and its splitting into two triose units. The possibility, however, does not seem to be excluded that the oxalacetic acid condenses not with triose, but with pyruvic acid or phosphopyruvic acid. In this case almost the whole of the anaerobic cycle would be involved in the aerobic one. Further work may of course reveal other chemical pathways of oxidation, and there is in addition the whole problem of the linking of fat and carbohydrate metabolism which is still shrouded in mystery. In other words, the carbohydrate derivatives designed by Krebs as "triose" may not really be triose in the strict sense but some other of the numerous 3-carbon-atom compounds included in the Embden-Meyerhof scheme. This point will no doubt be elucidated in the near future.

GEOLOGY. By G. W. TYRRELL, A.R.C.Sc., D.Sc., F.R.S.E., The University, Glasgow.

GEOLOGY AND PETROLOGY OF IGNEOUS ROCKS—AFRICA, NORTH AND SOUTH AMERICA, AUSTRALIA, ANTARCTICA, AND OCEANIC ISLANDS.—In a paper on "Igneous Rocks from the Abercorn and Kasama Districts, Northern Rhodesia," T. Deans (*Geol. Mag.*, LXXV, 1938, 547–58) describes a granodiorite—appinite suite of Pre-Cambrian age, with a series of older porphyrites which may be early members of the same suite. A large mass of hypersthene-gabbro is also described.

The "Petrology of Some Uganda Granites, with special reference to those associated with economic minerals," is dealt with by W. C. Simmons (*Geol. Surv. Uganda, Bull. No.* 3, 1939, 114–39). The characters of numerous occurrences are summarised, and the author demonstrates that the granites of the Ankole metallogenetic tin province possess distinctive characteristics. There was an enrichment in potash towards the end of the crystallisation period, resulting in sericitisation of earlier oligoclase and orthoclase. These solutions also brought in muscovite, cassiterite and quartz.

N. L. Bowen has studied the " Lavas of the African Rift Valleys and Their Tectonic Setting " (*Amer. Journ. Sci.*, XXXV–A, 1938, 19–33). Earlier work suggested that a definite correlation might be made between the chemistry of the lavas and their tectonics. The Eastern Rift Valley, thought to have been produced by tensional stresses, has associated with it lavas of soda-rich character; while the Western Rift Valley, believed to have been formed as a result of compressive stresses, has potash-rich lavas associated with it. Bowen shows that there are many exceptions to these tectonic and chemical generalisations, and cannot find any convincing relation between the chemistry of the lavas and their tectonic conditions.

The paper " Petrographic Description of Volcanic Rocks from Turkana, Kenya Colony, with Notes on their Field Occurrence from the MS. of Mr. A. M. Champion," by W. Campbell Smith (*Quart. Journ. Geol. Soc.*, XCIV, 1938, 507–54), deals with a series of rhyolites, trachytes, phonolites, tephrites, trachybasalts, basalts, basanites and nephelinites, of which the ages are not certainly known. In the southern part of the province the lavas are comparable with those of Tertiary age in other parts of Kenya. In the north, basalts and trachybasalts are succeeded by thick flows of trachyte and rhyolite, and this series may be altogether different in age. Within the region described there are many small volcanic plugs, dikes and larger intrusions, comprising alkaline types such as micro-foyaite, malignite, teschenite, etc.

The " Igneous Rocks from the Neighbourhood of Mount Jombo and the Sabaki River, B.E.A.," described by G. W. Tyrrell and the late Miss A. T. Neilson (*Geol. Dept. Hunterian Mus., Univ. Glasgow, Mon. No.* 5, 1938, 108–16), are found within a small volcanic plug or ring-dike series about one mile in diameter (Mt. Jombo). The core of the hill is composed of coarse nepheline-syenite. This is surrounded in succession outwards by a narrow ring of basic alkaline rocks including ijolite, malignite and melteigite, a broad ring of nepheline-sodalite-syenite, and a second narrow ring of basic alkalinites. The exterior contact zone shows indications of a third ring of nepheline-syenite composition. Mount Jombo is the focus of a radial dike-assemblage of like composition.

In his review " Eruptivos afro-brasileno-argentinos de edad permo-triaso-eojurasica " K. Walther (*Bol. Inst. Geol. Uruguay,* XXIV, 1938, 117–55 (Spanish text), 155–8 (German summary)) shows that the Late Trias to Jurassic Gondwana basalts and dolerites of South America and of the southern half of Africa are often closely associated with feebly-alkaline types, and occasionally with peralkaline rocks, including nepheline-syenite, tinguaite, etc. The question as to whether the alkaline rocks, which are later in time, are differentiates of the Gondwana magma, or represent a completely different magmatic sequence, is left open in the lack of decisive evidence.

In his memoir " Geological Investigations in Southern Greenland. Part I. On the structural Divisions of Southern Greenland " (*Medd. om Grönland*, **113**, No. 2, 1938, 148 pp.), C. E. Wegmann proposes a new working hypothesis for the Ilimausak batholith of highly alkaline rocks made famous by the classic investigations of Ussing. Ussing used the hypothesis of gravitative differentiation to explain the arrangement of the different magmatic types; Backlund, later, favoured Fersmann's hypothesis of " agpaitic " differentiation. Wegmann, however, believes that the massif is due to a combined igneous and metamorphic action. The Gardar formation, consisting of sandstones, porphyry lavas, tuffs and agglomerates, was intruded by a series of abyssal and syntectic magmas ranging from essexite to arfvedsonite-granite. These rocks, as well as the wall-rocks, were subsequently transformed into highly-alkaline types by the imbibition of fluids accumulated in the essexite magma. The volume increase thus produced caused a sagging of the mass with partial folding, in which the lujaurite behaved like a crystalline schist, and acquired its characteristic banded structure. Wegmann calls this the " migmatitic " hypothesis, and admits that its confirmation must await detailed investigation.

The paper on " Petrology and Structure of the Franconia Quadrangle, New Hampshire," by C. R. Williams and M. P. Billings (*Bull. Geol. Soc. Amer.*, **49**, 1938, 1011–44), deals with a closely-folded region of Early Palaeozoic and Devonian sediments and volcanic rocks which has been invaded by two magma-series; the subalkaline New Hampshire magma-series, probably of Late Devonian age; and the alkaline White Mountains magma-series, probably of Mississippian age. The New Hampshire series is mainly syntectonic, having been injected during the later stages of an orogenic episode. The White Mountains series, however, is post-tectonic, its various members forming ring-intrusions and stocks.

According to the detailed measurements by A. E. Sandberg, the

" Section across Keweenawan Lavas at Duluth, Minnesota " (*Bull. Geol. Soc. Amer.*, **49**, 1938, 795–830) has a total thickness of 39,482 feet, comprising 20,856 feet of bedded lavas (mainly basalts with subordinate rhyolites) and related sediments, 4,126 feet of diabase sills, and 14,500 feet of the Duluth gabbro lopolith. The lavas are fissure flows of the plateau (or flood) type, the axis of eruption of which coincided with the axis of the present Lake Superior basin. There are more than 225 separate flows. About 50 basaltic dikes were observed, mostly in the lower half of the section, but there is no evidence that the dikes were the feeders of the flows.

The " Bell River Complex, North-western Quebec," described by B. C. Freeman (*Journ. Geol.*, XLVII, 1939, 27–46), consists mainly of anorthosite and norite, and is believed to represent a deformed lopolith of the same type as the Bushveld (Transvaal) and Stillwater (Montana) complexes. The rocks are severely altered by serpentinisation, hornblendisation, and chloritisation. Serpentinisation is believed to have been caused by internal deuteric processes, but the other changes are probably due to hydrothermal solutions derived from a great dike of quartz-diorite.

F. F. Grout has assembled the " Petrographic and Chemical Data on the Canadian Shield " (*Journ. Geol.*, XLVI, 1938, 486–504), with no fewer than 39 new chemical analyses of granites and gneisses. The data have been used to calculate a new average of the igneous rocks of the Canadian Shield (494 analyses), as well as several other averages of petrological importance.

In their comprehensive paper " Petrologic Results of a Study of the Minerals from the Tertiary Volcanic Rocks of the San Juan Region, Col.," E. S. Larsen, J. Irving, F. A. Gonyer, and E. S. Larsen, 3rd (*Amer. Mineralogist*, **21**, 1936, 677–701 ; **22**, 1937, 889–905 ; **23**, 1938, 227–57, 417–29) have studied the common rock-forming minerals of a series of calc-alkaline lavas ranging from rhyolite to basalt. Quartz, tridymite and cristobalite occur in nearly equal amounts. The most important petrological conclusion is that many of the plagioclases and other phenocrysts of the San Juan lavas did not crystallise from a magma of the composition of the rock in which they are found, but must have been derived from the partial assimilation of other rocks, or by the mixing of two magmas, one or both of which carried crystals. The uniform distribution of the phenocrysts in widespread lava flows and in groups of flows, shows that there must have been a thorough mixing of very large masses of magma.

An interesting study of the " Laramide Igneous Sequence and Differentiation in the Front Range, Col." has been made by

T. S. Lovering and E. N. Goddard (*Bull. Geol. Soc. Amer.*, **49**, 1938, 35–68). The igneous rocks were connected with the gradual structural failure of the crust during the Laramide Revolution. The main types are porphyries, andesites and granodiorites. The igneous succession suggests the gradual fusion of a deep-seated crystalline sub-stratum. Under the western part of the range the initial magma approximated to augite-diorite, but in the eastern part to olivine-gabbro. The latter gave rise to more alkalic differentiates than the augite-diorite magma.

According to R. H. Jahns, in his paper on " Analcite-bearing Intrusives from South Park, Col." (*Amer. Journ. Sci.*, XXXVI, 1938, 8–26), sills and dikes of analcite-diabase and analcite-syenite cut Cretaceous sandstones and are probably of Laramide age. The analcite is considered to be only partly of pyrogenetic origin. Three mineral reaction-series are described ; all point to the crystallisation of a hydrous alkaline magma at low temperatures, with development of a richly-sodic residual liquid. Certain features suggest that the larger bodies are of composite structure, the analcite-syenite having been injected into not quite solidified diabase.

C. N. Fenner describes the remarkable " Contact Relations between Rhyolite and Basalt on Gardiner River, Yellowstone Park " (*Bull. Geol. Soc. Amer.*, **49**, 1938, 1441–84), where a rhyolite lava follows a small valley eroded in a basaltic surface. The rhyolite has penetrated deeply into the basalt as complex networks of veins and dike-like bodies, and has exerted a strong corrosive action on the basalt. Basaltic constituents have been removed and rhyolite constituents substituted in such a manner that the compositions of the altered rocks lie on straight lines between basalt and rhyolite in variation-diagrams. It therefore seems necessary to attribute properties to rhyolite magma (*e.g.* considerable super-heat) other than those usually postulated.

In his memoir " Genesis and Structural Relationships of the Absaroka Volcanic Rocks, Wyo.," J. T. Rouse (*Bull. Geol. Soc. Amer.*, **48**, 1937, 1257–96) shows that the massive pyroclastic formations and lava flows of this field occur in two groups, each of which is made up of a lower acid breccia, a middle basic breccia, and an upper series of basaltic sheets. The pyroclastic rocks were erupted from hundreds of small vents and a few volcanoes of moderate size. The basalts are all attributed to fissure eruptions.

W. H. Parsons has described the " Volcanic Centers [*sic*] of the Sunlight Area, Park County, Wyo." (*Journ. Geol.*, XLVII, 1939, 1–26). Two major centres and one small explosive vent have been found ; and with these, stocks, laccoliths, plugs, cone-sheets, and two remarkable radial dike systems, are connected. The rock-types exhibit a normal differentiation series from olivine-gabbro and basalt, through diorite and andesite, to sodic syenite and trachyte. Close resemblances to the Scottish Tertiary volcanoes are noted.

The first theralites were described from the Crazy Mountains of Montana by J. E. Wolff in 1885, and the veteran petrologist now deals with the whole suite of the " Igneous Rocks of the Crazy Mountains, Montana " (*Bull. Geol. Soc. Amer.*, **49**, 1938, 1569–1626). The theralite suite, including also nepheline-syenite, solvsbergite, akerite, bostonite, and camptonite, penetrates Tertiary sediments in the northern half of the mountains as stocks, laccoliths, sills, and countless dikes. Paradoxically, the igneous rocks of the southern half of the range consist of a great stock of diorite with innumerable consanguineous minor intrusions. The alkaline suite is shown to be the younger of the two. Notwithstanding the petrological contrast, the two suites are believed to have been derived from a common parent magma, the twenty new chemical analyses disclosing common features such as dominance of soda over potash, and unusual abundance of barium and strontium.

In their paper on " Quartz-monzonite and Related Rocks of the Libby Quadrangle, Montana, and the Effects on Them of Deuteric Processes," R. Gibson, I. Campbell, and W. F. Jenks (*Amer. Journ. Sci.*, XXXV, 1938, 345–69) describe numerous small stocks intrusive into the folded and faulted Belt Series. The age of the folding, faulting and igneous activity is probably Late Mesozoic. Deuteric effects are widespread, potash feldspar, sphene, magnetite and allanite, being recognised as late-stage minerals in nearly all the intrusive bodies.

In a memoir on " Granitisation, Albitisation, and related Phenomena in the Northern Inyo Range of California, Nevada," G. H. Anderson (*Bull. Geol. Soc. Amer.*, **48**, 1937, 1–74) shows that the Pellisier Granite has been formed by the replacement and recrystallisation of pre-Cambrian quartzites and argillites. At a late stage in granitisation, or shortly after, the rock was strongly albitised. The neighbouring Boundary Peak Granite, although much more homogeneous than the Pellisier, is also marginally albitised. It is thought that the fluids which were effective in both granitisation and albitisation processes had a common source in the Boundary Peak magma.

A. C. Waters summarises the results of his study of the " Petrology of the Contact Breccias of the Chelan Batholith " (Cascade Range, Washington) (*Bull. Geol. Soc. Amer.*, **49**, 1938, 763–94) as follows: The marginal granodiorite of the Chelan batholith is intrusive into hornblende-schist, with the formation of a broad zone of contact breccias and striped migmatites. "Field study, petrographic examination, and chemical analyses, all converge to show that the inclusions of schist caught up by the batholith were first thoroughly recrystallised, feldspathised, and silicified until they became fine-grained granoblastic rocks which megascopically resemble diorites, and then were differentially assimilated as a result of the liquefaction and running together of the felsic constituents allowing the remaining still-solid material which consisted chiefly of hornblende to pack together into clots of amphibolite." It is suggested that many "basic schlieren" originate in this way.

In his paper on the "Petrology of Later Tertiary and Quaternary Rocks of the North-western Cascade Mountains in Oregon, with Notes on Similar Rocks in Western Nevada," T. P. Thayer (*Bull. Geol. Soc. Amer.*, **48**, 1937, 1611–52) divides the Cascade lavas into two main groups, the Western Cascades and the High Cascades, on the basis of a marked unconformity and consistent differences in chemical composition. The Western Cascades include basalts similar to the Columbia Basalts, which interdigitate with another series consisting mainly of andesites. The lavas have been folded, faulted, and intruded by diorite plugs. The High Cascade lavas are mainly olivine-basalts (with the rather high average silica content of 53 per cent.) and subordinate andesites. Sixteen new chemical analyses are given.

A complex sequence of Pre-Cambrian plutonism is described by W. J. Miller in his paper on "Pre-Cambrian and Associated Rocks near Twenty-nine Palms, Cal." (*Bull. Geol. Soc. Amer.*, **49**, 1938, 417–46). The main intrusion is the Palms Granite, the *lit-par-lit* injection of which into gabbro-diorite produced a banded gneiss. The assimilation of gabbro-diorite by the Palms Granite has also produced a remarkable monzonite-porphyry with orthoclase crystals 6 inches in length. A discordant batholith of quartz-monzonite, probably of late-Jurassic age, cut the above Pre-Cambrian series.

A. B. Edwards describes "The Tertiary Volcanic Rocks of Central Victoria" (*Quart. Journ. Geol. Soc.*, XCIV, 1938, 243–320) with the aid of no fewer than 108 chemical analyses, of which 50, contributed by himself, are new. The volcanic suite, of Pliocene to Recent age, is essentially a trachyte-olivine-basalt kindred, in which olivine, diopsidic pyroxene, anorthoclase and ægirine, are the characteristic minerals. Kennedy's view of basaltic differentiation is favoured in preference to the theories of Daly and Shand, or of Bowen. The evolution of the suite has been brought about partly by gravity-differentiation involving the sinking of olivine, pyroxene and plagioclase, and the rising of anorthoclase and ægirine; and partly by the crystallisation of lime-rich pyroxene, removing lime from the magma, and leaving an alkaline residuum in which alumina combined with soda and potash. The parent magma was closely allied to the olivine-basalt magma-type, but was somewhat more alkalic.

The " Quartz-diorite Magma in Eastern Victoria," occurring in association with granites and hypersthene-dacite lava on the one hand, and with basaltic magma on the other, is believed by A. B. Edwards (*Proc. Roy. Soc. Vict.*, L, Pt. 1 (N.S.), 1937, 97–109) to have originated as a syntectic melt following a major orogeny. The types now exposed are explained as due to subsequent crystallisation differentiation of the mixed initial magma.

According to M. C. Bandy, in his paper " Geology and Petrology of Easter Island " (*Bull. Geol. Soc. Amer.*, **48**, 1937, 1589–1610), Easter Island is entirely volcanic, and was built up in three stages, an ancient eastern volcanic series, a central region with two volcanoes, and a south-western area containing a large volcano and some smaller cones. The lavas are mainly basaltic, but trachytes and rhyolites occur in the western area. The petrology is illustrated by five new analyses.

Recent French expeditions have provided material from Clipperton Atoll, Easter Island and Pitcairn Island, which A. Lacroix has investigated in his usual thorough manner with the aid of many new analyses (*Ann. de l'Inst. Océanographique*, XVIII, 1939, 289–330). Phosphatisation processes in the rocks of Clipperton Atoll are dealt with in detail. Easter Island has a large development of alkali-rhyolites. The Pitcairn collection consists of alkali-basalts and trachytes.

Some of the petrographical results of the B.A.N.Z. Antarctic Research Expedition, 1929–31, under the command of Sir Douglas Mawson, are now to hand. G. W. Tyrrell describes " The Petrology of Heard Island " (*B.A.N.Z. Antarctic Research Exped., 1929–31. Reports—Series A*, Vol. II (Geology), Pt. 3, 1937, 27–56), and " The Petrology of Possession Island (Crozets) " (*ibid.*, 57–68); while A. B. Edwards deals with the " Tertiary Lavas from the Kerguelen Archipelago " (*ibid.*, 1938, 72–100). Heard Island consists of Recent volcanoes with old and new lava flows. The rocks are limburgites, olivine-basalts, trachybasalts, trachyandesites, trachytes and phonolites, the petrography of which is illustrated by eight analyses, five new. Possession Island also consists of ankaramites, limburgites, olivine-trachybasalts, and trachyandesites, but so far trachytes and phonolites have not been found. Six analyses are given, of which three are new. Both islands, and especially Heard Island, represent typical examples of "oceanic island" assemblages.

A. B. Edwards describes phonolite, solvsbergite, trachyte, trachybasalt, crinanite, analcite-basalt, mugearite, andesine- and labradorite-basalt, olivine-basalt, limburgite and oceanite, from Sir D. Mawson's Kerguelen collection. The lavas form a typical "intra-Pacific", (Edwards) olivine-basalt-trachyte association. The alkaline types appear to have differentiated in cupola-like extensions of the main magma-reservoir by processes of crystallisation differentiation controlled by the early separation of diopsidic pyroxene.

Tyrrell points out the remarkable homology in geological constitution and petrological composition which obtains between Kerguelen and Heard Island on the one hand, and Iceland and Jan Mayen on the other, occupying, as they do, geographical "mirror-image" positions in the Indian and Atlantic Oceans respectively.

The "Olivine-Fourchites from Raymond Fosdick Mountains, Antarctica," described by C. N. Fenner (*Bull. Geol. Soc. Amer.*, **49**, 1938, 367–400), are lavas derived from an extinct volcano. Fenner has made a most complete chemical and quantitative study of the phenocrysts (olivine and augite) and of the groundmass of these rocks. The lavas are of ultrabasic composition, and to the writer of these notes appear to represent typical examples of Lacroix's oceanites. Fenner attempts to show that the differentiation of this rock-type, as shown by the composition of the phenocrysts compared with that of the groundmass, and with the bulk-composition, does not square with the current theory of differentiation by fractional crystallisation.

INVERTEBRATE PALÆONTOLOGY. By H. DIGHTON THOMAS, M.A., Ph.D., F.G.S., British Museum (Natural History).

A. RENIER, F. Stockmans, F. Demanet and V. Van Straelen deal with "Flore et Faune houillères de la Belgique. Introduction à l'Etude paléontologique du Terrain houiller," *Mus. Roy. Hist. Nat. Belg.*, 1938, 1–317. Among the invertebrates, a great number of species are described which belong to the Arthropoda, the Mollusca, the Brachiopoda, the Echinodermata, the Vermes, the Cœlenterata, and the Protozoa. The work gives a comprehensive survey of the Belgian Coal Measure fauna and flora.

P. Roché has described the "Aalénien et Bajocien du Maconnais et de quelques régions voisines" in *Trav. Labor. Géol. Univ. Lyon*, Fasc. XXXV, Mém. 29, 1939, 1–355. The deposits on the whole yield a rich fauna, of which the ammonites and brachiopods constitute the most important elements, though most of the invertebrate groups are represented. The author discusses the distribution of the fauna in the area considered and also the correlation of the various stratal units.

In " Contribution à l'Etude paléontologique du Nummulitique algérien " (*Matér. Carte Géol. Algérie*, 1 sér., *Paléontologie*, VIII, 1938, 1–158), J. Flandrin describes not only a large fauna of Nummulites and Lepidocyclines, but also several species of lamellibranchs and gastropods, and one echinoid. Montian, Yprésian, Lower and Upper Lutetian, and the Oligocene are all represented in the fauna.

During a study of insoluble residues from well and outcrop samples from Oklahoma, arenaceous foraminifera were discovered and these are the subject of a paper by H. A. Ireland on " Devonian and Silurian Foraminifera from Oklahoma " in *Journ. Paleont.*, XIII, 1939, 190–202. They include one new genus, twenty-one new species, and three new varieties from Silurian horizons, and one new genus and six new species from the Devonian.

R. Wright Barker writes on the " Species of the foraminiferal Family Camerinidæ in the Tertiary and Cretaceous of Mexico " in *Proc. U.S. Nat. Mus.*, LXXXVI, 1939, 305–30. The author considers that, in view of the inter-grading of so many of the so-called genera of the family, several of them are of doubtful value. The Mexican species considered, he attributes to *Operculinoides* and *Camerina*. He has identified vertical canals in *Camerina* and *Heterostegina*, and hence concludes that the Camerinidæ should embrace the Pellatispiridæ, which had been removed from it because of the formerly presumed absence of a canal-system in the Camerinidæ.

Limestones from eight localities on the island of Guam have yielded recognisable foraminifera, and W. Storrs Cole describes them in " Larger Foraminifera from Guam," *Journ. Paleont.*, XIII, 1939, 183–9. At least two distinct horizons are present, belonging to the lowermost Miocene.

R. and J. Bedford, continuing their investigations into the " archæocyathines " of the Cambrian of South Australia, discuss the " Development and Classification of Archæos (Pleospongia) " in *Mem. Kyancutta Mus.*, VI, 1939, 67–82. Their classification is based on the character of the earliest known stages, or " spitzes," of development of the skeleton. Two orders, 6 families, 3 genera, and 3 species are described as new. The order Monocyathina embraces the most primitive adult sponges, which resemble the *Olynthus* stage of sponges. The order Acanthinocyathina includes forms in which the spitz possesses two walls with an open net-like structure, while in the new order Ajacicyathina the spitz is in the form of a simple conical tube, closed below, within which arises almost immediately a porous inner wall connected to the tube by radial rods. The order Metacyathina is characterised by a spitz which commences as a small conical tube with transverse partitions. A new order Crommyocyathina is erected for a number of problematical forms whose relations to one another and to the better known orders of Archæos remain doubtful.

New "Siliceous Sponges from Mississippian and Devonian Strata of the Penn-York Embayment" are described by K. E. Craster in *Journ. Paleont.*, XIII, 1939, 1–20. They all appear to come from the same general facies zone as most of the hitherto described siliceous sponges from New York and Pennsylvania. They include one new family, one new sub-family, and three new genera.

In *Bull. internat. Acad. Sci. Bohême*, 1939, 1–10, F. Prantl describes "Some Silurian Rugose Corals from Bohemia." There are two new species involved, of which one, *Tryplasma formosum*, belongs to the group of *T. loveni*, while the other, *Microplasma flexuosum*, is closely allied to, and may even be conspecific with, *M. schmidti*. The author considers that *Aphyllum* Soshkina is congeneric with *Microplasma* Dybowski. He concludes that the Silurian corals of Bohemia show a much greater resemblance to those of the English and Gotlandian Silurian than had been formerly supposed.

The same author also describes "*Bojocyclus* nov. gen., A new Rugose Coral from the Hlubočepy-Limestones (gγ)" in *Priroda Brno*, XXXII, (3), 1939, 104–7. The new genus belongs to the Palæocyclidæ but differs from all other representatives of that family.

W. Weissermel writes on "Neue Beiträge zur Kenntnis der Geologie, Palæontologie und Petrographie der Umgegend von Konstantinopel. 3. Obersilurische und devonische Korallen, Stromatoporiden und Trepostome von der Prinzeninsel Antirovitha und aus Bithynien" in *Abhandl. Preuss. Geol. Landesanst.*, N.F., CXC, 1939, 1–131. Corals, especially tabulate forms, constitute the bulk of the fauna, which ranges in age from the Upper Silurian to the Lower Middle Devonian. Certain of the species and one genus are new.

The same author has also published his initial studies on "Die Korallen des thüringischen Devons. 1. Korallen aus Oberdevon im westlichen Schiefergebirge Thüringens" in *Jahrb. Preuss. Geol. Landesanst. für 1938*, LIX, 1939, 353–69. The thirteen species include six new species and one new variety, and are distributed among eleven genera.

In continuation of their re-description of the species described in James Hall's classical work, C. L. and M. A. Fenton deal with "*Heliophyllum* and '*Cystiphyllum*,' Corals of Hall's 'Illustrations of Devonian Corals'" in *Ann. Carnegie Mus.*, XXVII, 1938, 207–50. Whereas J. W. Wells reduced Hall's species of *Heliophyllum* to formæ of *H. halli* (see SCIENCE PROGRESS, XXXII, 1938, 743), the present authors prefer to recognise seven species and certain formæ and sub-species, some being new.

A. H. Sutton and W. H. Hagan re-define the genus *Zeacrinus* in "Inadunate Crinoids of the Mississippian—*Zeacrinus*," *Journ. Paleont.*, XIII, 1939, 82–96. Six new species are described from the Chester Series, and a revision of previously known species is carried out. This restricts the genus to twenty-five Mississippian species.

J. K. S. St. Joseph has studied "The Structure of *Lycophoria* Lahusen (Brachiopoda)," *Geol. Mag.*, LXXVI, 1939, 130–6. In the cardinal area, open delthyrium, and separate dental-plates, the ventral valve has points of orthid character. The pseudodeltidium remained throughout life an integral part of the dorsal valve, while at the same time it functioned in closing the delthyrial opening. The occurrence of a pseudodeltidium and the form of the cardinal process suggest that the genus is best included in the Strophomenacea.

M. A. Arber has investigated "The Nature and Significance of the Pedicle-foramen of *Leptæna* Dalman," *Geol. Mag.*, LXXVI, 1939, 82–92. She concludes that the pedicle in the youngest stages emerged through a foramen enclosed within the protegulum and nepionic ventral valve: with growth of the animal the pedicle either resorbed the margins of its foramen or atrophied. The nepionic stage is thus parallel to the adult stage of certain members of the order Neotremata, but this is not a case of recapitulation. The delthyrium of *Leptæna* never served as the pedicle passage, and can only be regarded at present as the interdental space through which the cardinal process projected into the ventral valve. It is therefore not homologous with the delthyrium of the Telotremata, nor with that of the Orthidæ. It appears that the Orthacea and Strophomenacea evolved on separate though somewhat parallel lines from a very early period.

The "Devonian Brachiopoda of Hunan" are the subject of a detailed investigation by C. C. Tien in *Palæont. sinica*, n.s. B, IV, 1938, 1–192. As a result, certain modifications in the generally accepted classification of Devonian rocks of Hunan are put forward.

The presence of *Stringocephalus burtini* and *Hypothyridina cuboides* enable correlation of part of the succession with that of Europe and North America to be effected, and with those definite links a correlation of the rest of the Devonian is suggested. Altogether eighty-seven different forms are described.

"The Mesozoic Brachiopoda of the Bannu District," which are described by M. R. Sahni in *Palæont. indica*, n.s., XXVII, (1), 1939, 1–23, consist mostly of well-preserved terebratellids and a few rhynchonellids and terebratulids, and all belong to new species. Part of the fauna is possibly of Callovian age and part probably of Oxfordian-Kimmeridgian age.

Two important monographs on lamellibranchs are those on "The Atlantic and Gulf Coast Tertiary Pectinidæ of the United States. Section III. Systematic Descriptions," by H. I. Tucker-Rowland, *Mém. Mus. Roy. Hist. Nat. Belg.*, (2), XIII, 1938, 1–76, and on "Oligocene Arcid Pelecypods of the Genus *Anadara*," by H. G. Schenck and P. W. Reinhart, *ibid.*, XIV, 1938, 1–73. The former is entirely systematic—in addition to some sub-species and varieties, fifty-two species, two of which are new, are described under seven genera or sub-genera.

In the latter work Schenck and Reinhart show that species of *Anadara*, *sensu lato*, occur earliest in any complete marine Tertiary sequence in the Oligocene, although the species found in different continents are never the same. A large number of Oligocene species are described by the authors, mostly falling into *Anadara*, *sensu stricto*, but a number belonging to related sub-genera, notably *Scapharca*, are also dealt with, as well as some late Tertiary and Quaternary species of *Anadara*. One result of the study is to show that there is no morphological character within the genus which will serve as a criterion of geological age.

J. Roger has monographed "Le genre *Chlamys* dans les formations néogènes de l'Europe. Conclusions générales sur la répartition géographique et stratigraphique des Pectinidés du tertiaire récent," *Mém. Soc. Géol. France*, n.s., XL, 1939, 1–294. The European Neogene species of the four genera *Chlamys*, *Pecten*, *Flabellipecten*, and *Amussium* are described in detail. The author divides the species of *Chlamys* into two groups, the one characterised essentially by the presence of flat, internal ribs, limited by sharp crests, and the other by radial, rounded folds on the inner surface, without sharp, limiting crests. The second group is the older phylogenetically, and probably originated in the Carboniferous or even earlier.

The British Silurian gastropod fauna is far from well known, so that the investigations made by B. L. Pitcher into "The Upper

Valentian Gastropod Fauna of Shropshire " is a notable contribution (*Ann. Mag. Nat. Hist.*, (11), IV, 1939, 82–132). Most of the material is in the form of external moulds, from which casts were made for the study of the fossils. No Bohemian species are found to be present, but several Gotland forms were recognised, as well as one American species, while one was also compared to an American species. An analysis of the fauna shows that several species in Shropshire are confined to the Pentamerus Beds and others to the Purple Shales, though a number occur in both of those horizons.

" The Eocene and Paleocene Turritellidæ of the Atlantic and Gulf Coastal Plain of North America " form the subject of a long work by E. Bowles in *Journ. Paleont.*, XIII, 1939, 267–336. Forty-one species and nine sub-species of *Turritella* are described and eleven species of *Mesalia*. Several are new. The *Turritellæ* fall into five major groups, based on their ornamentation.

A. K. Miller and J. B. Owen have investigated " An Ammonoid Fauna from the Lower Pennsylvanian Cherokee Formation of Missouri " in *Journ. Paleont.*, XIII, 1939, 141–62. It includes well-preserved representatives of nine species belonging to eight genera. An *Anthracoceras* horizon occurs 10 to 15 feet below the Jordan coal member of the Cherokee formation, while other ammonoids occur in the beds immediately overlying both the Tebo and Mulky coal members of the Cherokee. The fauna found just above the last-named coal is probably of about the same age as that which occurs in the Wigton Shale of Cumberland, England, and in Skipsey's marine band of Scotland.

The " Upper Jurassic Ammonites from Mexico " described by R. W. Imlay in *Bull. Geol. Soc. Amer.*, L, 1939, 1–78, occur in near-shore deposits, and show that, with certain slight modifications, Burckhardt's stratigraphical divisions for the off-shore deposits can be traced into them. The succession of ammonite assemblages agrees fairly well with that of the Mediterranean province, and is representative of horizons from Middle Oxfordian to Tithonian.

L. F. Spath discusses " The Cephalopoda of the Neocomian Belemnite Beds of the Salt Range " in *Palæont. indica*, n.s., XXV, (1), 1939, 1–154. They are all ammonites with the exception of three species of belemnites, and belong to zones ranging from the Infra-Valanginian to the Upper Valanginian. Those forms which are referred, more or less tentatively, to known species are compared, apart from extra-European forms, partly to French and partly to Silurian species, but the author considers this of no significance.

L. Størmer has written an important work on " Studies on

Trilobite Morphology. Part I. The Thoracic Appendages and their Phylogenetic Significance " in *Norsk geol. tidsskr.*, XIX, 1939, 143–273. His conclusions are based on a study of serial sections of specimens of *Ceraurus* and the construction from them of highly magnified models. He recognises a precoxal segment to which he claims the lateral branch generally called the exopodite is attached, so that, in his view, the latter represents not an exopodite but a pre-epipodite. The author considers that the trilobite appendage is a very characteristic and conservative structure, and that allied types of limb are found not in the crustacea (as is generally held) but in the merostomata, arachnida, and possibly the insecta.

In " The Cambrian Faunas of North-Eastern Australia. Part 3: The Polymerid Trilobites (With Supplement No. I)," *Mem. Queensland Mus.*, XI, (3), 1939, 179–282, F. W. Whitehouse completes his important study of the trilobites of that area. He suggests that the seven, discrete Lower Cambrian groups, with their descendent families, should be treated as seven sub-orders. These are: Mesonacida, Conocoryphida, Ptychopariida, Ellipsocephalida (nom. nov.), Corynexochida, Agnostida, and Eodiscida (nom. nov.). A great number of new genera and species are described, including, in a supplement, some agnostids. One specimen of a *Redlichia* showed some peculiar structures, which are apparently internal processes, though their function is obscure.

C. Deiss writes on " Cambrian Stratigraphy and Trilobites of Northwestern Montana " in *Geol. Soc. Amer. Spec. Papers*, XVIII, 1939, 1–135. The trilobites come from rocks of Middle Cambrian age and are largely new, and the author has described 102 new species and 10 new genera. He recognises at least seven trilobite faunas of different ages in the area.

Another useful work on trilobites is that by W. F. Whittard on " The Silurian Illænids of the Oslo Region," *Norsk geol. tidsskr.*, XIX, 1939, 275–95. Between five hundred and six hundred specimens were examined, and these fall into the two genera *Illænus* and *Bumastus*. Five species (one new) represent the former genus, and four species (one a new variety) the latter. Names are not given to pygidia which cannot be definitely linked to cranidia, but instead six types (A–F) are used in describing them. The author also adopts a standard method of orientation of the cranidia in illustrating them.

R. F. C. R. Gheyselinck has written on " Permian Trilobites from Timor and Sicily, with a Revision of their Nomenclature and Classification," Amsterdam, 1937, 1–108, and divides the youngest members of the Proëtidæ into the four genera, *Brachymetopus*, *Proëtus*, (subgenus *Permoproëtus*), *Phillipsia* (with two sub-genera, one of which is new), and *Griffithides* (with two sub-genera). Many genera and sub-genera are discarded by the author. Nine species (eight new) and one new variety are described from Timor, and three species (one new) from Sicily.

G. O. Raasch refers the " Cambrian Merostomata " to the new order Aglaspida and the two new families Aglaspidæ and Beckwithiidæ (*Geol. Soc. Amer. Spec. Papers*, XIX, 1939, 1–146). Several new genera and species are recognised. He concludes that the Aglaspida are the most primitive known group of Arachnida, and that the closest relationships of the Aglaspidæ are with the Eurypterida, and of the Beckwithiidæ probably with the Xiphosura.

An important monograph of the " Fossil Orthoptera Ensifera " by F. E. Zeuner has been issued by the British Museum (Natural History), 1939, 1–321. Descriptions are given of all the known families, sub-families, genera and species which can be referred indubitably to the group. An exhaustive study of Recent as well as of the fossil forms leads the author to important conclusions on the phylogeny of the sub-order, as well as on the general relations of the Orthoptera and the origin of the Saltatoria from the Protorthoptera.

" New Blattoid Insects from the South Wales Coalfield " are described by F. S. Wallis in *Geol. Mag.*, LXXVI, 1939, 23–5. There are four new species which all come from the *tenuis* zone. The author describes a new technique for copying the veins of wings—the wings are first photographed, and then the veins are carefully marked on the print in coloured inks, the print being subsequently bleached.

M. T. James discusses the fossil Asilidæ in " A Preliminary Review of Certain Families of Diptera from the Florissant Miocene Beds. II," *Journ. Paleont.*, XIII, 1939, 42–8. Six new species are described, and the author concludes that the Bibionidæ, Stratiomyidæ, Empididæ, and Asilidæ have not undergone much evolutionary development since the Miocene. The Florissant fauna indicates a warm, humid, but not tropical climate.

BOTANY. By Professor E. J. Salisbury, C.B.E., D.Sc., F.R.S., University College, London.

The gametophyte and young sporophyte of *Psilotum triquetrum* has now been fully described by J. E. Holloway (*Annals of Botany*, III, 313, 1939). The mature gametophyte is similar to that of *Tmesipteris*. It is subterranean, of an elongated cylindrical form, bearing numerous rhizoids, and contains a considerable proportion of mycorrhizal tissue. Growth takes place by means of an apical cell with four faces from which segments are cut off. The gametophyte exhibits forked branching and attains a length of up to 18 mm. It can reproduce by means of gemmæ, which are sometimes produced in large numbers. Two types of gametophyte are recognised, distinguished by their stout or slender form. A striking feature of the former is the presence of a tracheidal strand traversing the central region and consisting of from one to three elements, a feature which has not been observed in the slender type of gametophyte. This conducting strand consists of annular and scalariform tracheids and is limited by an endodermis, but a remarkable feature is that it is intermittently present along the axis of the gametophyte.

The antheridia and archegonia occur on the same gametophyte scattered over the entire surface. The mature archegonium has a straight neck of from six to seven tiers of cells, with four cells in each tier, and there are probably two neck canal cells. The antheridium normally projects in a globular manner from the surface. Both antheridia and archegonia differ markedly in size upon the slender and robust gametophytes, those on the latter being the larger. Embryo formation takes place as in *Tmesipteris*.

In 1929 Hartmann showed that there are three types of plants of *Enteromorpha compressa*, all externally alike, namely male plants, female plants and asexual plants. K. R. Ramanathan (*Annals of Botany*, III, 375, 1939) has now investigated this species cytologically, and finds that the sexual plant, which produces biciliate gametes, has 10 chromosomes in the nucleus, whereas the asexual plant, which produces quadriciliate zoospores, has 20 chromosomes. Sexual fusions between the gametes show all conditions from strictly isogamous to definitely anisogamous. There is thus a definite alternation of generations, and, though the plants are morphologically alike, the gametophytes have photosynthetic cells which contain from two to three pyrenoids, whilst those of the sporophytes contain from three to five pyrenoids.

In the same journal Mrs. Arber (p. 337) deals with the peculiar morphology of the petal of *Lychnis*, which has been the subject of various interpretations. On anatomical grounds Mrs. Arber concludes that the two lateral wings at the junction of limb and claw may be regarded as stipular, but that the two central "coronal teeth" are actually invaginations, morphologically similar in character to the median tooth of each corolla lobe in such a Boraginaceous flower as Cynoglossum.

Apomictic seed production in *Poa pratensis*, recorded by Rancken and Akenberg, showed that the stimulus of pollination, though not necessarily through pollen of the same species, was requisite for the apomictic seed development. The latter author now describes pollination experiments with six species, including *P. pratensis* itself. Unpollinated plants set no seed, but of the pollinated plants seed production was actually higher with the pollen of *P. alpina* than with the pollen of *P. pratensis*. The percentage of aberrant individuals in these pollination experiments, namely 9·2 per cent., may be taken as an indication of the proportion of sexually produced offspring, and observations on wild populations indicated about 7 per cent. of sexual individuals (*Hereditas*, XXV, 359, 1939).

The North American species of *Crepis* have been investigated by G. L. Stebbins and J. A. Jenkin in respect to their apomictic behaviour (*Genetica*, XXI, 191, 1939). The members of this genus vary considerably in respect to the frequency of apospory and the time at which it begins. In tetraploid *Crepis occidentalis* apospory occurs in 28 per cent. of the ovules and begins after megaspore tetrads have formed. In most types apospory is facultative and occurs in 78–87 per cent. of the ovules, but in one apomict of *C. occidentalis* with a somatic chromosome complement of 77 apospory occurs in all the ovules.

The osmotics of a mangrove area in Bengal have been investigated by Jatis (*Ber. d. Deut. Bot. Ges.*, LVI, 474, 1938). As T. G. Hill showed, for temperate halophytes there is a marked correlation between the osmotic value of the cell sap and the chloride content. Thus *Avicennia alba*, with an osmotic value for the cell sap of 41·29 atms., had a chloride content corresponding to 26·4 atms. At the other extreme *Malacchra capitata*, with an O.P. of 7·14 atms., had a chloride content corresponding to 2·76 atms. Amongst the values recorded the following may be cited: *Ægiceras majus*, 26·4–38·2 atms.; *Avicennia officinalis*, 25–42 atms.; *Sonneratia acida*, 11–19 atms.; *Bruguiera*, 22–8 atms.; *Acanthus*, 14–25 atms.

The *East Malling Research Station Report* for May 1939 contains several papers which have an ecological bearing. H. M. Tydeman (p. 103) reports experiments upon the germination of pear seeds which indicate that low temperatures favour germination and that overwintering the seeds under cold conditions is advantageous. Unwashed seeds yielded only 30·3 per cent. of seedlings, whereas washed seeds yielded 50·2 per cent. germination and, moreover, germinated earlier, so that the resultant seedlings were larger. Seeds sown in the spring appear to germinate better than those sown in the autumn. In the same report (p. 127) C. P. Field discusses the results of experiments in which the blossoms of two varieties of Apple, namely Crab and Court Pendu Plat, were subjected to low temperatures. These indicate that damage is caused when the temperature falls below 29° F. and that its extent is proportional to the duration of the low temperature. The dryness of the blossom, as was to be anticipated, materially affects the susceptibility, but no indication could be obtained of hardening, although slow cooling delayed the appearance of the damage.

An additional contribution to the study of the distribution of the Flora of Ireland by Dr. Praeger appears in the *Proceedings of the Royal Irish Academy* for February (XLV, B, 10, 231, 1939). Amongst the more interesting records may be noted a number of aliens which have become naturalised and some of which, like *Epilobium nummulaerifolium*, bid fair to spread at the expense of the native flora. Altogether these records involve some four hundred species.

The *Report of the Botanical Society and Exchange Club* (XII, 1, 1939) contains a number of new county and vice-county records, which include *Ranunculus sphaerospermus* from South Wilts, *Corydalis claviculata* from South Wilts, *Fumaria Bastardi* from Oxford, *Draba muralis* from Rutland, *Thlaspi calaminare* from Rhum, *Trifolium squamosum* from South Lancashire, *Rosa spinosissima* from South Wilts, *Bupleurum rotundifolium* from South Lincolnshire, *Cicuta virosa* from South Lancashire, *Antennaria dioica* from North Wilts, *Trientalis Europea* from South Lancashire, *Myosotis brevifolia* from Mid-West Yorks, *Euphrasia rivularis* from Cumberland, *Rhinanthus stenophyllus* from North Aberdeen, *Utricularia intermedia* from South Lancashire and Arran, *Euphorbia platyphyllos* from South Lincolnshire, *Empetrum hermaphroditum* from Banff, *Ceratophyllum demersum* from Renfrew, *Potamogeton pusillus* from North Wilts, Warwick, Oxford, Staffordshire, and Lanark, *Carex atrofusca* from Rhum, *Carex elongata* from Dorset, *Carex maritima* from South Northumberland, *Mibora verna* from South Northumberland, *Glyceria declinata* from West Kent, *Festuca heterophylla* from South Hants, and *Equisetum hyemale* from North Aberdeen. The same journal contains a paper on the British species of Utricularia by P. M. Hall and one on the British species of Rumex by J. E. Lousley.

PLANT PHYSIOLOGY. By Professor Walter Stiles, Sc.D., F.R.S., The University, Birmingham.

Micro-Nutrients.—Of late years the importance for plant nutrition of a number of elements, not previously regarded as essential for plant growth, has come to be realised, and these elements, often called micro-nutrients or trace-elements, because they are required by plants in only very small quantities, have attracted increasing attention on the part of plant physiologists. In 1936 W. E.

Brenchley published a review of the subject (" The Essential Nature of Certain Minor Elements for Plant Nutrition," *Bot. Rev.*, **2**, 173–96, 1936) and concluded that the evidence then available indicated that small amounts of boron and manganese are necessary for the healthy growth of many, and possibly all, plants, while copper and zinc are essential for the growth of, at any rate, some species. Two years later, J. E. McMurtney (" Distinctive Plant Symptoms caused by Deficiency of any one of the Chemical Elements essential for Normal Development," *Bot. Rev.*, **4**, 183–203, 1938) reviewed the symptoms due to deficiency of various elements exhibited by plants, and records various deficiency diseases caused by shortage of micro-nutrients. These include top-rot of tobacco, heart-rot and dry-rot of sugar beet, internal browning of cauliflower, internal cork of apples and cracked-stem of celery, all due to deficiency of boron, and grey-speck disease of oats, pahala blight of sugar-cane and chlorosis of tomatoes due to shortage of manganese. Rosette diseases of fruit trees were remedied by supplying the trees with zinc, while die-back of *Citrus* trees has been similarly remedied by supplying the trees with copper, and there is at least the possibility that in such cases very small quantities of zinc and copper are essential for the healthy growth of these trees. It has also been reported that small quantities of copper are necessary for flax and barley to set seed.

In a still more recent review J. W. Foster (" The Heavy Metal Nutrition of Fungi," *Bot. Rev.*, **5**, 207–39, 1939) has summarised the literature dealing with, among other data, the micro-nutrients of fungi. The conclusion is drawn that the need for small quantities of zinc, manganese and copper is widespread among fungi.

More attention has perhaps been given to boron than to other micro-nutrients. The state of knowledge relating to this element was summarised in this journal two years ago by R. W. G. Dennis (" The Relation of Boron to Plant Growth," SCI. PROG., **32**, 56–69, 1937). A more exhaustive account of the subject was published by the same author about the same time (" Boron in Agriculture," *West of Scotland Agric. Coll., Plant Husbandry Dept., Research Bull. No. 5*, 98 pp., 1937). In this it is shown that certain soils are deficient in boron, the content of this element in soils varying considerably although its effect is masked by secondary effects which determine the availability of boron to the plant. Water and calcium content of the soil are the principal factors concerned here, and liming and drought can bring about boron deficiency in the plant by reducing the availability of this element.

In addition to the diseases related to boron deficiency noted above, Dennis mentions also brown heart of swedes and turnips, Boron, he states, is of universal occurrence in living organisms, and flowering plants cannot develop fully in complete absence of this element. The function of boron appears to be connected with the water relations of the cell and with the translocation of manufactured food materials in the phloem.

Two summaries of more recent work on the effect of boron on plants have appeared, the first by R. W. G. Dennis ("Boron and Plant Life, II: Recent Developments in Agriculture and Horticulture," *Fertiliser, Feeding Stuffs and Farm Supplies Journ.*, 15 pp., Sept.–Oct. 1937), the second by A. C. Dennis and R. W. G. Dennis ("Boron and Plant Life, III: Developments in Agriculture and Horticulture 1937–38," *Fertiliser, Feeding Stuffs and Farm Supplies Journ.*, 19 pp., Feb., March and April 1939). In these papers are summarised the results obtained during the years 1935 to 1938 on the application of boron to field crops. The range of experiments noted in these summaries is surprisingly wide and no less than 264 contributions on the subject are listed in the bibliographies attached to the reprints of the papers. Among the more important results recorded we may note increased yield and increase in sugar content of sugar beet as a result of treatment with borax, and the control of the deficiency diseases already noted by treatment with borax or boric acid. Perhaps some of the most interesting experiments on the subject are those made on apples, in which boron deficiency produces a number of conditions known as drought spot, corky core, internal cork and corky pit. Experiments in Canada, New Zealand and the United States all show that the development of internal cork in apples can be controlled by the addition of borax or boric acid to the soil. Askew in New Zealand made determinations of the boron content of the leaves and fruit of healthy apple trees and trees in which the fruit had developed internal cork, and found a very definite difference in boron content of healthy and diseased plants. He also found that boron rapidly enters the roots of apple trees and penetrates through the tree so that it reaches the fruit two months after its application to the soil. The rate of intake by the fruit increases as the fruit develops and reaches a maximum just before maturity, after which the rate of intake declines. The relative amounts of boron absorbed by treated and untreated plants were, in the case of Jonathan apples, 252·4 and 138·2, and in the case of Dougherty apples, 222·5 and 61·5. The amount of boron required by an acre of New Zealand orchard was estimated at 44 gm. per annum.

While the Gramineæ, or at any rate cereals, require very little boron, they appear to be more sensitive to deficiency in manganese.

The grey-speck disease of oats and wheat appears to be definitely associated with shortage of manganese, and so does the pahala blight of sugar cane. In some cases it appears that chlorosis may be the result of manganese deficiency. As with boron, other conditions of the medium may be important in bringing about shortage of manganese in the plant, and the reaction of the medium appears to be of the first importance in this respect, alkalinity of the medium leading to non-availability of the manganese. Chlorosis in beans, due to manganese deficiency, has recently been described by G. R. Townsend and H. W. Wedgworth ("A Manganese Deficiency affecting Beans," *Florida Agric. Exp. Sta., Bull. No. 300*, 23 pp., 1936). In this case the chlorosis is followed by the loss of the leaves and death of the plant.

The effect of zinc in controlling rosette diseases of fruit trees has been mentioned earlier. Although not denying these effects Brenchley does not regard the evidence as affording definite proof that zinc is an essential nutrient for higher plants. In a recent contribution on this subject, however, W. H. Chandler ("Zinc as a Nutrient for Plants," *Bot. Gaz.*, **98**, 625–46, 1937) definitely supports the view that in *Citrus* zinc is a micro-nutrient and that the rosette condition is a deficiency disease. The nutrient effect of zinc in the case of many fungi appears to be definitely established.

Recent water culture experiments by D. I. Arnon ("Micro-elements in Culture-solution Experiments with Higher Plants," *Amer. Journ. Bot.*, **25**, 322–5, 1938) indicate that in addition to the four elements boron, manganese, zinc and copper, generally regarded as micro-nutrients, there may be others. Lettuce and asparagus plants were grown in four different culture solutions. The first contained only the ordinary nutrients, nitrogen, phosphorus, sulphur, potassium, calcium, magnesium and iron. The second contained in addition boron, manganese, zinc and copper. The third contained all the nutrients in the second solution and in addition seven other elements (Mo, Ti, V, Cr, W, Co, Ni) while the fourth contained in addition to all those in the third solution thirteen other elements (Al, As, Cd, Sr, Hg, Pb, Li, Rb, Br, I, Fe, Se, Be). The results of these experiments were striking. In the case of asparagus the respective fresh weights of the shoots of the plants grown in the four solutions were 16·2, 88·2, 118·1 and 121·7, and of the roots 12·8, 38·2, 81·3 and 74·5. The favourable effect of the micro-nutrients on the growth of lettuce was even more marked, for with this plant the respective fresh weights of the shoots were 71·4, 105·7, 1068·3 and 984·4, and of the roots 14·5, 22·0, 188·6, 196·2. Thus, although a marked increase in growth resulted from the addition of the four elements generally regarded as micro-nutrients, a further very definite increase in growth resulted when a small quantity of seven other elements (molybdenum, vanadium, titanium, tungsten, chromium, nickel and cobalt) were present in the culture solution. This suggests very strongly that one or more of these seven elements plays a part in the nutrition of the plants used.

The effect of molybdenum on a number of plants has been examined by K. Warington ("Observations on the Effect of Molybdenum on Plants with Special Reference to the Solanaceæ," *Ann. App. Biol.*, **24**, 475–93, 1937) the species used being potato, tomato, *Solanum nodiflorum*, barley and broad bean. Various compounds of molybdenum were formed in the tissues of plants supplied with sodium molybdate and such compounds with tannin and with anthocyanin resulted in the formation of definite bodies within the cell to such an extent that the general appearance of the plants were altered. Thus shoots of tomato and tubers of potato turned respectively golden and reddish-yellow, while leaves of *Solanum nodiflorum* and leaf-sheaths of barley acquired a blue, rather than purple, appearance as a result of the reaction of the molybdenum with anthocyanin. These changes appear to be distinct from toxic effects produced by larger doses of molybdenum. Miss Warington's conclusion is that molybdenum, although not yet ranked as a micro-nutrient, evidently plays a part in the cytological and morphological behaviour of the plant.

In water culture experiments designed to examine the effect of micro-nutrients on plants it is obviously of the first importance that the culture solutions should be free from metal impurities. P. R. Stout and D. I. Arnon ("Experimental Methods for the Study of the Role of Copper, Manganese and Zinc in the Nutrition of Higher Plants," *Amer. Journ. Bot.*, **26**, 144–9, 1939) describe methods for the preparation of the ordinary nutrient salts so that these are free from such impurities, and for the purification of the distilled water used for making up the culture solutions. A test using dithizone is described for examining the purity of the water and of stock solutions of the ordinary nutrient salts. The authors were able to confirm the purity of their solutions by growing tomato plants in a nutrient solution containing the ordinary nutrients but devoid of the micro-nutrient elements manganese, zinc and copper respectively. It was found that in each case the plants developed the condition characteristic of deficiency in the particular element omitted.

ZOOLOGY. By EMERITUS PROFESSOR W. GARSTANG, M.A., D.Sc.; and B. W. TUCKER, M.A., The University, Oxford.

PRIMITIVE MOLLUSCA.—The systematics of a great textbook exert a wide influence, but when Thiele in Kükenthal's *Handbuch der Zoologie* (**5**, 1925) excluded the Solenogastra (= Aplacophora) from Mollusca, thereby abolishing the class Amphineura, that influence could only tend towards a narrowing of the malacological outlook, and his substitution of the confusing term *Loricata* (Schumacher, 1817) for the long-established and unambiguous *Polyplacophora* was a needless change of the same order. Two recent papers on the Polyplacophora, despite these influences, are of special interest.

Vera Fretter (*Trans. Roy. Soc., Edin.*, **59**, 1937, 119–64) has carefully studied the anatomy and physiology of the digestive system in several Chitonids of normal type, especially *Lepidochiton cinereus*. Her descriptions of complex matters, from an over-concentration on detail, are sometimes hard to follow, but her admirable illustrations leave no doubt as to her meaning. She finds a general similarity to that of the lower Gastropoda, which have similar rock-haunting and herbivorous habits. Food rasped off by the radula is passed into a dorsal food-channel flanked by ciliated grooves and mucous ridges, where it is entangled in mucilage and driven past the elongated apertures of the characteristic " sugar glands " of the œsophagus. These pour upon the food-rope a copious amylolytic secretion, and the mixed mass is swept into the stomach. The lateral sugar-glands, though concentrated into a single pair, and the dorsal channel, clearly correspond to the numerous œsophageal recesses and the food-groove of *Patella*. The stomach, on the other hand, is quite unlike the simple tubular organ of the limpets, being asymmetrically distorted by the formation of a large " ventral sac." The author gives good grounds for regarding this sac as an outgrowth of the dorsal wall which has dropped over on the left side and then extended to the right. This twist brings up the ventral half of the stomach as an axial channel above the sac, and rotates the two ciliated grooves within it, which are right and left in the œsophagus and intestine, into dorsal and ventral positions respectively. The author terms this channel the " dorsal channel," which tends to confusion with the true dorsal channel in the foregut, and should be avoided. We suggest " axial passage " for it. As interpreted, it is really the ventral half of the original gut-tube.

The two ducts of the digestive glands open into the hinder part of this axial passage, one behind the other, so that, in effect, by the intermediation of the passage (which seems to correspond with the *ductus cholœdicus* of Haller), the right (anterior) gland communicates more freely with the ventral sac, and the left (posterior) gland with the anterior intestine. These relations seem from Fretter's account to be intensified in *Ischnochiton* and *Cryptochiton*. As in *Patella*, the glands yield a protease, but, whereas there the enzyme is retained in the glands for intra-cellular digestion of the food, in Chitonids it is passed into the ventral sac and anterior intestine, and digestion is entirely extracellular, except by amœbocytes, which are numerous. The fluid products are pumped at intervals into the glands by muscular action of the anterior intestine, assisted by a special valvular mechanism, and the glands furnish the sole absorbing surface. The long coiled posterior intestine seems to be concerned only in the moulding of fæcal pellets, as in *Patella*.

The author rightly emphasises the resemblances to the lower Gastropoda, as indicated above, but has overlooked some important differences, which point to more primitive relations. "The structure of the alimentary canal of the Aplacophora," she remarks, "is so different from that of the Polyplacophora that no homology between them seems justifiable"—a judgment in conformity with Thiele's views, but ignoring the remarkably prophetic character of the gut of *Proneomenia* and its allies. In its straightness and simplicity, with its dorsal ciliated groove and almost metamerically sacculated and glandular side-walls, it yields an ideal precursor of the divergent arrangements of Chitonids and Gastropods. The peculiar stomach of Chitonids seems to demand the pre-existence of a dorsal cæcum, and this, in very primitive form, is there too. Moreover, the entirely extracellular digestion of Chitonids is inexplicable as a deviation from Diotocard physiology, since food and conditions of life are identical. It points more strongly towards an ancestry with carnivorous or at least omnivorous habits. Although precise details are lacking, we know that *Proneomenia* takes in small Crustacea as well as diatoms (Heuscher, 1893).

C. M. Yonge (*Quart. Jour. Micr. Sci.*, **81**, 1939, 367–90) describes the gills and respiratory currents of various "Loricata." He confirms previous observations on the backward currents in the palliopedal grooves, but reveals the important fact that the series of gills on a side is so inter-connected and arranged as to constitute a functional unity. In the commoner forms without adanal gills the space around the anus is empty, the last (postrenal) gill is enlarged and blocks the channel, while the anterior (abanal) gills are functionally united by the interlocking of cilia at the tips of their filaments. The whole series thus constitutes a perforated septum dividing each groove into outer inhalant and inner exhalant channels, and all the water which escapes behind has been made to traverse the interfilamental gaps. The current is induced by broad bands of cilia on the flat proximal and distal faces of the filaments.

In *Lepidopleurus asellus*, on the other hand, the anus has shifted from the pedal to the pallial side of the groove, the adjacent space is occupied on each side by six adanal gills behind the postrenal, and the abanal gills are reduced to the same number in front of it. The latter retain their vertical position and the same *modus operandi*, but the adanal gills are directed outwards across the groove as a horizontal septum and the current through them is reversed, *i.e.* from their upper (inner) to their lower (outer) sides. Thus, if we understand the situation correctly, two streams enter the space above the adanal gills and supply them, *viz.* an outer stream of pure water from the general inhalant current, and an inner stream of effete water from the abanal gills. The development of adanal gills is regarded as a secondary adaptation, which has appeared in separate families independently. The puzzle to us is to find what advantage is conferred by this repeated change from good respiration to bad.

Yonge gives good reasons for regarding the gills as true ctenidia, and expresses his belief that the Chitonid type has been evolved as an adaptation to tidal existence from a primitive univalve mollusc with a single pair of ctenidia in a posterior mantle chamber, *i.e.* from Lankester's " Archi-mollusc." It might be urged that Yonge's own results, and the difficulty of the adanal gills, point rather to a primitive condition in which the gills were in lateral series from the first.

OSPREYS AND SWIFTS.—L. V. Compton (*Univ. California Publications in Zoology*, **42**, 1938, 173–212) has reached important conclusions regarding the systematic position of the Osprey (*Pandion*), which proves to have unexpected affinities with the New World Vultures (Cathartæ). The author shows that it agrees with the members of this sub-order in its pterylosis and in the arrangement of the plantar tendons and differs entirely from the *Accipitridæ*, with which, in recent years, it has usually been placed. His conclusion is that it should be transferred from the sub-order Falcones to that of the Cathartæ, with which it also shows other resemblances and of which it would be the sole representative in the Old World. It seems doubtful, however, whether such a drastic change is justified by the facts since, in other important features, *Pandion* agrees with the members of the Falcones and differs from the Cathartæ, so that it may be suggested that it would be better regarded as constituting an isolated family of the Falcones preserving primitive characters which the Cathartæ also retain.

P. R. Lowe (*Trans. Zool. Soc.*, **24**, 1939, 307–48) has re-studied the problem of the systematic position of the Swifts (Cypseli) and Humming-birds (Trochili). Kitchin Parker and others have insisted on more or less close affinities of these birds with the Nightjars (Caprimulgi), but as a result of a most thorough original study of the structure of the three groups mentioned, together with that of the Passeres and Broadbills (Eurylaimi) the author tabulates and discusses a large number of characters, from which it appears clear that whereas almost all the essential features of the Caprimulgi are non-Passerine, the Swifts and Humming-birds show a large and significant measure of agreement with the Passeres and Eurylaimi and hardly any with the Nightjars. Nearly all the differences between the first-named two groups are related to the striking adaptive specialisations which both Swifts and Humming-birds have undergone. The author's conclusion is that the Passares Eurylaimi, Cypseli and Trochili should all be associated in one great order, the Passeriformes, in which he tells us he would also include the Woodpecker group (Pici). His evidence that all these groups are more closely allied to one another than to any others is clear and cogent, but his order Passeriformes seems undesirably vast and heterogeneous and the distinctions of the component groups seem to the reviewer of more than sub-ordinal value. It would seem preferable to treat all the subdivisions as constituting a group of allied orders under some classificatory designation of higher value.

THE HAIRS OF MONOTREMATA.—A. B. Wildman and J. Manby (*Trans. Roy. Soc., Edin.*, **59**, 1938, 333–49) have applied to the hairs and spines of Monotremes the new technique of celluloid impressions by which cuticular scale-patterns can be revealed with marvellous precision. In Echidnidæ a similar pattern-sequence characterises spines and bristles alike, the annulations changing from "crenate" at the tip to "smooth" at the base, but in *Ornithorhynchus* the authors claim that only the "shield-hairs" of the outer coat show this sequence in full, and that the "fur-hairs" lack the whole of the crenated zone which is conspicuous in the shields. From this they conclude that the spiny coat of Echidnids is the more primitive, that the shield-hairs of *Ornithorhynchus* are a "last remnant" of it, and that its fur-hairs are a later development, brought about by suppression of the shield region. These would be big conclusions to draw from the most perfect analysis of cuticular patterns, but their beautiful photographs hardly bear out their case. There should be a demonstrable correspondence between the tips of the hairs and the "necks" of the shields, and it is true that the authors describe both as "scalloped"; but this term is not defined, and the photographs reveal that the "scallops" in the two cases are not homologous.

In the hairs (Pl. V, Figs. 6–10) they can be seen to be equivalent to the bold dentations which occur lower down in both kinds of hair; while in the shield-hairs (Pl. IV) these same dentations can be traced upwards into very broad, low, transverse scales, of which the "scallops" are merely secondary marginal crenulations. In reality the tips of the hairs agree with the tips of the shields in being "annulate," as Dry appropriately termed them in the Mouse (*Jour. Genetics*, **16**, 1926, 295), and there is no reason for doubting their complete homology. Still more crucial, however, than any mere cuticular correspondence, is an observation recorded fifty years ago by Sir Edward Poulton in his classical paper on the subject (*Quart. Jour. Micr. Sci.*, **36**, 161) that the peculiar interruption of the medulla in the neck of shield-hairs is recognisable in fur-hairs at a corresponding point.

NOTES

The Future of the Royal Observatory, Greenwich (A. Hunter)

Selection of the site for a new astronomical observatory is nowadays a matter for years of patient study and observation. It is scarcely surprising, then, that such a casual choice as that of Greenwich, made by Charles II in 1675 when the village was a pleasant hour's trip by Royal barge down-river from Westminster, has not always proved ideal. But until recently, astronomical work could be carried on at Greenwich without more serious interference than that of cloud. With the southward trend of industry, however, new factories have sprung up close to the Thames between London and the sea, and new housing estates creep ever onward into Kent. The 1939 *Report of the Astronomer Royal* to the Board of Visitors discloses a position which threatens the continued existence of the national observatory on its present historic site.

The trouble is threefold: the deterioration of "seeing" and atmospheric transparency as a result of the emission of smoke and flue gases by factory and domestic chimneys; the fouling of the mechanical and optical surfaces of the instruments by deposits of grit and acid; and the rapid brightening of the night sky caused by modern developments in street lamps and advertising signs.

The first of these disadvantages affects practically all the work, visual and photographic. Double-star observations on the 28-inch refractor are hampered to an increasing extent as the limiting magnitude attainable decreases: a binary with tenth-magnitude components is now almost an impossible object. Perhaps the most serious effect, however, is on photometric work. The recently-completed programme of colour temperature measurements, in which Greenwich has led the world, can never now be extended, since the accuracy of the results depends on uniform transparency of the sky in different directions. The number of nights at Greenwich on which this condition is fulfilled was never large, but has recently diminished to an extent which makes further work impossible.

With regard to the deposition of foreign matter, this also has widespread effects. It is frequently necessary to interrupt time observations on the small transit instrument in order to free the pivots from grit. The waste of time so occasioned is bad enough when every observing moment is precious, but far worse is the damage to the pivots. On the big new reversible transit circle the pivots are better protected, but even so, appreciable scoring has occurred on both pivots and Y's. As to the optical surfaces, the refractor surfaces are not likely to suffer damage whilst care is taken over the cleaning, but the effect of the Greenwich atmosphere on aluminised surfaces is unbelievable to the lucky visitors from the big Californian observatories, where aluminum coatings are regarded as practically indestructible. The coelostat mirrors of the spectrohelioscope are uncovered only when the sun is actually shining, but even so the corrosive deposits render them unusable after less than a year. The experience with these small mirrors has been so discouraging that the 36-inch mirror remains silvered, and is taken down twice yearly for re-coating. It is most unfortunate that the prisms of the slit spectrograph designed for use with this mirror are of ultra-violet glass, so that this instrument cannot be used to full advantage with silver surfaces, which reflect poorly in the ultra-violet.

As for the glare from the night sky, this is mostly due to the reflection of light from arterial road lamps and advertising signs by the dirt particles of a smoke-laden atmosphere. Long-exposure photographs are now impossible; and even the relatively short exposures in the stellar parallax programme on the 26-inch refractor may involve a total exposure of half an hour, which on a sensitive emulsion will now cause the beginnings of fog. There was a time when Greenwich took a prominent place in photographing the more spectacular sights of the heavens—the globular clusters, the diffuse galactic nebulæ, the external galaxies—but its activity in this direction is now at an end.

The Astronomer Royal sums up thus: "If the Royal Observatory is to continue to make important contributions to astronomy, as it has done for the past 264 years, and to provide an efficient time service of high accuracy, it is essential that it should be removed to a site where astronomical conditions are favourable. The present wasteful efforts to secure observations under increasingly bad conditions, and the restriction of programmes that are necessitated by such conditions, would thereby be avoided."

Medical Research at Kuala Lumpur (C. Price Jones)

In the *Annual Report of the Institute for Medical Research at Kuala Lumpur, F.M.S.*, edited by the director, A. Neave Kingsbury, we have an interesting and detailed account of the work done during 1937. An enquiry by Dr. J. O. Poynton on the distribution, etiology and treatment of filariasis shows that infestation is widespread among the riverine population along the river Perak from Parit to within a few miles of the sea and on the Kinta and Dedap tributaries, and he estimates that about 1 per cent. (50,000) have elephantiasis and about 2 per cent. more were infested. Infestations with *Wuchereria bancrofti* are occasionally found, mainly in immigrants, but the majority of infestations in rural areas are with *Microfilaria malayi*. Mr. E. P. Hodgkin finds that *Mansonia uniformis* and *M. longipalpis* are the chief local vectors of *Mf. Malayi*, though *M. annulifera, M. annulatus* and *M. Indiana* were found by experimental feeding to be susceptible to infection. Two species of pathogenic Filaria occur in Malaya; *Wuchereria bancrofti* transmitted by culicine mosquitoes (*Culex fatigans*) and found in towns and large communities, and *Microfilaria malayi* carried by Mansonia mosquitoes, which breed in swamps and occur among rural communities along river banks, and are the species of local importance. *Microfilaria malayi*, after developing for twelve days in the mosquito, is deposited on the skin at the time of biting and finds its way to the inguinal lymphatic glands, where the sexual forms mature and reproduce by discharging numbers of embryos, which find their way into the blood stream, or failing this produce local inflammatory reactions resulting from their death in the lymphatics and glands, including recurrent attacks of fever with lymphangitis of the legs and elephantiasis of legs and feet.

Adult worms have not yet been found but it is stated "there seems to be little doubt that the adult worms have a life up to fifteen years." Investigations by dissection and infection of monkeys are in progress. Infestation with *Mf. Malayi* involves chiefly Malays, as this race inhabits areas favourable for breeding of the vector. Infestation increases with length of time spent in infested areas, but liability to clinical manifestations increases as the subject grows older. Various staining methods were considered for showing morphological features of the filaria; generally Giemsa is easy to employ and gives good nuclear differentiation. This is conveniently carried out in troughs, using a 1 in 35 dilution in buffered solution for half an hour, staining and dehæmoglobinisation being achieved in one process. The films containing Microfilariæ are separated and the differentiation improved by flooding them for a few seconds with 0·1 per cent. acid alcohol. Treatment with the object of killing the adult worms by injection of drugs has not been especially successful, though intravenous injections of cyanide of mercury have resulted in marked improvement in some cases. A more hopeful method is prevention, and investigation of the breeding habits of the vectors seems a likely course to pursue. Control of oxygenating water plants through agricultural developments and adequate drainage would have good influence; removal of "lines" to a point at which the catches of Mansonia are lowest, the transfer of those with positive blood films to some non-epidemic area in exchange for uninfected subjects, and the alteration of routine so that workers are not exposed near the breeding grounds in the early hours of the morning, would all be helpful.

For estimations of therapeutic efficiency of antimalarial remedies, cases in wards of the General Hospital, Kuala Lumpur, are used, and each case before admission to a test series must satisfy the standards previously fully described.

During the latter half of the year the standard of asexual parasite concentration in peripheral blood was raised to a minimum count of not less than 1000 per c.mm.; the former limit was 500 per c.mm. This raising of the standard should increase the value of the elimination of mild cases in which there is a considerable possibility of spontaneous cure. All cases in the test series were observed clinically and parasitologically for at least seven days. Mixtures containing all the active principles of cinchona bark may be little, if at all, inferior to quinine. Such a mixture, known as cinchona febrifuge, has for long been in use. It has, however, no fixed composition and the proportions of the various alkaloids vary widely. To overcome this "Totaquina" was introduced; it has a guaranteed content of crystallisable alkaloids of 70 per cent., of which one-fifth is quinine. Dr. J. C. Niven has investigated 134 subtertian and 70 benign tertian cases treated with Totaquina, the quinine treated controls being 129 and 73 respectively.

Totaquina is supplied as a brown powder, the composition of which is quinine 19 per cent., cinchonine 50 per cent. (or 77 per cent. of crystallisable alkaloids), cinchonidine 5 per cent. and quinidine 3 per cent. (= 15 per cent. of amorphous alkaloids). Totaquina is insoluble in water, and solutions were made up in dilute acid. The results from the present series and those of 1932 are closely parallel—briefly there is no significant difference between Totaquina and quinine in the rapidity of action in destroying parasites or reducing fever. Neither is an efficient subtertian gametocide. The number of cases returning to hospital with a second attack of the same type was the same in each group. Nor is the mixture of alkaloids more toxic than quinine. Many people object to taking what is described as " dirty quinine." The cost of Totaquina is only about half that of quinine bihydrochloride and three-quarters that of quinine sulphate. Tests with " Paludex," a Belgian proprietary preparation, showed that it is not an efficient remedy for acute infections with the Malayan strains of *P. falciparum.* A few cases were treated with Protonsil in doses of 0·9 gm. daily; this method is described as not reliable.

Miscellanea

The honours list published on the occasion of H.M. the King's birthday included the following awards: *K.B.E.:* Prof. W. B. Benham, emeritus professor of biology, University of Otago; Prof. T. A. Hunter, professor of philosophy and psychology, Victoria College, University of New Zealand. *Knights Bachelor:* Dr. E. J. Butler, secretary to the Committee of the Privy Council for Agricultural Research; Mr. F. Carnegie, chief superintendent of Ordnance Factories, War Office; Prof. J. Graham Kerr, M.P., formerly regius professor of zoology in the University of Glasgow; Prof. O. W. Richardson, Yarrow research professor of the Royal Society; Prof. J. F. Thorpe, chairman of the Smoke Sub-Committee, Chemical Defence Committee, War Office. *C.B.E.:* Mr. A. S. Cox, assistant comptroller, Patent Office, Board of Trade; Mr. Seton Gordon, for services to literature and natural history; Prof. E. J. Salisbury, Quain professor of botany, University College, London. *O.B.E.:* Dr. G. P. Douglas, assistant superintendent (research), Royal Aircraft Establishment, Farnborough; Dr. W. E. Fisher, principal, Wolverhampton Technical College; Prof. W. G. R. Paterson, principal, West of Scotland Agricultural College, Glasgow.

Viscount Nuffield and Mr. John Davison Rockefeller have been elected fellows of the Royal Society under the terms of the Statute which provides for the election of persons who " either have rendered conspicuous service to the cause of science, or are such that their election would be of signal service to the Society."

The Royal Society has elected three more foreign members. Prof. W. B. Cannon, professor of physiology in Harvard Medical School, Prof. G. von Hevesy, research professor in the Institute of Theoretical Physics, Carlsberg Laboratory, Copenhagen, and Prof. H. Freundlich now at the University of Minnesota, Minneapolis, U.S.A.

Sir William Bragg, P.R.S., has received the John J. Carty medal and award (3000 dollars) from the National Academy of Sciences, U.S.A. He has also been elected foreign associate of the Academy. A like honour has been conferred on Sir Joseph Barcroft, professor of physiology in the University of Cambridge.

The Elliot Cresson gold medal of the Franklin Institute has been awarded to Sir Charles Vernon Boys. Other medals whose award was made or announced during the past quarter included the award of the gold medal of the Royal Agricultural Society to Sir George Stapledon of the Welsh Plant Breeding Station, Aberystwyth, for his conspicuous services to agriculture ; the Albert Medal of the Royal Society of Arts to Sir Thomas Holland for his services to the mineral industries ; the Charles P. Daly medal of the American Geographical Society to Prof. H. J. Fleure of the University of Manchester ; the Linnean medal of the Linnean Society of London to Dr. E. D. Merrill of Harvard University, and the Melchett medal of the Institute of Fuel to Mr. H. A. Humphrey, lately consulting engineer to Imperial Chemical Industries, Ltd.

The University of London will confer the degree of D.Sc. *honoris causa,* on Prof. Niels Bohr and Sir Robert Robinson.

Prof. W. L. Bragg has been elected president of the Institute of Physics for the session 1939–40 ; Dr. James Ramsbottom, president of the Linnean Society for the same period, and Dr. Frank B. Jewett, president of the U.S. National Academy of Sciences for the four years 1939–43.

We have noted with great regret the announcements of the death of the following scientific workers during the past quarter: Dr. E. T. S. Appleyard, physicist, University of Bristol ; Brig.-Gen. the Hon. C. G. Bruce, leader of the Mount Everest Expeditions, 1922 and 1924 ; Sir Frank Dyson, Astronomer-Royal 1910–33 ; Mr. H. Havelock Ellis, psychologist ; Prof. Rudolf Fick, anatomist, University of Berlin ; Dame Maria Ogilvie Gordon, geologist ; Prof. I. M. Gubkin, geologist, U.S.S.R. ; Dr. A. Harker, F.R.S., petrologist, University of Cambridge ; Sir Frederick Hobday, lately principal of the Royal Veterinary College, London ; Prof. W. A. Jolly, physiologist, University of Cape Town ; Mr. F. W. Jones, ballistics expert ; Prof. A. E. Kennelly, electrical engineer, Harvard University ; Dr. E. Kidson, director of the Meteorological Office, New Zealand ; Prof. S. L. Loney, mathematician, Royal Holloway College, London ; Dr. J. G. Lipman, soil specialist ; Dr. W. J.

Mayo and Dr. C. H. Mayo, joint founders of the Mayo Clinic, Rochester, Minnesota; Prof. J. Mellanby, F.R.S., physiologist, University of Oxford; Prof. R. I. Meyer, chemist; Engineer Vice-Admiral Sir Henry John Oram, from 1907 to 1917 engineer-in-chief of the Fleet; Miss M. F. Rich, botanist, Queen Mary College, London; Mr. E. H. Richards of the Rothamsted Experimental Station; Dr. Witmer Stone, emeritus director of the Academy of Natural Sciences, Philadelphia.

The second circular issued by the Committee of the International Geological Congress to be held in London from July 31 to August 8, 1940, contains a statement of the subjects to be discussed at the meetings, rules for the presentation of papers and details of the excursions before and after the meeting. The fourteen excursions before the meeting start at various dates from July 13 onwards and will accommodate about 850 persons. The nine arranged to follow terminate on or before August 24 and have accommodation for 775 persons. The inclusive cost is likely to be from £1 5*s.* to £1 10*s.* per day except in the case of the twelve-day trip to the West Highlands and Islands of Scotland, where it is expected that the cost may be £2 per day. All communications relating to the Congress should be addressed to the General Secretaries, International Geological Congress, Geological Museum, London, S.W.7.

The establishment of an International standard of concert pitch was discussed at a conference held under the auspices of the International Standards Association at Broadcasting House on May 11 and 12. Dr. G. W. C. Kaye presided over the meeting, which was attended by delegates from France, Germany, Great Britain, Holland and Italy. It was resolved that the international standard should be based on a frequency of 440 cycles per second for A in the treble clef. This is the standard generally adopted in the United States and does not differ greatly from the 439 used by British piano makers and, since 1927, by British Army bands.

In a letter to *Nature* (May 27) A. J. Turner of the Shirley Institute gave a simple rule for calculating an approximate value of the relative humidity of the atmosphere from readings of ventilated wet and dry bulb hygrometers. Expressed as a fraction the relative humidity is $1-3$ (wet bulb depression)/(dry bulb reading) when temperatures are expressed in Fahrenheit degrees. The formula can be applied to readings in Centigrade degrees by merely adding 18° C. to the dry bulb reading in the denominator of the ratio.

The rule breaks down if the relative humidity is less than 0·3; above that value and for dry bulb readings between 20° and 140° F. the maximum error is ± 4 per cent. Between 50 per cent. and 90 per cent. relative humidity and 40°—120° F. the rule gives values about 2 per cent. higher than those deduced from the standard tables.

The summary of the Report of the Building Research Board for the year 1938 which has been sent to us is not a very informative document. Among other items there is reference to considerable progress of the investigations concerned with sound transmission in buildings: a statement that satisfactory pozzolanas (substances mixed with Portland cement to make it chemically resistant) can now be made in this country from certain clays and from spent material from the Scottish shale-oil industry: a report of further success with the fine spray process for cleaning buildings with reference to certain disadvantages—water on the pavement, possible damage to internal decorations and the need for scaffolding instead of cradles; and a brief account of tests of rain penetration through brickwork. Many other matters are mentioned in the summary and presumably full details are given in the Report, which may be obtained from H.M. Stationery Office, 3*s*. 6*d*. net.

The Report of the Board of Education for 1938 states, *inter alia*, that the number of pupils in State-aided secondary schools was 470,003, an increase of nearly 4000 as compared with the previous year. 81·3 per cent. of the new entrants came from elementary schools and 57·4 per cent. of these were given free tuition. Of the pupils leaving 5·2 per cent. went to the Universities, 4·6 per cent. to training departments, and 11·7 per cent. entered other educational establishments. 58·7 per cent. left for some form of employment, professional or commercial occupations absorbing the majority. No fewer than 437 of the 798 open scholarships awarded at Oxford and Cambridge were won by pupils from these schools and a majority of whom had started in elementary schools. The number of competitors for State scholarships was 6245 and 360 scholarships were awarded. Seventy per cent. of the successful candidates choose to study at Oxford or Cambridge, 18 per cent. at London, and the remainder at other Universities. As compared with 1926 the Oxford and Cambridge percentage increased by 12 at the expense of the " other " Universities. It is doubtful whether this concentration of what should be the best of the secondary school pupils in the two older Universities is advantageous; it weakens the other Universities and probably benefits the older ones very little, since they are amply provided with funds for attracting students of adequate ability. It does not appear, however, that the Board is in any way concerned over the matter.

Leaflet No. 6 in the series issued by the Department of Scientific and Industrial Research on methods for the detection of toxic gases in industry deals with carbon bisulphide vapour (published by H.M. Stationery Office, 3*d.* net). The situations where this vapour may occur in dangerous concentrations include works where artificial silk (viscose), chemicals, coal gas, vulcanized and "dipped" rubber goods, and tar distillation products are manufactured.

In high concentrations it may cause delirium, coma, and death from respiratory failure. The better-known effects, however, are those of a severe chronic poisoning of the nervous system with a great variety of symptoms, varying in degree from slight fatigue and giddiness to serious mental derangement, blindness, and paralysis. The permissible concentration of carbon bisulphide vapour in the atmosphere of workrooms should be kept well below one part in 30,000 of air, and preferably not above one part in 100,000.

The standard method adopted for the detection of low concentrations of carbon bisulphide vapour in industry depends upon its interaction with diethylamine and copper acetate, to produce a coloured compound, copper diethyldithiocarbamate. Samples of the air under test are drawn, by means of a handpump of definite capacity, through a bubbler of alcohol containing these reagents and the colour developed compared with those obtained by adding small quantities of the reagents to dilute alcoholic solutions of carbon bisulphide of known strength. Concentrations down to 1 part in 120,000 can be estimated by the test.

Several papers in recent numbers of the *Journal of Research* of the U.S. National Bureau of Standards are of general interest. The April number contained a paper on the International Temperature Scale, by H. T. Wenzel. It is concerned mainly with the values of the factors required to calculate the constant c_2 in Planck's radiation formula. The more recent and accurate determination of these factors are reviewed and it is concluded that the following values are the best available at present:—Velocity of light, c, $(2{\cdot}99776 \pm 0{\cdot}00020) \times 10^{10}$ cm. sec.$^{-1}$; ice point, T_0, $273{\cdot}16 \pm 0{\cdot}02^{\circ}$ K; gas constant, R, $(8{\cdot}3142 \pm 0{\cdot}0010) \times 10^7$ erg. deg.$^{-1}$C. mole^{-1}; Boltzmann's constant, k, $(1{\cdot}378 \pm 0{\cdot}002)$

$\times 10^{-16}$ erg. deg.$^{-1}$ C.; Faraday, F, 96490 abs. coul. mole^{-1} or $(2{\cdot}8926 \pm 0{\cdot}0002) \times 10^{14}$ e.s.u. mole^{-1}; e $(4{\cdot}800 \pm 0{\cdot}004) \times 10^{-10}$ e.s.u.; Planck's constant, h, $(6{\cdot}61 \pm 0{\cdot}01) \times 10^{-27}$ erg. sec. The corresponding value of c_2 is $1{\cdot}436 \pm 0{\cdot}001$ cm. deg. C. and not 1·432, the value taken for the calculation of temperatures above the gold point on the International scale. The difference is not unimportant since a change of 1 per cent. in the value of c_2 changes calculated temperatures in the neighbourhood of the melting point of platinum by 10° C. and those near the melting point of tungsten by 60° C.

The May number contains an account of Curtis, Curtis and Critchfield's latest determination of the value of the international ampere. The result, 1 int. ampere = 0·99986 abs. ampere, is in excellent agreement with that obtained by Vigoureux at the National Physical Laboratory. There is also an account of the construction and testing of standard resistance coils wound with wire made of gold alloyed with 2·1 per cent. of chromium. These coils appear to be stable and by suitable heat treatment the temperature coefficient of their resistance over the range 18° to 35° C. can be made as small as is desired. A third paper, by Coblentz and Stair, contains an account of the distribution of ozone in the upper atmosphere as determined by six ascents of unmanned balloons carrying ultraviolet-intensity meters and radio-operated barometers. The intensity meter employed was a cadmium photoelectric cell coupled with a radio-transmitter so that both ultraviolet intensity and barometric pressure were recorded graphically at the ground station. The results indicated that the main portion of the ozone is situated between the 20 and 30 km. levels with a region of maximum concentration between 23 and 27 km. The total amount present is equivalent to a layer 2·5 mm. thick at atmospheric pressure and of this about 0·9 mm. exists at heights above 27 km. The maximum altitude attained by the balloons was 26·5 km. (88,200 ft.), and at this level the intensity of the ultraviolet radiation below 3132A was about ten times as great as indirect sunlight at sea-level.

The June number of the Journal contained a paper by Scott and Curtis concerned with edge corrections in the determination of dielectric constants with parallel plate condensers. Theoretical and empirical formulæ are discussed and details of experimental tests given. It is concluded that the most accurate method for the determination of dielectric constants, apart from the guard-ring method, is that using one sheet of dielectric and having one of the parallel plate electrodes appreciably smaller than the other. Part of the accuracy with this arrangement is due to the fact that the electrodes do not need accurate centring.

Special Report No. 5, issued by the Forest Products Research Laboratory (H.M. Stationery Office, 9*d.* net), deals with the effects of growth and structure on the properties of wood. It has usually been supposed that growth-rate, the proportion of summerwood in the growth-rings and specific gravity are the chief factors which determine the strength of timbers, but studies of typical hardwoods have shown that in some cases the physico-chemical composition of the fibre-walls may have a greater influence on strength than all the other factors put together.

Up to the present it has not been possible to define the variations in cell-wall composition in either chemical or physical terms, but they are distinctly recognisable by means of the standard histological reagents of the botanist, and some indications have been obtained of the types of growth condition responsible for them. It has been found, for instance, that the condition of the cell wall that makes for a high resistance to longitudinal compression is dependent on climate and that on the whole tropical timbers are stronger in this respect than temperate timbers. Resistance to shock appears to behave in the reverse manner, the temperate zone timbers being tougher than tropical timbers, weight for weight. The composition of the wood substance is also influenced by quite small deviations of the tree from the vertical. In sloping trees there are important differences in strength, shrinkage, and machining properties between the wood from the upper and lower sides, and these differences have been traced to the composition of the fibre walls.

REVIEWS

MATHEMATICS

The Geometry of Determinantal Loci. By T. C. Room, M.A. [Pp. xxviii + 483.] (Cambridge: at the University Press, 1938. 42*s.* net.)

Those who have long awaited Prof. Room's compendious work will not be disappointed. We have here just the full account of this important branch of geometry which was hoped for. It is dry and often heavy reading; but that seems to be inevitable, and it is as well relieved as possible by numerous examples applying the general results to familiar loci of fairly low order and dimensions.

The topic of loci whose equations can be expressed by the vanishing of all minors of a certain order in a certain matrix might seem to the uninitiated too specialised to be of general interest; but when it is realised that this class includes almost all loci about which anything particular and elementary is known, impatience vanishes. The determinantal form is directly related to a projective generation analogous to that of a conic by two related pencils, and it is from this point of view that Prof. Room mainly treats the subject, though liberal use is made of algebra where it shortens or clarifies the argument.

The work is in three nearly equal parts: the first gives the general theory; the second applies it to various loci of interest, such as the Grassmannians of lines, the generalisations of the Veronese surface, and some loci generated by linear series on elliptic curves; and the third deals with determinantal quartics and the Bordisa surface in four dimensions.

The author's notation is elaborate and consistent throughout the book, as may be exemplified by the presence of four differently indicated summation conventions. He still uses the curious notation of reversed brackets)like this(to indicate stars; it does not seem quite so confusing as it did in some of his earlier papers, whether because I am used to it or because he uses it more discreetly, but it is still tiresome not to know at once, when one sees a bracket, whether to look before or behind for its fellow.

The printing seems excellent; I have only found one misprint—an interchange of suffixes on p. 173. Two pages of index seem a little meagre in a work whose main value will probably be for reference.

Patrick Du Val.

Introduction to Bessel Functions. By F. Bowman, M.A., M.Sc. Tech. [Pp. x + 135, with 21 figures.] (London: Longmans, Green & Co., 1938. 10*s.* 6*d.* net.)

This book should be of considerable use both to the engineer or physicist who is concerned with applications of Bessel functions and to the mathematical student as an introduction to the more advanced theory.

The treatment is clear and simple throughout and with the possible exception of the chapter on the asymptotic expansions of the functions of zero order, which requires some acquaintance with the methods of contour integration, the book should be well within the grasp of the reader whose mathematics has reached the subsidiary standard.

The first four chapters are devoted to the properties of the ordinary and modified Bessel functions of zero order as far as the Fourier-Bessel expansion, and applications to some physical problems are fully worked out.

Asymptotic expansions of these functions are obtained in the next chapter and the last two are concerned with the properties of $J_n(x)$ and applications to some well-known problems.

The examples given at the end of most sections are a valuable addition to the text and there is a table of values of $J_0(x)$ and $J_1(x)$ at unit intervals from $x = 0$ to $x = 14$, and of the first five roots of $J_n(x)$ for integral values of n up to $n = 5$.

L. A. W.

Advanced Mathematics for Engineers. By H. W. Reddick and F. H. Miller. [Pp. x + 473, with 130 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 20*s*. net.)

The authors have constructed this book from courses they have given at the Cooper Union Institute of Technology; it is meant for engineers who have mastered the usual calculus course. The first half deals with ordinary and partial differential equations. The methods of solution are clearly explained and illustrated in detail by applications drawn equally from the four main branches of engineering. There are plenty of exercises, with answers, for the reader. Fourier series, elliptic, gamma, and Bessel functions are made part of an evolving technique for the solution of differential equations, and the authors have done wisely to relate their treatment intimately to certain specific engineering problems. A chapter on vector calculus (theorems of Stokes, Gauss, Green) is followed by an introduction to functions of a complex variable including Cauchy's integral theorem and the Schwarz-Christoffel transformation. The theory is applied to engineering problems such as equipotentials in a conducting sheet and, on the other hand, to evaluating real integrals by the method of residues. It is again used in an interesting chapter on Heaviside's operational methods and Bromwich's account of them by means of contour integrals.

The authors do not shrink from discussing topics like uniform convergence, but they are tactful in not burdening the engineer with excessive mathematical rigour. Where the matter is really deep they enunciate precise general theorems and indicate sources of proof. In one or two such places errors have crept in; but the general impression is one of care, accuracy, and lucidity.

H. K.

Elementary Mathematical Statistics. By William Dowell Baten, Ph.D. [Pp. x + 338, with 49 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 15*s*. net.)

This book covers the groundwork of mathematical statistics in a comprehensive manner. The first five chapters are devoted to introducing the better-known descriptive statistics. The remainder of the book contains elementary probability, theory and sampling theory, correlation analysis and an introduction to the use of the analysis of variance, together with chapters on index numbers and on the analysis of time-series.

A useful collection of tables is given, including a reproduction of the tables of significance levels for variance ratios from Snedecor's *Analysis of Variance.*

The author's aim, as stated in the preface, is to develop the subject without employing the differential and integral calculus. In this he succeeds, albeit at the cost of cumbrous methods on occasion. A peculiar feature of the book is that, although elementary tests of significance are introduced, no mention is made of the χ^2 test in any connection whatever. One would have expected some consideration of χ^2, both from the instructional and historical viewpoints.

A minor flaw is a certain inconsistency of notation. In Chapters 1 to 13 $\bar{x}$ represents a deviation from a mean, while in Chapter 14 it represents a mean.

There is an ample supply of illustrative problems to be worked by the student. In addition, the text is fully illustrated by worked examples. Although there are occasional lapses from clarity of expression, each section of the subject considered is treated thoroughly. On balance it may be said that this book is a reasonably straightforward introduction to mathematical statistics, but that its usefulness to readers in England will probably be limited.

N. L. J.

ASTRONOMY

Stellar Dynamics. By W. M. SMART, M.A., D.Sc. [Pp. viii + 434, with 75 figures.] (Cambridge: at the University Press, 1938. 30*s*. net.)

As stated on the cover, Prof. Smart's book is essentially mathematical in character, and indeed the author has achieved a very full and lucid account of the various mathematical aspects of the dynamics of stellar systems. For people commencing research in this domain the book will be invaluable, and will save a lot of reference to the original papers, but anyone meeting the subject for the first time will probably find the main ideas somewhat obscured by the mass of rather technical mathematics. In his *Stellar Movements and the Structure of the Universe*, Eddington says: "At first sight it might seem possible to divide the subject into compartments . . . but it is not possible to pursue these different branches of inquiry independently. . . . We have therefore adopted the unusual course of placing what may be regarded as a summary thus early in the book." No doubt Prof. Smart is writing for readers who already have such a summary in their minds, but as his book will rank as a standard treatise on the subject it is perhaps to be regretted that he has not included some non-mathematical discussions of the main assumptions and ideas involved in the various chapters.

The first part of the book is devoted to a very full discussion of the phenomenon of star streaming and the various methods employed are amply discussed and illustrated. Next come the ways of deriving parallaxes and space distribution from the observed proper motions. The latter half of the book deals with general stellar systems—general statistical theorems, clusters and galactic problems. The chapters on Galactic Rotation and Dynamics are particularly stimulating, but one cannot help feeling that the models of the Galaxy which can be discussed mathematically may be very different from the actual state of affairs. A very useful feature of the whole book is the numerous references to the original papers, which will certainly make it an invaluable source book for those interested in the problems of stellar dynamics.

H. R. H.

PHYSICS

The Principles of Statistical Mechanics. By R. C. TOLMAN. The International Series of Monographs on Physics. [Pp. xix + 660.] (Oxford: at the Clarendon Press; London: Humphrey Milford, 1938. 40*s*. net.)

THIS book aims at providing an exposition of the principles of statistical mechanics without discussing detailed applications to physical phenomena. The method used is the deductive one, starting with certain clearly defined postulates from which a detailed theory is developed. *A posteriori* justification is provided by the agreement with observation of the deductions from this theory. It is a particular merit of the book that the postulatory basis is most clearly specified, and although, in accordance with the author's scheme, no attempt is made to justify the postulates with logical rigour, the plausibility of the assumptions made is discussed in detail.

The text is in three main divisions. In the first part (Chapters II–VI) the classical treatment is developed. The necessary generalised dynamics is introduced in Chapter II and the fundamental postulates admirably discussed in terms of Liouville's Theorem in Chapter III. Chapter IV provides a treatment of systems in equilibrium by the method of the microcanonical ensemble, while in the next two chapters a very thorough discussion of the change of systems with time, leading to Boltzmann's H-theorem, is given. The second part (Chapters VII–XII) is a corresponding quantal development. Chapters VII and VIII deal with quantum mechanics and are well worth reading apart from any application to statistical mechanics. Chapter IX is concerned with the quantum analogue of Liouville's theorem and the classical postulates leading, in the next chapter, to the treatment of systems in equilibrium. The treatment of systems varying with the time given in Chapters XI and XII is most satisfactory and fills a serious gap in literature in this direction. In the final part (Chapters XIII and XIV) the correlation between statistical mechanics and thermodynamics is examined in detail from a point of view closely resembling that of Gibbs but adapted to a quantal outlook.

Apart from its importance in providing the first systematic exposition of statistical mechanics from the standpoint of Gibbs, this book can be thoroughly recommended to all mathematical physicists as it is a mine of information on all aspects of the subject and, while following one particular mode of approach, never fails to discuss the relation to other methods.

H. S. W. M.

Theoretical Mechanics: A Vectorial Treatment. By CARL JENNESS COE, Ph.D. [Pp. xiv + 555, with 102 figures.] (New York: The Macmillan Company, 1938. $5.00.)

DURING recent years there has been an ever-increasing use of vectors in the presentation of more than one branch of applied mathematics. Consequently teachers of mechanics who have wished to introduce vectors at the commencement of their courses have been faced with a very real difficulty in the matter of a suitable text-book. They have been unable to recommend to their classes a book from which "the student learns the vector analysis almost incidentally in the study of mechanics and learns it well because he not only studies it directly but continues to use it thereafter," to quote from the preface to Prof. Coe's *Theoretical Mechanics*. This book is certainly one we can recommend for this purpose.

After two chapters dealing with the elementary principles of mechanics and one-dimensional problems, the author introduces the vector analysis and afterwards uses it throughout. He deals with particle and rigid body dynamics and statics, including an interesting chapter on Virtual Work. A chapter on the General Principles of Mechanics takes the student as far as Legrange's and Hamilton's Equations and the principle of Least Action. The remaining chapters of the book are concerned with an introduction to the vector calculus and its use in Potential Theory. A useful selection of examples concludes each chapter.

Prof. Coe defines a vector quantity as being characterised by three ordered numbers and not from the viewpoint of a directed segment. Consequently co-ordinates are used when a strictly vector or geometrical treatment might have been preferable. For example, a vigorous vector proof of the expansion of a triple vector product would have been most instructive to the student. But this and a few similar points are minor criticisms which in no way detract from our opinion that this book should find a place in the library of every applied mathematician.

R. C. K.

Grimsehls Lehrbuch der Physik. Band I: Mechanik—Wärmelehre—Akustik. Neubearbeitet von Professor Dr. R. Tomaschek. Tenth edition. [Pp. viii + 681, with 740 figures.] (Leipzig and Berlin: B. G. Teubner, 1938. Price abroad RM. 14.85.)

In reviewing a book of this nature the reviewer's primary duty is clearly to indicate its standard, which is approximately that of an Intermediate textbook with special emphasis placed upon certain aspects of applied physics, although in some particulars the standard approaches more closely to the first-year B.Sc. course in an English university. For example, much attention is paid to the motion of bodies through fluids, the physics of "streamlining" and of aeroplane and glider flight. Further, the distinction between group velocity and wave velocity, which is usually omitted from our intermediate courses, is clearly elaborated in this book, and, incidentally, is excellently explained with the help of a very instructive diagram.

The whole of the subject-matter is set forth in a clear and readable manner, the important features being most carefully stressed and lavishly illustrated with photographs and figures. Indeed, one can honestly say that every teacher of Higher School Certificate or intermediate physics will find something to arouse his interest in this volume, and as an aid to the study of scientific German it should be most valuable.

In the back of the book is inserted an interesting folder giving data of the lives of distinguished scientific personalities from Leonardo da Vinci to Hasenöhrl. Two notable omissions would appear to be Madame Curie and Lord Rutherford; for, surely, the discoverer of radium and the discoverer of the laws of radioactivity and of artificial disintegration should be honoured in our generation.

L. F. B.

Theoretical Hydrodynamics. By L. M. MILNE-THOMSON, M.A., F.R.S.E. [Pp. xxiv + 552, with 4 plates and 330 diagrams.] (London: Macmillan & Co., Ltd., 1938. 31*s.* 6*d.* net.)

IN this book Mr. Milne-Thomson has devoted his attention mainly to giving an account of the mathematical theory of the motion of *inviscid* fluids. Chapters I and III are concerned with the general properties of inviscid flow, Chapters IV, VI–XIV deal with two-dimensional problems including two-dimensional flow past aerofoil shapes, the drag arising from the Kármán vortex street, and wave motion. Chapter XV is devoted to three-dimensional problems in which there is axial symmetry, and Chapter XVI is concerned with the general motion of spheres and ellipsoids. In Chapter XVII a definition of vector differentiation is formulated and applied to Kirchhoff's equations. Vortex motion is considered in Chapter XVIII, which contains a very brief account of the motion of an aerofoil of finite span.

The author emphasises his consistent use of the vector notation. The application of vector analysis to hydrodynamics is developed *ab initio* in Chapter II. Whilst in many places this notation combines conciseness with clarity, difficulties arise unless one is prepared to continue and introduce tensor notation for such purposes as the analysis of the rate of strain. Unfortunately the author has not permitted himself this extension.

Mr. Milne-Thomson says that he attempted to keep the book reasonably self-contained. With this end in view in addition to the introductory chapter on vector analysis, Chapter V is devoted to introducing complex variable theory, as far as it concerns hydrodynamics, from first principles.

The last chapter—XIX—is devoted to a study of the effects of viscosity on fluid motion.

Any criticism that is made must be concerned largely with material omitted. Considerable progress has been made in recent years on problems of viscous motion and some more detailed treatment might well have come within the scope of a work on theoretical hydrodyamics. Problems connected with turbulent flow and with the effects of compressibility on high-speed flow are not discussed.

The text is written with great care and is illustrated by many good and clear diagrams.

L. H.

Advanced Experiments in Practical Physics. By J. E. CALTHROP, M.A., M.Sc. [Pp. xx + 121, with 2 plates and 170 figures.] (London: William Heinemann, Ltd., 1938. 8*s.* 6*d.*)

THIS book provides a selection of forty-eight experiments in advanced physics suitable for students in their final year who are preparing for an Honours Degree. In his introduction the author gives useful advice on the simple personal equipment which every student of advanced physics should possess, and emphasises the need for acquiring skill in laboratory arts such as soldering and glass-blowing. His remarks on the recording and treatment of experimental data and the need for attention to the units and dimensions of the result might, with advantage, be displayed on the notice-board of every physical laboratory. Brief but clear instructions are coupled with a generous supply of explanatory diagrams and only essential theory is given. Many of the exercises provide excellent illustrations of the value of graphical treatment.

The experiments are grouped in three sections: Properties of Matter and Heat; Light; Electricity. It is to be regretted that the author has not included one or more experiments on Sound, a subject which often receives scant attention in the advanced laboratory. In view of its importance in modern applied physics an experiment on the measurement of sound intensity would have been a valuable addition.

Section I includes exercises on the elasticity of solids, the viscosity of liquids, the vacuum pump, surface-tension, specific heat, thermal conductivity and emissivity. The experiment on the estimation of the temperature of a bunsen flame is a useful application of the reversal of the D lines of sodium. In Section II interferometry is treated at considerable length. The instructive experiments with a Fabry and Perot étalon are carried out with an instrument which the student can make for himself from two good glass flats. The Michelson interferometer is used to measure the separation of the D lines of sodium, but few laboratories are fortunate enough to possess this costly instrument. An exercise on the photography of spectra with a spectrograph of small dispersion is added, together with an explanation of the method of calculating the wave-length of unknown lines.

In Section III a balance has been preserved between classical experiments and those based on modern electron developments. The measurement of critical potentials together with some applications of the valve and photoelectric cell are described.

Radio-activity is somewhat incompletely represented by experiments on β and γ rays only. A number of experiments with the X-ray spectrometer and the cathode ray oscillograph complete a valuable course of practical work which can be warmly recommended to both students and teachers.

H. B. K.

A Text-book of Thermodynamics. By F. E. Hoare, Ph.D., M.Sc., A.R.C.S., D.I.C. Second edition. [Pp. xii + 307, with 49 figures.] (London: Edward Arnold & Co., 1938. 15*s*. net.)

This, to all intents and purposes, is the same book as that which first appeared in 1931, the textual additions and alterations being few. A definitely new feature is the introduction of some 23 pages of examples, arranged in groups corresponding to the subject-matter of each chapter. A useful appendix gives the values of certain of the fundamental physical constants. The book in the main covers the Thermodynamics of an Honours Degree in Physics. It is particularly valuable for the discussions of the principal applications of Thermodynamics to Physical and Physico-chemical problems. The collection of examples, many with hints for solution, will be of great assistance to all students and help to give reality to a branch of Physics which, to many, is full of subtleties. The printing and general lay-out of the book are excellent.

S. Marsh.

Principles of Electricity and Electromagnetism. By Gaylord P. Harnwell. International Series in Physics. [Pp. xiv + 619, with 383 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 30*s*. net.)

The radio-engineer will find here all that he requires in the principles of electricity, and the treatment is sufficiently thorough for a student of physics to obtain a good knowledge of the subject. The mathematical appendix gives the necessary treatment of elementary differential equations and of vector analysis together with a section on units and standards. As the author says, "emphasis is placed on the experimental aspect rather than the theoretical." It is chiefly with modern experimental work that the book is concerned, particularly with oscillations and the transmission and reception of waves.

It is reasonable that in a book written primarily for students in the U.S.A., the references given should be chiefly to American sources. It might, however, be noted that the falling-drop method of measuring the electronic charge was not entirely due to Millikan, but that it grew out of the work of C. T. R. Wilson. Similarly, that the bridge method of comparing a capacity with a resistance was due to Maxwell.

The system of units adopted is the kilogram-meter-second system, but occasionally the ordinary electrostatic and electromagnetic units appear, and when this is the case it is indicated fairly clearly.

Among the important features of the book are the treatment of electromagnetic theory with its application to various forms of vacuum tubes and valves with their use in radio-circuits and an extensive selection of problems given at the end of each chapter.

The student of physics will find the treatment of such subjects as electrolysis, thermo-electricity, etc., rather meagre ; in fact, these two, together with photo-electric effects, are all included in one chapter of about 30 pages.

S. G. S.

Physics for Technical Students : Sound, Electricity and Magnetism, Light. By William Ballantyne Anderson, Ph.D. Third edition. [Pp. x + 436, with 308 figures, including one plate.] (New York and London : McGraw-Hill Publishing Co., Ltd., 1937. 15*s.* net.)

Prof. Anderson's book, of which the volume under review is the second part, is a survey of intermediate physics rather than a text-book. As the title indicates, more applications are described than usual, but the technical student will not receive the thorough grounding in the fundamentals of physics he really requires before he can apply the subject. To give but one example, there is no mention of the potentiometer. However, it is stated in the preface that this book is designed to be accompanied by lectures as well as laboratory work.

The first 47 pages deal briefly with sound, especially in relation to music. (According to Chapter XXV, Kundt's tube is a musical instrument !) The treatment of magnetism is superficial ; for example, no method of measuring the horizontal component of the earth's field is given, and there is a danger that a student reading the last two sentences of Chapter XXVII will be left with the impression that there is really a bar magnet at the earth's centre. Among the good points of the electricity section are the interesting descriptions of a telephone system, dynamos, etc., and the introduction of alternating current methods at appropriate places.

The treatment of mirrors and lenses is based on the wave or "curvature" method, and the emphasis is on physical optics throughout the section on light. The "incident light positive" sign convention is used. Finally, there are short chapters on radioactivity, "recent physics," and radio, much of which intermediate students will find interesting but difficult. At the end of each chapter there is a review of vital facts and a collection of short examples (no answers) which appear to have been carefully chosen. The mathematics is nowhere beyond matriculation standard, and most of the text is very clearly worded. A few minor errors still remain in this edition.

Ultrasonics. By Dr. Ludwig Bergmann. Translated by Dr. H. Stafford Hatfield. [Pp. x + 264, with 148 figures.] (London: G. Bell & Sons, Ltd., 1938. 16*s*. net.)

The rapid progress in the science and technical applications of sound is nowhere more manifest than in that branch known as supersonics, or ultrasonics. The range of frequencies included in the term ultrasonics may be regarded as extending from, say, 10,000 cycles per second to the highest conceivable values. At present the upper limit of frequency lies in the region between one and ten million c.p.s., but in view of recent progress in the study of molecular phenomena by this means it will not be very surprising to see still higher frequencies used in the near future.

During recent years, the number of published papers has increased enormously as a result of a world-wide interest in the subject. Apart from a few text-books giving very brief summaries, there are few, if any, reliable books of reference in which the scattered literature has been collected together and presented in a logical sequence. This situation was remedied by the publication of Dr. Bergmann's book first in German in 1937 and now in an English translation. For the latter we are indebted to Dr. H. Stafford Hatfield. As an illustration of the rate of development of the subject an appendix to the book contains a list of about a hundred references to papers published during the interval between the appearance of the German and English editions, *i.e.* between July 1937 and June 1938. The original German edition, and now the translation, contains in addition to this a list of about 500 references. The subject is treated mainly from the experimental point of view, but the theoretical side is by no means neglected. The book is divided into sections which deal with (1) the generation of ultrasonics—mechanical, thermal, magnetostriction and piezo-electric methods ; (2) detection and measurement —mechanical, thermal, electrical and optical ; (3) measurement of velocity and absorption of sand in liquids and gases ; (4) velocity in solids—elastic constants and (5) applications.

The sections dealing with optical methods of observation by striation and diffraction methods are particularly good and include the author's own researches on the subject. In the section on applications, reference is made to the Langevin piezo-electric oscillator for marine depth sounding but not to the British Admiralty magnetostriction type. The application of ultrasonics to television—the basis of the " Scophony " system—is an interesting example of recent technical progress. (This section is due to Dr. Hatfield, the translator.)

Dr. Bergmann's book provides a very useful source of reference to a subject which is in a state of rapid growth.

A. B. W.

The Physical Properties of Colloidal Solutions. By E. F. Burton, B.A., Ph.D. Third edition, prepared with the assistance of May Annetts Smith, M.A., Ph.D. Monographs on Physics. [Pp. viii + 235, with 36 figures.] (London, New York, Toronto: Longmans, Green & Co., 1938. 15*s*. net.)

Many books on colloidal solutions provide a wealth of facts and often a minimum of theory. In this volume the balance is restored. The principles of the subject find a prominent place, and the facts are chosen to illustrate the application of the principles. While stress is laid on the physical properties of colloidal solutions, this arrangement will give the student, for whom the book is mainly intended, a much better grasp of the subject. It also makes it easier for him, later on, to consult the larger works on colloidal chemistry.

The book has undergone very extensive modification since the appearance of the second edition no less than seventeen years ago, but its general character remains unaltered. After a brief historical introduction and a summary of the various methods of making colloidal solutions, there is a short chapter discussing the part played by molecular forces in determining the stability of colloidal particles. The Brownian movement is next dealt with critically at great length—a most valuable feature of the book. Then follows an excellent chapter on the various methods of determining Avogadro's number. The theory of the ultra-centrifuge is of course an entirely new part of the book. While realising the commendable purpose of the book a little more of this aspect of the subject would have been welcome. Electrokinetic phenomena and the associated effect of electrolytes on the precipitation of colloids complete the volume, which is eminently suitable for chemists, physicists and biologists.

H. W. MELVILLE.

Ions, Electrons, and Ionising Radiations. By J. A. CROWTHER, Sc.D., F.Inst.P. Seventh edition. [Pp. xii + 348, with 117 figures, including 7 plates.] (London: Edward Arnold & Co., 1938. 12*s.* 6*d.* net.)

ALTHOUGH the present edition of this very successful book is the seventh, a total of twelve editions and reprintings have appeared since the original publication of the book in 1919, so there can be no questioning its utility, especially to students of physics, to whom it is primarily addressed.

Any book dealing with such a progressive subject is naturally bound to undergo many changes in this period, especially when one considers the many researches and discoveries, experimental and theoretical, of the past twenty years, but as the author points out, the book's purpose remains unaltered, *viz.*, to provide students with a systematic knowledge of the later developments in atomic physics, together with a substantial outline of the present position of the subject, and to correlate its different branches.

As in earlier editions, the mathematical treatment is simple enough for a second-year Honours man, but throughout Professor Crowther emphasises important physical principles and does not distract attention from them by complicated mathematical analyses. Such a treatment should appeal to all. The descriptions of the earlier work stand as they were first written, but minor additions and alterations in parts bring the book up to date, while the section on Cosmic Rays and the chapter on the Nucleus have been rewritten and necessarily enlarged. The difficulties associated with the presentation of important results and theories encountered in these branches are evident to all, but the author has made a wise and useful selection.

The new edition, which will be welcomed with unreserved pleasure, is a pattern of compression, in fact, to survey such a wide subject in 350 pages so satisfactorily is a feat of which the author may well be proud.

F. H. NEWMAN.

The Mobility of Positive Ions in Gases. By A. M. TYNDALL, D.Sc., F.R.S. Cambridge Physical Tracts. [Pp. x + 93, with 35 figures.] (Cambridge: at the University Press, 1938. 6*s*. net.)

THIS is far from being an exhaustive report of all work done on the subject, fortified, *more teutonico* by one page of bibliography to five of text. Instead it conforms admirably with the purpose of the new series of Cambridge Physical Tracts, which is to treat certain subjects from the personal points of view of those actively engaged in the researches. No subject could be better suited to this kind of treatment than the work on ion mobilities which Prof. Tyndall has been carrying on at Bristol for the last fifteen years or so.

After a brief introduction in which are disclosed some of the difficulties as to the actual constitution of ions in gases and the possible effects of impurities, Prof. Tyndall quickly introduces us to the methods devised by himself and his collaborators. These methods had for their objects in the main three purposes: first, to produce ions of which the constitution could not be doubtful; second, to drive these ions through pure gases; and third, to measure accurately the velocity of the ions by a method which could separate, if necessary, groups of ions lying closely together. A high measure of success has attended experiments made with ions of the alkali metals in the more inert gases, and the agreement with theory has been very encouraging. But many problems remain and the author is bound to confess that the precise nature and constitution of the ordinary small ion of the atmosphere is as yet obscure.

J. J. N.

Atomic Structure. By LEONARD B. LOEB. [Pp. xvi + 446, with 111 figures.] (London: Chapman & Hall, Ltd., 1938. 22*s*. 6*d*. net.)

THERE is need for a book which sets out the fundamentals of atomic structure without the detailed rigour of a standard work such as that of Sommerfeld, but Prof. Loeb has not met this need in an entirely satisfactory manner.

If a book of this kind is to be at all complete, it cannot fail to take account of the wave mechanical treatment. It should not be impossible to introduce, in a simple manner, wave mechanical methods, which need not be abstract, as the author suggests, and would certainly be less empirical than his approach. In an attempt to bring his treatment into alignment with modern ideas, the author introduces formulæ from wave mechanics without apparent reason, and selection rules appear with scarcely more justification. The mathematics and the ideas required for such a method would be less difficult than those employed in the final chapter on the theory of metals.

Apart from this general criticism, the book is very uneven in quality. Some parts are extraordinarily well written while others are disappointing. Information from various sources has, quite rightly, been used, but has not always been welded into a compact whole. There are several mistakes and a carelessness which often leads to confusion. Two examples must suffice. In a diagrammatic representation of the trace on the photograph film in Bucherer's experiment, the trace is shown as extending throughout the whole 360° for a fixed orientation of the fields. Secondly, in the treatment of the Bohr theory and the vector model the notation used for the twenty-three quantum number symbols introduced is inconsistent and confusing, and the same symbol, without any distinguishing suffix, is used to represent different quantities; for instance, in related formulæ m is used to represent a quantum number, a mass, and a magnetic moment.

In spite of the above criticisms there are parts of the book where the exposition is exceedingly clear, and the historical perspective enhances the interest of the subject.

W. E. D.

Stream and Channel Flow. By E. E. MORGAN, M.C., A.M.Inst.C.E. [Pp. xxii + 240, with 5 figures and 12 graphs.] (London: Chapman & Hall, Ltd., 1938. 25*s.* net.)

THIS is a series of tables conveniently set out for the rapid calculation of the dimensions of open channels to convey water. Based on the simple and reliable formula of Manning, values are given for channels of various shapes and for three different roughnesses well enough defined by the usual lists of the various types of construction, though by a curious omission some of these lists have been left in their original form and must undergo a tedious conversion before use with the tables. The estimation of run-off is briefly summarised. The important question whether the channels so designed will be stable or not is cursorily dismissed in the last two pages of the volume.

Some of the tables seem redundant; square roots, for example, are better found by slide-rule; and one hardly needs a chart, and not too clear a one at that, to convert feet-per-second to feet-per-minute. Why must the compilers of such tables invest them with a false suggestion of accuracy by using redundant places of decimals: in some of the present ones there are six figures where it would be difficult to justify the use of more than two, and the remaining ones serve only to confuse and delay the user. Otherwise the tables are well set out and clear. But this is more than can be said for the graphs; for example, that on p. 227 is so carelessly drawn that two errors are seen at the first glance; and the scales of all are reminiscent of ancient maps.

In the many pages of explanatory matter much is quoted from earlier sources. Here, a little editing and collation would have helped the reader who, if unfamiliar with the subject, is left without guidance as to how much is acceptable to-day. A chapter deals with recent research, but here the author is perhaps a little out of his depth, and fails to grasp the opportunity of showing how the work of the past ten years explains and co-ordinates the older matter. Still, it is for the tables and the lists of numerical data that the book will be bought, and an elaborate thumb index assists the reader to sort these from the remaining 150 pages.

C. M. W.

Mechanical Engineering Science: A Second Year Course. By ARTHUR MORLEY, O.B.E., D.Sc., M.I.Mech.E., and EDWARD HUGHES, Ph.D., D.Sc., M.I.E.E. [Pp. xiv + 260, with 77 figures.] (London, New York, Toronto: Longmans, Green & Co., 1938. 4*s.* 6*d.*)

THIS book, specially written for part-time students in their second year of the National Certificate Course, is a comprehensive treatise in mechanical engineering science. The subject-matter of each section has been carefully presented and thoroughly illustrated by a large number of representative examples.

In the section on mechanics it is noted that the authors determine change of velocity by the vector subtraction method, whereas in Dr. Morley's previous work the vector addition method was mostly employed at this stage. It is also noted that equation (3), p. 35, gives a well-known equation in a somewhat unusual form. It reads as follows:

$$\text{P in pounds} = \frac{\text{W lb. force}}{g \text{ ft./sec./sec.}} \times f \text{ ft./sec./sec.}$$

This equation is unusual because W denotes lb. force. No doubt the authors have good reasons for this change, but it would seem preferable to have written W lb. mass or W lb. weight. The young student may be confused by the expression W lb. force in this equation. Furthermore, equation (4), p. 36, appears to contain a clerical error. It should read as under :

$$\frac{\text{Pounds force}}{f \text{ ft./sec./sec.}} = \frac{\text{Pounds mass}}{32{\cdot}2}.$$

The chapters on elementary thermodynamics and heat engines are arranged to provide a version which is truly representative of modern conditions. The steam engine now takes its proper place as one of several equally important power-producing units. This is a good feature. Furthermore, the treatment of the internal combustion engine has been extended a little beyond the average requirements for second year. The ideal cycle, with combustion at constant volume as in gas and petrol engines, is an interesting and useful extension, while in addition the authors have, frequently, used the kinetic theory of gases in explaining the various changes in the physical condition of the working fluid.

There appears in this section on p. 168 an error : " through a small fraction " should read " though . . ."

This book will be welcomed by both teachers and students.

W. A. M.

Engineering Mechanics. By SEIBERT FAIRMAN and CHESTER S. CUTSHALL. [Pp. xii + 267, with 376 figures.] (New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1938. 13*s.* 6*d.* net.)

THIS should prove a valuable text-book for teaching purposes, and that is its avowed object. The ground covered is extensive, taking in all the usual range of statics and dynamics, including the motion of rigid bodies. The treatment is nowhere exhaustive, but that was deliberate on the part of the authors, who write, " instead of planning a comprehensive treatise on the subject, the authors have attempted to plan a text-book which can be covered in the time usually allotted to the subject." This they have done very well, but the book definitely wants a teacher to accompany it. The exposition is very clear, but necessarily brief, and all the fundamental material is there. The arrangement is good : " all the necessary tools " are presented " at the beginning." A valuable feature is the large collection of problems of a practical nature, both worked and unworked, and in the former case the solutions are set out very clearly. The make-up of the book is excellent.

J. C.

Mechanics of Materials. By P. G. LAWSON, M.S., and W. J. COX, C.E. [Pp. x + 408, with 467 figures.] (New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1938. 18*s.* 6*d.* net.)

THE general standard of this book may be described as somewhat higher than pass degree standard in this country. In addition to elementary theory, many aspects of practical design are considered, so that the work constitutes a combination of elementary strength of materials with elementary structural design. On this account the diagrams in the text and the examples have a much more practical appearance than those in many similar books. Such diagrams are always a help to the student in grasping what an author is endeavouring to explain. With one or two exceptions, none of the theoretical treatment calls for extensive mathematical knowledge, never anything more advanced than the second order differential equation as used in the simple theory of deflections and struts. In this connection some of the work might have been made easier by more use of the method of superposition of loads. The treatment of failure under combined stresses might with advantage have been more extensive, and have included recent experimental results : three pages seems very inadequate for this important subject. In the same way one would have liked to find more about fatigue in a book of this practical nature, but it is good to see that the importance of stress concentrations is recognised. A valuable feature of the book is the collection of miscellaneous problems at the end, problems involving at once several of the theoretical principles which have been treated, so that the student must use some judgment and not merely substitute in a formula derived in the preceding paragraph.

A very good book within the range aimed at.

J. C.

The Strength of Materials. By JOHN CASE, M.A. Third edition. [Pp. viii + 602, with 427 figures.] (London : Edward Arnold & Co., 1938. 30*s.* net.)

BOOKS on Elasticity fall generally into two simple classes—those that are too mathematical for the engineer and those that are of too engineering a flavour for the mathematician. To bridge this gap successfully is something of an achievement, for it implies that neither the engineering content nor the mathematical form must be sacrificed. The present volume by Mr. Case approaches as closely to this ideal as could be desired ; the fact that a third edition has now been produced is in itself a public recognition of its success. It has won its place by sheer merit alone.

This arises not merely because Mr. Case is so obviously a painstaking teacher with a real appreciation of what is of mathematical and what of practical importance, but also because his successive editions have always been brought right up to date as regards the most recent work on the subject. In this edition the author has seized the opportunity of rewriting the chapter on the failure of materials under steady compound stresses, to bring it completely into line with the modern viewpoint. Underlying it all is a clear philosophic and scientific appreciation of how the general laws of physical change are manifested in the particular class of problem with which the author is concerned. To meet the needs of students at different stages, a programme of study for first and second year and for advanced students is outlined. The book contains about 400 examples distributed in representative fashion at the end of each of the 34 short chapters of the book, in addition to many worked out in detail in the text. A complete set of answers is provided. Attractively produced with a very full index and 427 diagrams, the book can be heartily recommended both on general scientific and on specialist grounds.

H. LEVY.

Surveying and Field Work. By JAMES WILLIAMSON, M.Inst.C.E. Second edition. [Pp. xxvi + 511, with frontispiece, 6 plates and 316 figures.] (London : Constable & Co., Ltd., 1938. 20*s.* net.)

THE second edition of Williamson's *Surveying and Field Work* is a sound, practical book, clearly written and well illustrated. It has been brought up to date by the inclusion of illustrated descriptions of the newer types of level and theodolite, but still retains particulars relating to the use and adjustment of the older forms, large numbers of which, of course, are still in use.

In connection with the descriptions of surveying apparatus, it may be remarked that chains of the ordinary type are provided with indicating tags at 10-foot or 10-link intervals only, and not at 5-unit intervals as shown in Figs. 9 and 10, and, so far as English practice is concerned, the Sopwith levelling staff has become completely standardised to the exclusion of other types, whereas the notes in Chapter XII rather suggest that other forms are used as extensively as the Sopwith. This chapter would have been rendered more complete by the addition of an illustration of a modern Dumpy level—a type which is still preferred by many engineers and surveyors to an instrument of the tilting telescope pattern.

In Chapter IX a great deal of space is devoted to detailed instructions for plotting traverses by protractor and by elementary trigonometrical methods, other than by co-ordinates, but in any traverse of importance the co-ordinate method is by far the most widely used and most suitable means of setting out the survey lines on paper.

No less than four chapters are occupied by Tacheometry, including a full treatment of an ingenious simplified system introduced by the author of the book. This section is most useful, particularly as tacheometric methods do not appear to be adopted to the extent which they might be if their advantages were better understood.

The setting out of circular curves is dealt with fully, but transition curves have received somewhat brief notice and there is opportunity for rather fuller development of this subject.

There is little scope for criticism in this excellent text-book. It covers every phase of plane surveying from the general viewpoint of the civil engineer, without a bias in any specialised direction.

B. G. M.

Elements of Water Supply Engineering. By E. L. WATERMAN, C.E. Second edition. [Pp. xviii + 329, with 82 figures including 1 folding plate.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 17*s.* 6*d.* net.)

IN this volume the college student preparing for a career in Civil or Municipal Engineering is offered the elements of at least four subjects conveniently compressed into a single volume. Rainfall and Run-off, Groundwater, the Hydraulics of Pipe Lines and Networks, Contamination and Purification, Health Statistics, Collection and Storage, Pumping, Demand, and Finance are all there, each in skeleton of course, though not in disjointed bits but rather merged together to make another subject. Often the author condenses whole branches of learning into a page or so, complete in itself, for somehow he manages to do it without giving the impression that he is withholding information or that "space does not permit"; and one feels that he is writing with the confidence of a wider knowledge than the syllabus requires, wide enough indeed to enable him to sift the elements from the surrounding litter of interesting but unessential matter.

There are faults of course, and plenty of them; while in places, even for a whole chapter the writing is fluent, elsewhere it deteriorates into those short sentences whose very clarity confuses and tires the reader by tedious and unnecessary repetition. Not every reader will have the patience to face the task, neglected by the author, of blending sentences together, and will give up the struggle or suffer mental indigestion. Turning to technical matters, the main cause of water pollution, *Bacillus Coli* is repeatedly described as *Bacterium Coli,* a classification unfamiliar to the English reader, but possibly the author has grounds for the change. A table of settling speeds for silica particles is given, p. 205, but the value for the coarser particles is two and a half times too great, an error which throws doubt on the author's verification of his numerical data. Again, in the section on pumps, although various types are described, the author makes no attempt to explain why one type is better suited for one purpose than another.

Despite such faults the book has a certain appeal. It was written for students—for very junior students, one surmises. Some writers of text-books flatteringly assume that the reader's knowledge equals theirs, but the present author makes no such mistake; after reading it, the dullest student could never feel that he had not been told what a pipe was for. For this reason it is hardly suitable for the office bookshelf of the specialists whose work it concerns, though others whose work brings them in contact with such specialists might find it of value. That students do so is proved by the demand for a second edition.

C. M. W.

Electrical Engineering Practice. Vol. I. By J. W. MEARES, C.I.E., F.R.A.S., M.Inst.C.E., M.I.E.E., and R. E. NEALE, B.Sc. Hons., A.C.G.I., A.M.I.E.E. Fifth edition. [Pp. xii + 780, with 48 plates and 92 figures.] (London: Chapman & Hall, Ltd., 1938. 25*s*. net.)

IN this latest edition of a well-established classic the material has been substantially revised and extended. The book is about one-third longer than it was previously. It thus remains a record of recent practice in all branches of electrical engineering as well as allied subjects.

This new edition retains the character of a book primarily designed for the use of overseas engineers. An engineer in the " out-posts of empire " must " travel light " both mentally and physically. His job demands that he shall not overburden his mind with too much theory. He has to make many calculations but is not concerned with the derivation of the formulæ which he uses. Nor has he room in his luggage for a great number of specialist text-books. He must be content to remain a Jack-of-all-trades and master of none. He would be happiest if he could have recourse on all occasions to but one single book sure to find just what he needed therein. Of course, that cannot be provided. But the book under review comes nearer to this ideal than one might have thought possible. Evidently the authors have given more care and anxious thought to the needs of their particular public than most authors of engineering text-books do.

The nineteen Chapters cover a very wide range. Definitions, standard symbols and abbreviations are dealt with, so that a person whose scientific training has been quite elementary may obtain the necessary enlightenment, and the presentation should be helpful to an engineer who has to instruct untrained subordinates. The conscientious use of British and International symbols and nomenclature is to be welcomed. We have noticed only one lapse where " capacity " is used instead of " capacitance." The properties of materials and measurements are dealt with just as the practical engineer will require. The part dealing with generation gives quite rightly more space to the steam and hydraulic aspects of the subject than to the electrical aspects. After all, by far the greater amount of capital in generation is spent on non-electrical plant. This demands by far the greater amount of thought and is the more frequent source of trouble. Economics are dealt with rather briefly and the addition of guidance in the comparison of tenders might have been useful. The section concerned with transmission and control includes the calculation of line regulation, mechanics of transmission lines, protection and switchgear.

Thus the engineer who places his trust in this volume will find in it something about nearly everything in which he is interested as an engineer. He will find all the most necessary formulæ and some guidance as to their use. He will find tables taken from the I.E.E. Wiring Rules and similar publications. He will find descriptions of most electrical and allied engineering plant and devices. He will find the advantages and disadvantages of this and that set forth clearly and with a proper sense of proportion. He will find arguments based on common sense. He will find many diagrams of connections of the internal parts of apparatus. At the end of each chapter he will find an extensive bibliography, and this will mention all the relevant British Standard Specifications and reports of the British Electrical and Allied Industries Research Association. He will find no mathematics, no explanation of the formulæ or the statements which are based on theory. He will find himself expected to take most things on trust, blindly. He will meet his first vector diagram on p. 261 in connection with the Varley test for phase rotation, and we feel sure that he will not understand it. But the test must be given, and though the diagram may mystify the type of engineer who will require this book, it may at least convince him that the test does work somehow by science and not by magic.

In short, the book is neither an encyclopædia nor a text-book, but a little of both and exactly what every practical engineer requires.

R. O. K.

CHEMISTRY

A Textbook of Inorganic Chemistry for Colleges. By JAMES F. NORRIS and RALPH C. YOUNG. Second edition. [Pp. x + 803, with 33 figures, including 1 plate.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 21*s*. net.)

PROF. NORRIS is not alone in holding the view that "the average student finds it difficult to understand the apparently simple concepts of Chemistry." His object in writing this book was to present elementary chemistry in a form which the private student could easily follow with the minimum help from a teacher. It was therefore necessary to develop the subject slowly, with frequent repetitions of fundamental principles, and consequently the book contains a more leisurely approach than is usual in text-books of this kind.

The second edition has been almost completely rewritten with the help of Prof. Ralph C. Young of the Massachusetts Institute of Technology, and although modern theories have been introduced at an early stage, so that they may be used as soon as possible, care has been taken to present only those parts which are necessary to interpret chemical facts, and which may legitimately be considered part of an elementary course. Short accounts of the more recently discovered elements and newer technical processes have also been included, and the resulting text-book is one which may be recommended with confidence to the beginner, especially if he has to depend upon private reading. One important and welcome feature is a comprehensive list of 246 references to text-books, monographs and original articles covering the whole field of inorganic and technical chemistry. These are classified under subject headings, so that a student seeking additional information can obtain it immediately and without further search.

J. N. S.

An Inorganic Chemistry. By H. G. Denham, M.A., D.Sc., Ph.D. Third edition. [Pp. viii + 693, with 149 figures.] (London: Edward Arnold & Co., 1939. 12*s.* 6*d.* net.)

Prof. Denham's *Inorganic Chemistry* first appeared in 1921. It was soundly based upon the Periodic Table and the fullest possible use was made of the analogies presented by the various families of elements. The material was set out in a logical and natural sequence and the book was soon recognised as an admirable text-book for those working for the Inter-Science and similar examinations.

The latest edition differs from the preceding one in the somewhat fuller treatment of strong electrolytes, the electronic theory of valency and the structure of matter. It is a moot point how soon a student should be introduced to the more modern developments of Chemistry, certainly not before he has obtained a thorough grounding in the fundamentals of the Science, nevertheless, one of inter-science standard ought to be familiar with the simpler electronic structural formulæ. It is therefore to be regretted that by postponing consideration of atomic structure to the last chapter of the book, Prof. Denham has been unable to make use of these conceptions in the text. Had this been otherwise, some structural formulæ would have disappeared and it would have been possible to include deuterium with hydrogen and give some account of its reactions, and of heavy water, which is not mentioned.

The style of the book is very pleasing and errors are few, although the statement (p. 185) "up to the present no oxides of bromine have been isolated" is no longer true, and a purist would prefer that the Latin prefixes of number be used with the word *valent* (p. 87 and throughout).

J. N. S.

Second-Year College Chemistry. By William H. Chapin. Fourth edition, revised by William H. Chapin and Luke E. Steiner. [Pp. xv + 407, with 43 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 9*s.* 6*d.* net.)

Books which are written to fit a special curriculum can only rarely be grafted on to a course in a different institution, and when (as in this case) the book is really an introductory treatise on theoretical and physical chemistry, a more descriptive title would be an advantage.

The second-year course for which the book has been written is that of Oberlin College, Ohio, and it does not differ appreciably from that in many colleges elsewhere. The historical method of treatment has been used whenever possible, and this is to be commended, for although it is a little more time-consuming, the student gains a solid background to his knowledge. The most important changes made in the fourth edition are in the chapters dealing with strong electrolytes, X-rays and radio-activity, valency, and electro-chemistry, all of which have been revised and modernised. The treatment is clear and lucid and the book may be recommended to those seeking a text-book of approximately pass degree standard.

J. N. S.

The Elements of Chemistry. By WILLIAM FOSTER. Third edition, second printing. [Pp. xx + 706, with frontispiece, 1 coloured plate and 225 figures.] (New York: D. van Nostrand Co., Inc.; London: Chapman & Hall, Ltd., 1938. 10*s*. 6*d*. net.)

THIS text-book is intended for those beginning the study of Chemistry and a previous knowledge of the science is not assumed. The author is firmly convinced of the value of chemistry as an intellectual training of the highest order, in addition to its general utility, and this point of view is maintained throughout. The treatment is nowhere very profound, but the elements of chemistry and many facts of industrial and domestic interest are stated in a plain and straightforward manner, and each chapter ends with a list of other works for reading and reference.

Although written primarily for the use of American students the book would serve for the Inter B.Sc. or similar examinations, but the standard set by other works of a similar scope is very high, and in spite of the many pleasant features of the book it is unlikely to displace any of the well-established favourites.

J. N. S.

Revision Notes in Inorganic Chemistry to Higher School Certificate. By E. P. WILSON, M.Sc., A.I.C. and F. W. AMBLER, M.Sc. [Pp. viii + 240, with 29 figures.] (London: William Heinemann, Ltd., 1938. 4*s*.)

THIS book of revision notes will appeal to certain students, though the majority will wisely use their own notes. Notes are given on each of the common elements and their compounds, arranged according to the Periodic groups. The book adequately fulfils its aim, but this aim should not be forgotten by the reader, who may be tempted to substitute it for a larger and more comprehensive text-book. Comparisons between elements and compounds are frequently given in tabular form, and will prove of considerable use to the student. Questions taken from the Higher Certificate papers of a number of examining bodies are appended to each chapter.

A. J. M.

The Essentials of Volumetric Analysis. By JOHN LAMBERT, M.Sc., in conjunction with A. HOLDERNESS, M.Sc., and F. SHERWOOD TAYLOR, Ph.D., M.A., B.Sc. [Pp. viii + 92, with 10 figures.] (London: William Heinemann, Ltd., 1938. 2*s*. 6*d*.)

THIS little book covers the Higher School Certificate syllabus in practical Chemistry. Fifty-two experiments are discussed, which is all that a candidate for this examination would need, but unfortunately the book contains several disappointing features. The calculations often fail to emphasise the simplicity of volumetric principles, the results of standardisations are too often expressed in grams per litre instead of normalities, and such practices as weighing out exact quantities of materials, making standard solutions exactly normal by diluting them in large measuring cylinders, and boiling solutions in graduated vessels ought not to be encouraged.

J. N. S.

Theoretical Qualitative Analysis. By J. H. REEDY. International Chemical Series. [Pp. ix + 451, with 34 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 18*s*. net.)

THE title of this book is somewhat misleading as only a little more than one-third of the text is devoted to theoretical matter, the remainder which is labelled "Descriptive" being really a text-book of practical qualitative analysis.

Part I deals with the usual subjects of theory of analysis clearly and comprehensively. Both the partial ionisation theory of Arrhenius and the complete ionisation theory of Debye are outlined without bias, so that the student may not be prejudiced in favour of either theory during his early studies, but for simplicity of treatment analytical problems have been dealt with from the point of view of incomplete ionisation. In Part II the reactions of the common anions and cations are set out in detail, those of the anions being discussed at much greater length than is usual in an elementary text-book. Equations are usually given in the ionic form and only skeleton separation tables included. The latter are very uninformative and although full instructions are given in the text, the student is unable to see the separation as a whole and this is a disadvantage.

In spite of its size the author claims no more than that the book is a study book for the beginner and not a reference book for the analyst. Nevertheless, the limited field of elementary qualitative analysis is covered in great detail. Like all the volumes in the International Chemical Series it is faultlessly produced, but the price will limit it to the reference shelf rather than permit its ownership by the average student.

J. N. S.

Modern Atomic Theory. By J. C. SPEAKMAN, M.Sc., Ph.D. [Pp. 207, with 1 plate and 10 figures.] (London: Edward Arnold & Co., 1938. 6*s*. net.)

THIS book gives a clear and accurate statement of the basic facts and modern developments of atomic theory. The author has successfully combined the logical and historical aspects of the development of the subject, and has eliminated the artificial barrier between atomic physics and atomic chemistry. As a result his text is eminently suited to the requirements of university students of either chemistry or physics. It supplies a sound background, upon which it would be easy to build up a more detailed knowledge, and in this respect is suitable for the senior forms of schools. The treatment throughout is non-mathematical.

The reader is introduced at once to the mass-energy conservation law which, although so fundamental in the discussion of atomic weights, is customarily brought in at a much later stage. The author then deals with the fundamental particles—negative and positive electrons, protons, neutrons, and α-particles—and with the wave properties of matter. The remainder of the book follows conventional lines, atomic structure, isotopes, radioactivity and nuclear structure being discussed in turn. The treatment is critical, and one is impressed throughout by the very careful explanations given, and also by the emphasis which has been placed on the experimental basis of modern theories. As in the case of the companion volume by the same author on *Modern Theories of Valency*, the printing and binding of the book leave nothing to be desired. It has the further merit of being inexpensive, and can certainly be very strongly recommended.

H. J. E.

Understanding the Atom. By J. Rowland, B.Sc. The New People's Library, Vol. XVII. [Pp. 95.] (London: Victor Gollancz, Ltd., 1938. 1*s.* 6*d.* cloth; 1*s.* paper.)

There is a genuine need for books which will give the modern man some understanding of what scientists are doing, for there exists a wide circle of readers who really wish to know something about the main streams of scientific thought. It is most essential, however, that writers who set out to satisfy this thirst for scientific knowledge should realise the great responsibility which devolves upon them. They must bring to their task not only enthusiasm and simplicity of writing but, above all, a profound knowledge of their subject. Then there emerge the joyful, popular books of science which are so successful. The author of the present volume is an enthusiast, but he woefully misleads. To cite but one example from his chapter entitled "What is Valency?"—"Chlorine, for instance, which has atomic number 17, and consequently seventeen electrons outside the nucleus, will have formed two complete rings of eight, and will have one electron revolving outside these. It will therefore be able to give up this electron to another atom which lacks one, and will then form a compound which is very stable. Common salt is an example of this, where the chlorine atom has given its electron to a sodium atom. (Sodium has atomic number 23, and thus lacks one electron to make three complete rings of eight.)" The author states on p. 73: "Someone once said that valency is a subject of which the schoolboy thinks he knows everything and the professor knows he knows nothing." It is a very great pity that the writer did not heed his own warning, and it is a greater pity that the publishers did not submit this book to a referee before publication. It is definitely a volume which should never have been published.

W. W.

The Phase Rule and its Applications. By A. Findlay. Revised with the assistance of A. N. Campbell. Eighth edition. [Pp. xvi + 327, with 163 figures, including 4 plates and 1 folding plate.] (London, New York, Toronto: Longmans, Green & Co., 1938. 12*s.* 6*d.* net.)

The eighth edition of this well-known and widely used book remains substantially unaltered. A small amount of the subject matter has been deleted and additions made where necessary. The most important is the elimination of the last chapter on practical methods of determining the position of transition points. There is now, however, a short discussion of the use of X-ray technique for detecting phase changes. This section might have been made more comprehensive without going too far into the fundamental principles involved in interpretating the X-ray data. The more recent work on alloys is only mentioned on p. 142. Here too an extension would have been welcome to most students. Another addition is a discussion of the very completely investigated binary system aniline—sulphur dioxide.

The volume has been reproduced from the last edition by a photographic process. In the reviewer's copy the printing tends to be uneven and sometimes blurred, with the result that a few of the characters in the figures are difficult to decipher.

H. W. Melville.

Organic and Bio-Chemistry. By R. H. A. PLIMMER, D.Sc. Sixth edition. [Pp. x + 623, with 1 coloured plate and 69 figures.] (London, New York, Toronto: Longmans, Green & Co., 1938. 21*s.* net.)

EXCEPT insofar as it has been, in part, brought up to date, this new edition of Prof. Plimmer's well-known text-book does not differ in scope or treatment from the last. It provides a complete course, theoretical and practical, in organic and biochemistry particularly for students of medicine. The treatment has, of necessity, had to be very condensed, and the author is remarkably successful in covering a large field and giving most of the essential information in a very lucid manner. Throughout more attention is given to compounds of biological importance than to purely synthetic products.

The earlier part might, however, be improved by judicious modernisation. In view of the oft-heard complaint of the heavy nature of the medical curriculum all obsolete matter should be rigidly excluded, such as, for example, the preparation of methyl alcohol and acetic acid by wood distillation, of malic acid from natural sources and benzaldehyde from benzyl chloride. A number of other points also call for modification such as the Werner structure for urea with its quinque-covalent nitrogen, the preparation of glyoxal from acetaldehyde with nitric acid, the short accounts of the theory of colour and the process of dyeing and the statement anent the structures of silver and potassium cyanides. It may also be remarked that the printing is not up to the publisher's usual standard.

Attention is directed to these minor blemishes because the greater part of the book is so good that it is a pity there are even these faults.

O. L. B.

Unit Processes in Organic Synthesis. P. H. GROGGINS, Editor-in-Chief. Second edition. [Pp. xiii + 769, with 149 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 36*s.* net.)

THIS volume is essentially the same as the first edition (1934), but much more information has been added and in particular sections have been introduced "explaining the theoretical basis of halogenation and hydrogenation reactions by means of thermodynamics." Manufacturing chemists will find the more theoretical matter worth while reading, and academic chemists will realise the necessity of remembering, what is so apparent on glancing through this book, that the chemicals they use are always being made by new processes. The value of the book to technical chemists has been increased by the introduction of several new flow sheets for important operations.

The description of nitration follows normal lines, except that as mentioned already the use of aliphatic hydrocarbons is discussed. The chapter on "diazotisation and coupling" is a considerable improvement on "diazotisation" in the first edition and is up to date. Thus we find mentioned Hodgson's work on practical methods of diazotising and Conant's work on the theoretical side of coupling and so on.

Halogenation becomes an interesting matter when it is dealt with as in Chapter IV. The value of a knowledge of bond- and activation-energies is made clear. An unusual process is that for converting carbon tetrachloride into difluorodichloromethane by means of antimony pentachloride and hydrogen fluoride: another is the converson of β-methylpropene into its γ-chloroderivative.

A valuable account is given of the replacement of halogen atoms in a variety of compounds by the amino-group. Under this heading we find the preparation of morpholine and of $\alpha\gamma$-diamino-β-hydroxypropane. The conversion of alcohols into amines by catalyst-assisted vapour-phase reactions is also mentioned.

The chapter on hydrogenation is excellent and illustrates well the difference between this book, which is well thought out and many so-called technical books, which are mere compilations. A good account is given of the "methanol" synthesis.

In the writer's opinion this book should be in the library of every University and Technical College in which chemistry or engineering is taught. Its value to industrial chemists and engineers speaks for itself. The publishers are to be congratulated on the low cost of the book and the various collaborators of Mr. Groggins for the obvious interest with which they have written their sections.

E. E. TURNER.

Systematic Qualitative Organic Analysis. By H. MIDDLETON, M.Sc. A.I.C. [Pp. viii + 279 pages, with 12 figures.] (London: Edward Arnold & Co., 1939. 8*s*. 6*d*. net.)

NEARLY four decades ago the first book (it was really a pamphlet) devoted entirely to the systematic identification of organic compounds made its appearance, and since then several such books have been published each in turn containing certain novel features provided by the great progress of organic chemistry during the twentieth century. The present volume brings this analytical branch of the subject right up to date, and not only contains the latest modern technique but is a personification almost of its author. Mr. Middleton has assembled some 600 separate compounds together with a considerable number of their metallic derivatives, and therefrom has built up simple schemes for their identification. The work itself bears the stamp of thoroughness, since the author not only himself prepared all the derivatives mentioned and determined their constants but also tested out his schemes on literally hundreds of students. Some verification of melting points has been sorely needed for a considerable time, since different values are only too frequently to be found in works of reference, and this personal determination of them by the author has rendered the science itself a great service. A very important economic point which has been kept in view throughout has been to choose only those derivatives whose preparation entails relatively inexpensive reagents, and on this count alone the general use of the book should be considerably facilitated.

An outstanding and unique feature of the author's treatment is the time factor which has always been kept in mind, and minimum times are given for a large number of reactions, *e.g.*, The time required for the hydrolysis under specified conditions of about 100 esters is given, so that for examination requirements or for work during the brief practical periods available for evening students, suitable examples may be chosen from the lists. This feature should be invaluable to teachers as well as to students.

While the book should receive a warm welcome from all students of organic chemistry in Universities and Technical Colleges, particular attention has been paid to the requirements of Pharmaceutical trainees, the schemes permitting of the rapid identification of the majority of the single organic compounds included in the *British Pharmacopœia*.

H. H. HODGSON.

Laboratory Manual of Organic Chemistry. By H. L. Fisher, Ph.D. Fourth edition. [Pp. xxiv + 412, with 21 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 13*s*. 6*d*. net.)

It is not surprising that this book has reached its fourth edition: it is full of interesting and useful information, *e.g.* on wiring stoppers into flasks, loosening stopcocks (p. 7) and cutting Pyrex combustion tubing (p. 302). A novel m.pt. apparatus is described (p. 27).

Methods of preparation are brought up to date in this edition, *cf.* the use of aluminium *iso*propylate in preparation of methylphenylcarbinol (p. 199). The conversion of this into the plastic polystyrene is one of those departures from laboratory practice which arrest the attention and develop the interest of the student—an aim which although fundamental in teaching is in practice usually ignored. The polymerisation of isoprene to a synthetic rubber (p. 274) is of similar value.

One quarter of the book is devoted to the *macro* methods of estimating C, H and N. We endorse the author's remark (Preface, p. vi), "In this day of micro and semi-micro methods the author will probably be criticised for not changing to these newer methods." Semi-micro methods, especially for N estimation, are well within the scope of students. Further, the macro methods given seem to be unnecessarily elaborate, *e.g.* in the N estimation the evacuation apparatus is redundant if an adequate supply of air-free CO_2 is available (*q.v.*, Tucker, *The Analyst*, 1939, **64**, 410).

On the other hand the details given in this analytical portion are most illuminating and helpful—for students about to undertake *semi-micro* analyses. The account of the introduction and preparation (p. 204) of CeO_2 as an oxidation catalyst is characteristically well done (*cf.* Ingram, *J. Soc. Chem. Ind.*, 1939, **58**, 34T).

One striking, and we think unfortunate, omission is of formulæ. Textbooks and references in the questions presumably give the student the required information *if he takes the trouble to look it up*—he will certainly need to do so for the formula of 1 : 5-diphenylthiocarbazone (p. 193)! The formula of Monastral Blue (p. 253) *has* been included, but is it worth while for the average student to try to remember it?

S. Horwood Tucker.

The Chemical Analysis of Foods and Food Products. By Morris B. Jacobs, Ph. D. [Pp xxii + 537, with 56 figures.] (New York: D. van Nostrand Co., Inc.; London: Macmillan & Co., Ltd., 1938. 25*s*. net.)

The preface of this book states that its objects are to present "the salient facts of the chemical analysis of foods and food products and to include certain of the newer aspects of food analysis, such as the chemical assay of vitamins, the detection of improper pasteurisation of milk, the homogenisation of milk, the detection of gums and methods for the detection of newer types of sophistication in foods." The "salient facts" are based to a large extent on the methods of the Association of Official Agricultural Chemists of America, but considerable use is made of modern European methods, particularly those of Great Britain. The descriptions are given in an essentially practical manner and one may assume that the techniques given are those in everyday use in the Bureau of Food and Drugs, Department of Health, City of New York, of which the author is the Chief Chemist. Although there are certain differences in detail from current practice in this country, no criticism can be made against the choice of methods and the ground is substantially well covered. The " newer aspects " are collected from widely differing sources and include matter published within the last year. Practising chemists will be glad to find such information collected in one volume. This applies particularly to the ten gums dealt with, the use of which as binders and fillers in certain types of foods has increased greatly in recent years.

The volume covers a wide range of food products ; but it seems curious that cereals are dismissed in 14 lines plus a table of compositions, and that no reference is made to baking powders. With certain articles several pages are devoted to interpretation of analytical results. It is to be hoped that in any future edition this useful feature will be extended and applied to every article and product dealt with.

Although bearing the imprint of an English Publisher, the book is written solely from an American standpoint. Nevertheless, it is to be recommended as an essential addition to the library of those engaged in the analysis of food products in all parts of the world.

J. R. N.

An Introduction to the Chemistry of Cellulose. By J. T. Marsh, M.Sc., F.I.C., F.T.I., and F. C. Wood, Ph.D., F.I.C., F.T.I. [Pp. xvi + 431, with 111 figures, including 18 plates.] (London : Chapman & Hall, Ltd., 1938. 21*s.* net.)

This book covers more ground than the modestly worded aims expressed by the authors in the Introduction might lead the prospective reader to suppose. It gives in condensed form, but with all essential details and with numerous diagrams and a wealth of references to the literature, an account of the chemistry of cellulose and its practical applications in the textile industries. The time was opportune for the publication of such a volume. On the academic side, the structural investigations into the constitution of cellulose have reached a stage when there is general agreement concerning the main features of the molecular architecture, and although the time is probably still distant before the finer details will be known with full certainty, it is possible to make use of what is already substantiated concerning the behaviour of the long chains of glucose units to offer tentative explanations of the mysteries of the hydrocelluloses and oxycelluloses which are so important in technical practice. The constitution of cellulose is considered in Part V (pp. 345–405), which provides a valuable summary of the evidence derived from chemical transformations and from X-ray data. This section, although placed at the end, is really fundamental to the whole book, and a reader who is not already acquainted with the work described in Part V will find the admirably written but unavoidably difficult chapters on the modified celluloses rather heavy going, unless he reverses the order of reading and takes Part V first. Part I, in which the authors give an account of the occurrence and general properties of cellulose, also deserves special mention. This is illustrated by a series of beautifully reproduced photo-micrographs, and serves to emphasise the difficulty of defining exactly what is meant by the term cellulose—a difficulty which is too often overlooked with unfortunate results. The accounts of the theoretical side and the practical applications are clear and up to date, as one might expect from authors actively engaged themselves in the development of cellulose chemistry, and the book brings together in an ordered array a host of facts for which search would otherwise have to be made through a widely scattered literature. It can be said without hesitation that for all engaged in work connected with cellulose, whether on the technical or purely scientific side, the authors have performed a most valuable service in writing this book.

E. L. H.

Die Chromatographische Adsorptionsmethode. By Dr. L. Zechmeister and Dr. L. v. Cholnoky. Second edition. [Pp. xiii + 354, with 74 figures.] (Wien: Julius Springer, 1938. RM. 19.80.)

That the great possibilities of chromatic absorption are being increasingly realised is shown by the constantly increasing number of papers dealing with the subject, a circumstance which has necessitated the incorporation of no less than 200 new references since the appearance of the first edition a year and a half ago. Despite the constantly increasing scope of usefulness of the method comparatively little is known about the relation between absorbability and molecular configuration, but beginnings have been made in that direction and these are outlined in a chapter devoted to that purpose. In regard to the choice of adsorbent material, the peculiar fact has come to light that climatic conditions may have an influence on the activity of the adsorbent. Among the new fields in which the method has been employed with success may be mentioned inorganic substances, but no results of outstanding importance have yet been achieved in this field. A chapter is devoted to the application of chromatographic analysis of technical and commercial products and pharmaceutical preparations and drugs. The book should be consulted by all who are in search of the latest information regarding the utility of the valuable method of separation of complex mixture into their constituents.

P. H.

Handbook of Chemical Microscopy. Vol. I. By E. M. Chamot, B.S., Ph.D., and C. W. Mason, A.B., Ph.D. Second edition. [Pp. xvi + 478, with 165 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 22*s*. 6*d*. net.)

The second edition of this excellent treatise will be warmly welcomed by all who appreciate the value of microscopy in chemical practice. By the use of slightly smaller type and a longer line, room has been found for much new material with only a small increase in the number of pages. The arrangement is the same as in the first edition, except that the section on the determination of particle size, which has been much expanded, has been given a chapter to itself. Several other sections have been rewritten and expanded, notably those dealing with the study of crystals and cryptocrystalline aggregates, fibres, illumination, photomicrography, and preparation techniques. Throughout the book the descriptions of methods and apparatus have been brought thoroughly up to date.

The treatment is so comprehensive, that it may seem a little ungracious to call attention to omissions, but one cannot help wondering why there is no mention of the very useful quarter wave mica compensator, and no description of von Federov's universal stage.

The following minor points call for mention:

The sections dealing with the relation between the direction of propagation of light and refractive indices in anisotropic crystals could be made much clearer by the addition of simple diagrams showing the optic axes and the principal axes of the optical indicatrix.

The statement on p. 292 that the extinction angle may vary with the direction of propagation, if the extinction is oblique, is misleading. It must vary.

On p. 307 it is implied, incorrectly, that crystallographic axes are always axes of symmetry. On the same page, the paragraph on the relation between optical properties and molecular shape is rather misleading, since it takes no account of the relative orientations of the molecules.

The production of the book maintains, and in some respects improves on, the high standard set by the first edition, and authors and publishers are to be congratulated on a most valuable addition to chemical literature.

N. H. H.

Thorpe's Dictionary of Applied Chemistry. Vol. II. By J. F. Thorpe, C.B.E., D.Sc., F.R.S., F.I.C., and M. A. Whiteley, O.B.E., D.Sc., F.I.C., assisted by eminent contributors. Fourth edition. [Pp. xxiv + 712, with illustrations.] (London: Longmans, Green & Co., 1938. 63*s*. net.)

In the second volume of this new edition of Thorpe's well-known work the editors have, while keeping to the general policy outlined at the beginning of their task, tried to meet some of the criticisms made of the first volume, but in the Foreword they successfully rebut the accusation that organic chemistry bulks too large and that too much attention has been given to fundamental matters.

The articles maintain the high standard of those in the first volume, as is only to be expected from the authority with which the various distinguished contributors write. There is, however, a rather serious lack of balance in the amount of space devoted to various topics which can, perhaps, best be brought out by comparing the twenty-two columns of the excellent article on synthetic camphor with the three columns on casein plastics and the one column on chalk, which, by the way, cites first among the uses of chalk "as a writing material in form of white and coloured crayons!" Again, in the articles on cereals, fourteen columns are devoted to millet, twenty-nine to rice but only five to wheat.

One wonders whether the editors considered the wisdom of devoting nearly a quarter of this volume to an article on chemical analysis; of necessity, such an article cannot be sufficiently detailed to enable a laboratory worker to carry out an analysis and surely in every library containing the present rather expensive work, standard books on analysis will be available. Exclusion of such articles might well reduce the total work by a volume, quite an important consideration in view of the heavy cost of chemical books nowadays. In any case the illustrations in this article, especially those on pp. 542, 543, 546, 618 and 624, are not worthy of reproduction. O. L. B.

Industrial Chemistry. By W. T. Read. Second edition. [Pp. x + 605, with 115 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 25*s*. net.)

The plan and arrangement of this book, which follow very closely that of the first edition, are designed presumably to give the reader a general picture of chemistry as applied to industry, and as such the work is likely to prove useful to young chemists who intend to embark upon an industrial career or to anyone who seeks to obtain a superficial knowledge of the various branches of the industry to which reference is made. The method of treatment is definitely descriptive, rather than quantitative or critical, and in many cases only the barest outline of even widely used processes is given. Thus, less than eighteen pages are devoted to the chapter on Fixed Nitrogen (including by-product ammonium sulphate, Chile nitrate and retort nitric acid), and, in the chapter on Sulphur and Sulphuric Acid, the subject of sulphuric acid concentration is dismissed in thirteen lines. Admittedly, one does not look for process details in a single-volume work of this kind, and one realises that severe compression of information is inevitable, but the impression remains that a more appropriate balance could have been achieved in the presentation of the subject matter, and that the author might have gone considerably further towards acquainting his readers with the fact that the chemical industry is by no means confined to the American continent.

The early chapters deal with general subjects, including chemical organisations, sources of information, analytical control, chemical economics and materials of construction. The non-industrial reader will find some useful information in the chapter on Unit Operations and Equipment, which includes the handling of materials, heat generation and exchange, size reduction, mixing and agitation, mechanical separation, drying, evaporation, distillation and other typical plant operations.

The illustrations are mostly in the form of line drawings, which are very clear and which add greatly to the value of the text.

H. W. Cremer.

An Introduction to Metallurgy. By Joseph Newton. [Pp. ix + 537, with 159 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 20*s*. net.)

The author has aimed at producing a book which shall serve as an introduction to the whole field of metallurgy and provide a basis for more advanced and specialised study. This end has been admirably achieved, and the book will prove of very great value to all students commencing a study of the subject. The treatment falls under the main heads of Adaptive Metallurgy, the converting of metals to useful forms, and Extractive Metallurgy, the winning of metals from their ores.

As examples of particularly successful sections special mention may be made of Chapter XI and Chapter XII, accounts of chemical principles and fuels in relation to pyro-metallurgy, and the interesting introduction to the basis of payment for ores, etc., in Chapter XV. A very valuable feature of this book is the admirable exercises at the end of most chapters which compel the student to supplement for himself the information provided. The books listed for further reading are, however, predominantly American and sometimes not the best available.

The illustrations are not always wisely chosen, and some are poorly reproduced; there are tables too which require further explanation. In a book of such wide scope it is inevitable that certain sections should be less satisfactory than others. Mention may be made of some of these shortcomings. The chapter on heat treatment is inadequate, the illustrations of micro-structures of steels are not representative and are in part erroneous; the Fe—C diagram should receive fuller consideration and the exposition of the application of the phase rule, always a difficult matter, is not sufficiently clear. The section on corrosion is too brief and, in particular, mention should be made of the differential æration principle. The account of the laws of crushing is unsatisfactory, the limitations of their application not being pointed out. In the description of filtering no mention is made of filter presses, and in the section on refractories the important matter of firing receives scant attention. There are a number of misprints, mostly obvious.

A second edition should be called for, and when that happens the various shortcomings indicated may easily be remedied. A notable feature of the book is the admirable index.

S. J. Kennett.

Steel and Its Heat Treatment. Vol. I: Principles, Processes, Control. By D. K. Bullens and the Metallurgical Staff of the Battelle Memorial Institute. Fourth edition. [Pp. xvi + 445, with 208 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 22*s.* 6*d.* net.)

For the fourth edition of this well-known work on Steel and its heat treatment the original author has taken no active part in the revision which has been undertaken by the Metallurgical Staff of the Battelle Memorial Institute. With the exception of a chapter on nitriding, added in the 1935 printing, the book has not been materially revised since 1927, so that there has been a large amount of new information to be incorporated and this has resulted in the work now appearing in two volumes, the one under review dealing with the principles, processes and fundamentals of control which relate to the heat treatment of steels in general, leaving for Vol. II, published separately, a discussion of the various engineering and special purpose steels. This Volume I undoubtedly follows the previous editions in serving as an effective link between theory and practice, because the sound and practical point of view adopted in emphasising the importance of the human element and in dealing with the various processes as a broad engineering problem has been maintained. Special attention has been given to certain aspects of heat treatment in which major advances have been made during recent years, and these include the significance of grain size in steels and the development of methods for the regulation of this grain size, the use of controlled atmospheres in heat treatment furnaces in order to avoid scaling and decarburisation, the development of low carbon steels of high yield strength and the specific effect of various alloying elements, the advantages of normalising instead of annealing steel castings, forced gas circulation for more uniform heating in tempering and similar operations and the use of copper as an alloying element with the precipitation-hardening heat treatment of the resulting steels.

C. O. B.

Sampling and Analysis of Carbon and Alloy Steels. [Pp. 356, with 20 figures.] (New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1938. 22*s.* 6*d.* net.)

This book gives an account of the methods of the chemists of the subsidiary companies of the United States Steel Corporation as revised to 1937; it represents more than two years co-operative effort on the part of a selection committee and replaces two former booklets dealing with the same subject. The methods described for the most part are in frequent use in the several laboratories represented by the committee of seventeen, including thirteen chief chemists of the Corporation. The details given are not entirely confined to those used by the companies as other methods are included especially from the publications of the American Chemical Society, the American Society for Testing Materials and the National Bureau of Standards. The importance of the means to be adopted for taking samples of materials during the course of manufacture and also of blooms, billets and finished products is made clear and the most suitable methods are described and illustrated. Besides the various methods for the determination of the commonly occurring elements in plain carbon and various simple and complex alloy steels, such matters as the determination of total aluminium, metallic aluminium and alumina, zirconium, columbium, tantalum, selenium, boron, beryllium, antimony and cerium receive attention, and appendices are devoted to the methods for the determination of sulphur or sulphur plus carbon by combustion in oxygen, to a discussion of various methods for the determination of oxygen or oxygen, hydrogen and nitrogen in steel and to the preparation of *p*H ranges and colour changes of various organic chemical indicators. This book will prove most useful to all engaged in the routine analysis of all classes of steels, especially as a means of comparing the details of American and British practice, and will be particularly useful to analysts who occasionally have to determine the rarer constituents of special steels.

C. O. B.

Contributions to the Knowledge of the Chemical Composition of the Earth's Crust in the East Indian Archipelago. I. The Spectrographic Determination of the Elements according to Arc Methods in the Range 3600–5000Å. II. On the Occurrence of Rarer Elements in the Netherlands East Indies. By DR. W. VAN TONGEREN. [Pp. xii + 181, with 14 figures.] (Amsterdam: D. B. Centen's Uitgevers-Maatschappij, N.V., 1938; English Agents: H. K. Lewis & Co., Ltd., London. 11*s*. net. Part I, separately, 7*s*.)

ALTHOUGH a large amount of work on the distribution of the rarer elements has been carried out in recent years, chiefly by Goldschmidt and his collaborators, no attempt has hitherto been made to determine by systematic investigation the extent to which they are present in a given restricted area of the earth's crust. The present work represents a first step in this direction, the area in question being that of the Dutch East Indies. The method employed was a spectroscopic one, using a glass prism spectrograph, photographs being taken of the arc spectra of the elements over the range 3600 to 5000 A.

Full details are given in the first part of the book of the apparatus and technique employed; specially purified carbon rods were used as electrodes, and duplicate photographs were taken with and without the addition of sodium carbonate to the material under examination.

The results obtained from the examination of over 300 specimens of rocks from all parts of the Dutch East Indies are collected and tabulated in the second section of the book. Owing to the fact that certain elements, such as B, Be, and As either do not yield lines, or only give unsuitable ones, in the portion of the spectrum examined (3600-5000 A), the list given is necessarily incomplete; only 16 of the less common elements could, as a rule, be quantitatively determined, those being Sr, Ba, Sc, Y, La, Ce, Nd, Ti, Zr, V, Cr, Mn, Ni, Co, Ga, and Pb. In a few special instances the presence of other elements in abnormally large amounts has been demonstrated, *i.e.* a serpentine from Sumatra proved to contain 0·3 per cent. zinc oxide.

The book contains a considerable amount of useful and interesting data, but must be regarded in the light of a preliminary publication rather than as a detailed treatment of the subject, and it is to be hoped that the author will continue his work in this particular field.

H. F. HARWOOD.

Chemical Engineering Economics. By C. TYLER. Second edition. [Pp. x + 241, with 37 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 18*s*. net.)

THE new preface calls attention to the fact that tremendous advances have occurred during the twelve years since the first edition was published, *e.g.* synthetic ammonia production has increased fifteen-fold, and rayon production five-fold.

Much statistical matter has been eliminated in this edition and its place taken by additional chapters on Organisation, Research and Development, Sales Development, and Patents. The chapter on Research is particularly interesting and should provide many a research manager with ideas of organisation or arguments to submit to boards of directors for the increased utilisation of research facilities.

Under the chapter entitled "Project Analysis" the author gives the sources of information commonly used in making market surveys and cost estimates, but these apply exclusively to the United States. The same criticism can be made to the chapter on Patents and the value of the book would have been much enhanced on this side of the Atlantic if the author had adopted an international point of view when he was writing them.

Subjects such as foreign exchange, theory of money, the function of banks, financing of companies, horizontal and vertical integration of industry are omitted. Although it is not suggested that a book such as this should deal exhaustively with these subjects, surely any book on economics should consider briefly the basic theory and facts of the science.

The question of the ratio of capital to annual sales is dealt with independently on pp. 10 and 190 and is given as an average of about 1·8 for the whole chemical industry. The average is apt to be misleading, since two cases the reviewer has in mind give 40 and 0·4 respectively. It might have been better if the author had analysed the point in more detail.

The chapter on "Unit Operation Costs," which is retained from the previous edition, is most valuable and should be read by every chemical engineer.

M. B. DONALD.

Library Guide for the Chemist. By BYRON A. SOULE, Sc.D. International Chemical Series. [Pp. xiii + 302, with 4 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 15*s*. net.)

THIS book, originally prepared for the use of students and graduates in Chemistry of the University of Michigan, is the outcome of the author's contention that training in library technique is of value to scientific workers, particularly in the field of chemistry. It is intended to act as a guide to the more important sources of chemical literature available and to indicate the best methods of approach to them.

The author begins with a general description of the arrangement of a library and the more usual systems of indexing and cataloguing. A section on biography is followed by a history of the growth of periodicals, and useful information is given concerning the various abstract journals published. The more important monographs, year-books, chemical dictionaries and encyclopædias are then enumerated. The standard reference books and journals on inorganic and organic chemistry are given, together with a useful guide to Beilstein's *Handbuch der organischen Chemie*. Similar sections on analytical and physical chemistry follow. A comprehensive description of the patent literature of various countries, a section on United States Government publications, and finally a chapter containing general hints on the writing of reports, complete the contents.

Some errors occur here and there; *e.g.* the arrangement of "Bulletin de la Société Chimique de France," given on p. 71, was changed in 1934; the inclusion of "Physiological Abstracts" in *British Chemical Abstracts* in January 1938, has not been noted; the title of the "Reports of the Progress of Applied Chemistry" is incorrectly given (p. 78); and SCIENCE PROGRESS is included in the list of year-books (p. 83) instead of in the list of periodicals frequently publishing reviews (p. 78).

Although naturally the tendency is to include American books in preference to those of other countries, the field of chemical literature has been very well covered and the book should be of value both to chemists and to librarians in special libraries containing chemical literature.

M. S.

German Grammar for Chemists and Other Science Students. By JOHN T. FOTOS and JOHN L. BRAY. [Pp. xxii + 323.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 11*s*. net.)

THIS is Volume 1 of a four-volume series of German text-books for science students prepared jointly by the School of Chemical Engineering and the Department of Modern Languages at Purdue University. Volumes 2, 3 and 4 are graded readers (Volume 3 has recently been reviewed in SCIENCE PROGRESS), while Volume 1 contains the essentials of grammar and some reading exercises. It is obvious that a great deal of care has been taken in preparing the series. In one frequency count alone, more than 100,000 words of chemical literature were studied for vocabulary and grammatical constructions, and the results are embodied in this volume, the first 1000 most frequently occurring words being starred and introduced as an integral part of the graded reading exercises. On the other hand, common words of ordinary conversational German hardly appear. To work through all four volumes would be a rather formidable task for a science student with an already overloaded syllabus, but Volume 1 contains all the necessary grammar and many reading exercises, and it may be recommended with confidence to all who wish to acquire a knowledge of "Scientific German."

J. N. S.

GEOLOGY

Outline of Historical Geology. By A. K. WELLS, D.Sc. [Pp. xiv + 266, with 2 plates and 99 figures.] (London: George Allen & Unwin, Ltd., 1938. 12*s*. 6*d*. net.)

DR. WELLS has written this book for the general reader and for the undergraduate with no geological background.

After a useful introduction, chapters on the Cambrian, Ordovician, Silurian and the Building of the Caledonides follow. Then comes a very short chapter on the Pre-Cambrian Rocks. The present reviewer has a prejudice, which others may share, in favour of beginning with the pre-Cambrian and passing upwards chronologically, or of beginning with the Pleistocene and working downwards, and is thus a little disconcerted, in spite of the arguments in the text, to find the pre-Cambrian placed between the Lower and the Upper Palæozoic.

After the chapter on the pre-Cambrian the Upper Palæozoic and later systems are dealt with chronologically, with a chapter on the Armorican Mountain Building between those describing the Carboniferous and Permain rocks. The treatment is interesting and adequate.

In general the book's weaknesses are in the parts referring to Scotland. Carboniferous rocks occur in the Pass of Brander and in Morven, both north of the Highland Boundary Fault (p. 87). Dr. Wells appears to accept Dr. Sherlock's views on the New Red Sandstone, as he has no reference to Permain sediments in Scotland. A protest must be made against "Lock Tua" (map, Fig. 82, p. 210).

Principles and examples are happily blended throughout the book, so that the student does get a picture of the testimony of the rocks. Another pleasing point is that the names of living investigators are included as well as those of the great workers of the past.

The book has an attractive form and is well illustrated. It is a most useful addition to the library of those for whom it is designed, for there is no similar work. It is not necessarily a simple task to write a simple book, nor to make stratigraphy interesting. Dr. Wells has had a large degree of success in both undertakings, and his book will also be useful to the more advanced student.

B. H. BARRETT.

Geology for Engineers. By BRIGADIER-GENERAL R. F. SORSBIE, C.B., C.S.I., C.I.E. [Pp. xxii + 348, with 46 figures.] (London: G. Bell & Sons, Ltd., 1938. 12*s*. 6*d*. net.)

THE first edition of this book appeared in 1910, and this revised edition of 1938 refers to works on roadmaking written over thirty years ago, when the motor-car was a rarity. It is distressing to find that the majority of the references made are similarly out-of-date, and a grave omission is that no mention is made of stratigraphical geology, not even a chronological table of the succession of the systems being given.

The general plan of the book is commendable, but the absence of much information available during the past twenty years robs it of much of its usefulness to the engineer. It purports to deal with dynamic and structural geology, minerals, rocks, field and applied geology, building stones, bricks, clays, limes and cements and plasters, roads and canals, rivers and coast erosion and land drainage, a wide field indeed. Of this field, 8 pages are devoted to minerals, many of which are of no interest to the engineer, while it is annoying to find the same thing described again in different parts of the book (see " aqueous rocks " on pp. 20–21 ; and again on p. 48). The petrological section has also run to seed, far too many uncommon rock-types being included. There are too many wholesale quotations from other writers *en bloc*, unmasticated and undigested. Geological maps as such are not described at all, a grave omission, since a whole chapter on maps and mapping would not be too much for engineering readers.

The important question of the effect of surface topography on the slope of the water-table is not mentioned, while the treatment of the selection of dam sites is entirely inadequate. Again, chemical composition of granites is of no interest whatever to the engineer, while rocks such as basalts, seldom used for building stone, receive mention. The important sandstone class is dismissed in a mere two pages, and limestone (our most important class of building stone) in one. There are 10 pages on lime, while Portland Cement is dismissed in two pages only, and gypsum plasters in less than a page. Structural geology as affecting road location is scanty in the extreme, and no mention of the important Page Impact Test is made in the testing of road aggregates, cementing value, of historical interest only now, being included. The statements made as to the use of " granite " in road construction are in the opinion of the reviewer most misleading, and the wholesale quotations made here from a work thirty-six years old are not helpful in the light of modern knowledge.

No mention whatever is made of the work of the Road and Building Research Stations, and it is decidedly disappointing to have to record that, admirable as this volume may be in its scope, it appears to have failed in its object by completely inadequate revision, when judged by the standards of to-day.

B. H. KNIGHT.

Applied Geophysics in the Search for Minerals. By A. S. EVE, C.B.E., M.A., D.Sc., F.R.S.C., F.R.S., and D. A. KEYS, M.A., Ph.D., F.R.S.C. Third edition. [Pp. xii + 316, with 112 figures.] (Cambridge: at the University Press, 1938. 16*s*. net.)

THIS is the third edition of a well-known text-book, but unfortunately the revision has been confined to minor alterations of the 1932 edition, which was itself only slightly modified from the first edition of 1928. As applied geophysics has been revolutionised since then, the book is given a rather quaint and archaic flavour, which is scarcely dispelled by a chapter on "recent advances." The proportion of the 316 pages given to the various divisions of the subject is curious; for instance, 108 pages are devoted to electrical methods, and only 32 to seismology, of which only two are concerned with the reflexion method. As this latter method, in conjunction with the static gravimeter, which is not mentioned at all, has yielded by far the most spectacular successes of applied geophysics, so cursory a treatment is scarcely adequate. If some of the discussions of miscellaneous topics such as micropalæontology, searching for buried treasure and the history of the divining rod were omitted, it would have been possible to find room for more relevant matter without increasing the size of the book.

These criticisms, however, are concerned with omissions, which the student can supply by reading elsewhere, and do not detract from what has always been the main value of the book, the careful and detailed description of electrical methods with the account of practical examples, many of them drawn from the experience of the authors and their students.

E. C. B.

Field Determination of Rocks. By E. H. DAVISON, B.Sc., F.G.S. [Pp. viii + 87, with 18 plates and 4 figures.] (London: Chapman & Hall, Ltd., 1938. 7*s*. 6*d*. net.)

THIS book may be regarded as the companion volume to the author's *Field Test for Minerals* reviewed in SCIENCE PROGRESS, XXXIII, No. 131, 1939,

599. Owing, perhaps, to the less definite field characters of rocks as compared with minerals, the book under review is not nearly as successful as the earlier volume. It aims at instructing the prospector and field geologist in the identification of rocks as seen in outcrops and hand specimens. The six chapters are headed: Hints on Field Work; The Rock-forming Minerals; Igneous Rocks in the Field; Origin and Classification of Igneous Rocks; The Secondary or Sedimentary Rocks; Metamorphism. While the treatment is competent and workmanlike, its presentation is somewhat jejune and uninspired.

A number of errors and questionable statements occur in the text, partly, perhaps, owing to the necessity for compression. For example, the statement that the solidifying temperature of acid magma is higher than that of basic magma (p. 32) requires revision in the light of recent work. The definition of "lopolith" (p. 25) is in error as it implies that a lopolith is merely a synclinal phacolith. The definition of analcite-syenite as "phenocrysts of anorthoclase and augite in a base of analcite" (p. 43) is incorrect. Quartz-dolerite, a rock of world-wide distribution and abundant in this country, can hardly be called a "rare" type (p. 49). And is there such a thing as "amorphous" calcite (p. 4)? Some of the photographic illustrations, *e.g.* those of augite-syenite, gabbro, etc., will convey little to the reader; others are good. The book is bound in flexible covers, but is somewhat too large for the ordinary pocket.

G. W. T.

Lexicon de Stratigraphie, Vol. I: Africa. Edited by S. H. HAUGHTON, D.Sc., F.G.S. [Pp. vi + 432.] (London: Thomas Murby & Co., 1938. 31*s.* 6*d.* net.)

THE Fifteenth International Geological Congress, which met in South Africa in 1929, set up an International Commission whose task it was to compile a lexicon of stratigraphy. The present volume, dealing with Africa, is the first fruits of its labours. The Commission decided to attempt to produce one volume, containing definitions of all unit stratigraphical names that have appeared in geological literature, for each continent.

The definitions are arranged in alphabetical order, and each gives the original status of the term, any changes that have taken place in its meaning, the present-day usage, the geological relationships and geographical distribution, a list of the chief fossils, and references to the most important literature. The items are published in the language, English, French or Italian, in which they were submitted by Dr. Haughton's collaborators. At the end of the book the items are again indexed under countries.

Dr. Haughton and his co-workers are to be congratulated on the completion of an arduous task in the compilation of this lexicon, which will prove of inestimable value to all engaged in the investigation of African stratigraphy.

G. W. T.

Practical Oil Geology. By DORSEY HAGER. Fifth edition. [Pp. xix + 466, with 204 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 24*s.* net.)

SOME twelve years have passed since the publication of the fourth edition of *Practical Oil Geology*. In order to cover the new developments of that period, the work has been revised for the fifth edition and material added, in particular, chapters on Geophysics and Palæontology; but how much more acceptable the result would have been if only more attention had been paid to quite general matters. Often there are evidences of a lack of clear thinking. The author in many instances fails to express himself clearly, and even if the aim is to cater for those who have no previous knowledge of geology, this is little excuse for inaccuracies or for glossing over, especially on points where the full story is not complex and can be given quite briefly. It is irritating to be told the obvious and, indeed, to have it stressed, *e.g.* that there are two hundred and fifty $\frac{1}{250}$ in. in one inch. Such instances are frequent and the space thus occupied could have been better used.

The book contains chapters on oil origin, composition and accumulation; on stratigraphy, structure, palæontology, mapping and the location of wells; on drilling and production; and on natural gas, oil-shales, geological field methods, geophysics and the geologist's work. There is an appendix of twenty-one tables covering a variety of subjects, from the producing formations in a number of regions in the U.S.A., to casing weights and various equivalents, and there is also a selected bibliography. The object of the book is praiseworthy, namely, to bring a better understanding of geology and its importance to the oil industry to non-geological workers. Its framework is good and it contains useful information, but there is considerable need for judicious editing, and rewriting in many parts, before it can be recommended unreservedly to those for whom it was intended.

G. D. Hobson.

BOTANY

An Introduction to Botany: with Special Reference to the Structure of the Flowering Plant. By J. H. Priestley, D.S.O., B.Sc., F.L.S., and Lorna I. Scott, M.Sc. Illustrated by Marjorie E. Malins and Lorna I. Scott. [Pp. x + 615, with 170 figures.] (London: Longmans, Green & Co., 1938. 17*s*. 6*d*. net.)

This text-book is an entirely new departure from the usual type of elementary work and it is clear, partly from the sub-title and partly from the preface, that this is the very definite purpose of the authors. They have not sought to give a general elementary account of the whole content of Botany, some branches of the subject *e.g.* Ecology, are no more than defined. Instead they have chosen out for treatment what they regard as fundamentals especially in relation to the structure of the flowering plant. In their preface they point out how difficult it is to give a connected story without dealing with matters that are controversial. They prefer to risk this so long as the facts are correct. It would indeed seem that the book contains many of the results of very recent work and that not all of these have met with easy acceptance on the interpretative side. This approach must be of interest and prove a stimulus to an advanced student but it is a matter of opinion as to whether it is the best method for an elementary one.

In the chapter on the carbon atom it would seem that too much has been attempted in the allotted space. The description of the daffodil suggests that the bulb is a sympodial structure, it is suggested that all Ranunculaceous plants have their stamens arranged spirally and are all exotipulate. There are few slips of this nature, however, and the general get-up of this stimulating work is very good. The excellent illustrations deserve a special word of praise.

E. M. C.

An Introduction to Botany. By Arthur W. Haupt. McGraw-Hill Publications in the Botanical Sciences. [Pp. xii + 396, with frontispiece and 278 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 18*s* net.)

This book is a general text-book of intermediate standard. The scope is therefore well-defined and the treatment of the subject limited to the general aspects of morphology, anatomy and physiology, together with chapters on ecology, genetics, evolution and palæobotany. There are also sections on the systematics of the lower forms, as well as on the flowering plants.

The clear and simple explanations of morphology and anatomy are amply supported and clarified by very good illustrations, and the author has happily chosen for his examples plants most of which are familiar to British students as well as to American. In only a few cases are the popular names unfamiliar and unclarified by absence of the Latin terminology, although in at least one case (speaking as an examiner) it would have been more than a pleasure to have seen an unfamiliar American counterpart of the sadly overworked pin-eyed and thrum-eyed primrose! A refreshing feature of the book is the abundance of clear habit-photographs to illustrate morphological and ecological points, as well as members of the different systematic groups.

One or two departures from common practice are interesting. The author, for instance, deals with insectivorous plants in the morphological section, and takes them in his stride. This, together with the inclusion of the Angiosperm parasites in the section devoted to environmental relations, avoids the undue emphasis often placed upon the relatively small class of biological curiosities by grouping them together in a special chapter.

The section on palæobotany is fuller, more interesting and better illustrated than in most books of this standard, and the combining of pollination with floral evolution in one chapter helps the understanding of both.

Finally, the pleasure of reading this text-book is in its absence of text-bookishness. There is a feeling of easy continuity throughout, engendered largely by the author's incidental explanations of points as they crop up.

F. Y. H.

Plant Physiology. By E. C. Miller, Ph.D. Second edition. McGraw-Hill Publications in the Botanical Sciences. [Pp. xxxii + 1201, with 39 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 45*s.* net.)

Compared with the first edition of this work, which appeared in 1931, there is an apparent increase in subject matter of 300 pages, but actually the effective enlargement is greater, as by exclusion of the questions formerly included additional space has become available. The extent to which the text has been brought up to date can best be gauged by the fact that we find reference to Virtanen's work on the beneficial effects of Vitamin C when added to cultures of Peas (1936), to the work of Johnson on the differential response of various species to the action of X-rays (1936), and even to the work of White published in 1938 on the root pressure of excised root tips.

Although, however, the title of the work might well lead one to anticipate a comprehensive treatment of the subject, there are certain aspects which are entirely neglected. The approach is indeed essentially metabolic, and this perhaps accounts for the regrettable omission of any section treating of either geotropism or heliotropism, and only a very brief account of hydrotropism. So too with regard to the more ecological aspects of plant physiology we find no recognition of that fundamental factor in the physiology of plants in nature, namely competition, an omission that is strikingly obvious in the section devoted to the extent of root growth.

Not least of the merits of this work are the extensive bibliographies appended to each chapter, which in the aggregate must represent considerably over 4000 citations, and, even though the student would probably have welcomed a rather more eclectic treatment in the body of the text, this cannot fail to be a valuable aid to student and teacher alike.

E. J. Salisbury.

A Laboratory Guide for a Course in General Botany. By Lee Bonar, Lucile Roush, and the late Richard M. Holman. Fourth edition. [Pp. xviii + 110, with 1 figure.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 6*s*. net.)

This book, which has been compiled from duplicated laboratory work-sheets, is intended for use by very large classes of American elementary students, and is of a general standard somewhat above that required by medical students in this country, but below that demanded by the usual syllabus for Intermediate B.Sc. It is frankly an adjunct to the two text-books of general botany by Holman and Robbins well known in the United States.

The first half of the book is devoted to a general elementary course in which there are sections covering the morphology, physiology and anatomy of each part of the plant. Each section consists of an introduction, reading assignment, material for study, and directions for work, followed by a questionnaire based partly upon the practical work of the section, but mainly upon the reading which has been done. The student is evidently fed on pretty strong meat right from the start, for after a little elementary cell anatomy and physiology he is asked (on p. 11) " What is meant by a differentially permeable membrane ? " and " Give an account of the changes which the nucleus undergoes during cell division."

It is difficult, of course, to compare standards, but judged by our ordinary intermediate courses the physiology and morphology are fairly well treated, but the anatomy falls a long way behind. There are also some curious and interesting differences from the usual British practice in teaching technique. On the first day the student dissects soaked seeds, and then passes straight on to an examination of branches—he does not touch seedlings until the very end of the course, and the last thing he does is to stain starch grains with iodine. Another feature which seems strange is the treatment of the woody stem before that of the herbaceous one, of which nothing is required of the student but a low-power regional diagram.

The second part of the book is devoted to systematic work and is much more advanced in standard than the first. All the groups of plants are fully treated, with the exception of the angiosperms, which are definitely relegated to field study.

The guide has been drawn up for a one year's course of 2½ to 3 hours weekly. Naturally with such a course it is inevitable that the treatment of certain parts of the subject should be subordinated to that of others. Where the scheme of work here adopted differs from that generally in use in Britain is in the comparatively short time spent in morphological and anatomical training and the important place occupied by the examination of a large number of types of the systematic groups, and while admitting that the exigencies of short-course teaching enforce curtailment of certain parts of the subject, the reviewer feels that on the whole the balance in this book leans rather heavily towards the systematic side, to the disadvantage of the training in general fundamentals.

F. Y. H.

The Evolution of Genetic Systems. By C. D. DARLINGTON. [Pp. xii + 149, with 26 figures.] (Cambridge: at the University Press, 1939. 10*s*. 6*d*. net.)

IT appears that in biology there are periods in which analytical work predominates and synthesis is almost anathema; from time to time, synthetic thought comes into its own again. This is evidently happening with genetics and cytology at present. In the preface to the book under review, the author explains that he is attempting "to show genetics as the study of systems of heredity and variation, systems which rest on a basis of the chromosomes and are related to one another by processes of natural selection. I believe that the combination of the material basis with the evolutionary framework provides the only means of making sense of biology as a whole."

An idea about the field covered may be gained from the chapter headings: Premises of Genetics; The Substance of Heredity; Sexual Reproduction; Meiosis: Pairing and Crossing-over; Meiosis: The Process of Assortment; Change of Quantity: Polyploidy; Change of Position; Hybrids: Diploid and Polyploid; Change of Proportion; The Atom of Genetics; Chromosome Mechanics; Genotypic Control; The Mechanism of Crossing-over; The Biology of Recombination; The Pursuit of Hybridity; The Permanent Hybrid; The Evolution of Sex; Sterility: The Contradiction; Apomixis: The Escape; The Integration of the Cell; The Evolution of Heredity. The book therefore deals with pretty well every important phenomenon of genetics and cytology, and there is no doubt that the author has succeeded in picturing this vast field as a consistent whole. Much, of course, is speculative, and some dogmatic statements may have to be modified as detailed knowledge increases. But the author's treatment is very stimulating, and, although the book cannot be read like a novel, it fully deserves a careful study by every biologist interested in the wider aspects of his science.

H. G.

An Introduction to Industrial Mycology. By GEORGE SMITH, M.Sc., A.I.C. Foreword by HAROLD RAISTRICK, Sc.D., F.R.S. [Pp. xii + 302, with 127 original photomicrographs.] (London: Edward Arnold & Co., 1938. 16*s*. net.)

THIS book is very correctly described as an introduction to Mycology, and those who have had to delve unaided into the subject will readily acknowledge its value. The first two chapters deal with general questions of Morphology and classification and include definitions of most mycological terms, rules of nomenclature and pronunciation. Chapters III to VIII are devoted to the taxonomy of those moulds which may be encountered in industrial work, with passing references to less common moulds. Conveniently interleaved in this section are the majority of the photomicrographs; these are excellent and are perhaps the outstanding feature of the work. The remaining chapters deal respectively with laboratory equipment and technique, physiology of mould fungi, maintenance of culture collections, control of mould growth, industrial uses of fungi and mycological literature.

One may perhaps question the use of the term "industrial mycology," because the line of demarcation between knowledge of academic value and that of industrial value is indistinct in the science of mycology. A mould is not necessarily of industrial importance because it is encountered in industrial work, unless the product in question is liable to be attacked either by reason of its composition or the conditions under which it is stored. Data concerning the limiting conditions of growth are not included under the descriptions of the various moulds, except in some specific cases, consequently one is not enlightened as to why they are important industrially. The data available are admittedly limited, but Galloway's figures for limiting relative humidity, for instance, could have been usefully mentioned individually, rather than the general reference to his work in Chapter XII.

Mr. Smith is frank concerning the inconsistencies in the various taxonomical schemes, but appears to yearn that all moulds should reach the "perfect stage," rather than that they should be classified according to their biochemical activities. There is some confusion in his description of the Asymmetrica-Velutina section of the genus Penicillium, which is stated on p. 177 to be divided into seven sub-sections, whereas only five are given and those do not agree with those described by Thom.

Typographical errors were difficult to find. An 'o' is omitted from sporangiophores on p. 38. "Rhizopus" is spelt inaccurately on p. 32 and "cymose" in the caption of Fig. 11. On p. 246, Aspergillus fumigatus is stated to be thermogenic as well as thermophilic; surely all heterotrophic organisms are thermogenic and the property bears no definite relation to thermophilism. These errors, however, do not seriously affect the usefulness of the book.

D. H. C.

AGRICULTURE AND FORESTRY

Introduction to the Botany of Field Crops. Vol. I: Cereals; Vol. II: Non-Cereals. By J. M. Hector. [Pp. 1127 + lxvii, with 448 figures.] (Johannesburg: Central News Agency, Ltd., 1938. London Agents: Gordon & Gotch, Ltd. £3 10s. net per set.)

These two handsome volumes treat of those plants commonly cultivated as field crops as distinct from those which are essentially products of the garden or orchard. The character of the text renders these volumes a useful and valuable summary of what is known respecting most botanical aspects of the more important field crop plants. The morphology, biology, anatomical structure, cytology and genetics are adequately summarised for those species and groups which have been more fully investigated. The more physiological aspects are only occasionally considered and the ecological relations are intentionally omitted. It will be realised that the work is thus essentially descriptive, and within these limits should be of great value alike to advanced students and postgraduate workers, who will find the bibliographies both comprehensive and conveniently attached, either to sections dealing with individual species or related groups. Some idea of their extent will be gathered from the fact that the section dealing with Wheat is followed by over 400 references.

Needless to say it would be quite impossible to do more than indicate the contents of these volumes. The first, concerned with the Cereals, has chapters on the following topics: Oat species, Wheat species, Rye, Barley, Rice, Millets, Saccharum, Serghums and Maize. In the second volume the chief topics are: Onions and Asparagus, Hemp and Hops, the Buckwheat, the Beets, the Cabbages and their allies, the Leguminous seed and fodder crops, Flax, Cottons, the Carrot Family, the Sweet Potato, the Potato, Tomato and Tobacco, the Cucurbitaceæ and Compositæ.

The subject matter comprises a great variety of interests. To those attracted by the problem of the origin of cultivated plants the summaries of our knowledge, based on geographical distribution and cytogenetics, respecting the cereals will appeal. They will find here a kind of scientific basis for the legend of the garden of Eden. Wild Oats for instance probably originated in the Western Mediterranean region, where diploid wild types occur, from which were perhaps derived migrants that produced the tetraploids of the Eastern Mediterranean and the hexaploid types of S.W. Asia. The cultivated Oats, which differ in their non-articulating character from the wild types, probably arose in three different centres. Again, the Wheat species on morphological and genetical grounds would appear to comprise three groups, of which the diploid types are mainly a feature of Asia Minor, the hexaploids of Afghanistan, and the tetraploids of the Mediterranean Basin and Abyssinia. The Barleys seem to present two centres of dispersal, namely North Africa and Eastern Asia, but the cultivated Barleys all differ from the wild types in possessing a double bundle in the scutellum instead of a single one. The vast number of varieties of Rice, of which perhaps some 12,000 are known exclusive of those in China, serve as a tribute to the biological isolation resulting from the normal self-fertilisation.

The student of geographical distribution will also be interested in the map showing the range of the Atlantic, continental and alpine segregates of the common Red Clover. The student of floral biology will find food for speculation in the differentiation within a species of various types of compatibility, or the fact that the explosive mechanism of pollination in *Medicago sativa* is still of importance in those types which are normally self-pollinated, since it results in a rupture of the stigmatic surface that is necessary for bringing about the germination of the pollen. Indeed just because of their economic importance we probably know more about our cultivated plants and from a greater diversity of points of view than of any wild species, and these pages will enable many, who have neither the time nor opportunity to peruse the mass of technological literature, to make contact with the accumulation of knowledge during recent years.

E. J. SALISBURY.

Cotton: History, Species, Varieties, Morphology, Breeding, Culture, Diseases, Marketing and Uses. By H. B. BROWN, A.M., Ph.D. Second edition. [Pp. xiv + 592, with frontispiece and 140 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 30*s*. net.)

IN this, a second edition, the general plan of the first edition has not been changed, but parts of nearly all chapters have been rewritten or have new paragraphs added, to bring them up to date. The additions amount to about 80 extra pages, and an already imposing list of references has been increased by about a hundred items.

Spinning and subsequent processes are lightly touched, but the stress is laid on the technical aspects of growing, breeding, marketing and classifying of cotton. Reference is made to all the well-known crops but attention is mostly directed towards the author's familiar ground, American cotton.

By assembling such a multitude of facts from a diversity of sources, the author has performed a useful service, the more so since he has the gift of condensation and an easy style. In so conscientious an assembly, however, some curious omissions are noted. The author is evidently unaware of the existence of the *Journal of the Textile Institute,* so that the work of the British Cotton Industry Research Association and other research institutes whose publications are released in that journal, finds no place here. It also seems peculiar to find discussions on cotton quality without any reference to hair-weight per centimetre, now that the importance of this character is so generally recognised. Nevertheless, the author is conscious of the inadequacy of staple length and waste measurements as an index to quality, and quotes results of spinning-tests when possible.

The lack of contact between the various divisions of the cotton industry is notorious; here is an opportunity for those at the manufacturing end to link up with the agricultural end of the industry and the hinterland between growing and spinning.

H. A. HANCOCK.

Commercial Fruit and Vegetable Products. By W. V. CRUESS. Second edition. McGraw-Hill Publications in the Agricultural Sciences. [Pp. x + 798, with 109 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 36*s*. net.)

THE second edition of this book of American origin, where the cultivation and preservation of fruit and vegetables are carried out with such high efficiency, indicates there has been much investigation and progress made during the intervening years since the publication of the first edition.

This, no doubt, accounts for the increased consumption of preserved and canned fruits and vegetables.

The revision dealing with the causes of the spoiling of canned foods incorporates a brief discussion of the general electrolytic theory of the corrosion of metals and a laboratory method for the examination of spoiled canned foods. The canning of the citrus fruits which was formerly unsuccessful is shown to have become an important industry in California due to research finding suitable methods of storage. Additional work is given on the subjects of pectin in the jellying of jams and marmalade, candied and glacé fruits, the preparation of tomato juice and its food value with reference to its vitamin content.

The chapter on "Vitamins" is considerably enlarged with up-to-date information. The value of the book has been greatly enhanced by the addition of four new chapters, *viz.*, on Wines, Frozen-pack Fruits and Vegetables, Plant Pigments and related Compounds, and Enzymes of Fruit and Vegetables.

Under the heading of wines, a descriptive classification of wines produced from various grapes, the analysis of grapes, fermentation, artificial ageing, pasteurisation and packing of wines, etc., are given. It deals also with diseases, non-bacterial and bacterial, and the tasting of wines. The definitions and regulations of wines are in accordance with the United States food and drug regulations. A brief account of wines made from other fruits is included. Of recent years, the freezing of fruit and vegetables has greatly increased and the author has outlined the process with its advantages and disadvantages.

The information given on " Plant pigments " and " Enzymes " will be found to be equally useful and interesting. The photographic illustrations of food machinery are modern and there is an increased list of references at the end of each chapter.

The book supplies valuable information to all connected with foodstuffs and horticulture, whether student, food chemist or manufacturer ; therefore its possession is an asset.

MARION CHADWICK.

An Introduction to American Forestry.

By S. W. ALLEN. American Forestry Series. [Pp. viii + 402, with 122 figures.] (New York and London : McGraw-Hill Publishing Co., Ltd., 1938. 21*s*. net.)

PROF. SHIRLEY ALLEN's book is an admirable production. The general plan is to state the facts in concise and moderate language and at the end of each chapter to cite the most important literature. That makes the book a real " introduction " from which the stranger may readily reach out to more intimate relations with the subject.

The first chapters cover, in non-technical language, the significance of forests and forest products in the economy of the United States ; the processes of growing timber, including forest management and protection ; and the utilisation of timber and other forest products. This, no doubt, is intended for a reading public which in the United States (as in many parts of the British Empire) has only the dimmest perception of what forestry is and means.

The later chapters deal with Forest Policy in its wider aspects as a part of Land Policy in general, the development of American thought and action in relation to its forests ; how the Federal and State Forest Services function ; the difficult problem of securing the efficient management of privately owned forests ; the professional forester and his education and finally the place of Forestry in National Planning.

These are problems in which foresters the world over are closely interested. The scale of the whole forest problem in the United States is gigantic. Starting with apparently inexhaustible resources, one forest region after another has been depleted by felling and by fire. The remaining resources are no doubt still great, but adequate only to an expanding population provided the forests are brought under reasonable forms of continuous management.

Fire control, being one of the major duties and difficulties of the American Forest Service, is dealt with at considerable length by the author and useful suggestions emerge from a perusal of this section.

Two important chapters are devoted to public policy as affecting forestry in the United States. The brief history of developments in American forestry during the last hundred years contained in these chapters and more especially the account of happenings since 1907 are of particular interest.

The book, like others of the series, is well printed, fully indexed and abundantly illustrated.

R. R.

Trends of Agriculture and Population in the Ganges Valley.

By BIRENDRANATH GANGULI, M.A., Ph.D. [Pp. xx + 315, with 1 map.] (London : Methuen & Co., Ltd., 1938. 18*s*. net.)

THIS book describes agricultural practices, their relations to soils and water supply, and reactions between agriculture and population density. It is in three parts, *viz*. Upper Valley, Middle Valley and Delta. Each consists of four chapters with identical titles ; I, Extension of Cultivation and Density ;

II, Double Cropping and Density; III, Agricultural Water Supply and Density; IV, Crops and Density. This plan involves much repetition. The one variation is an Introduction to Part III, pp. 203–10, which is as much a summary as an introduction and for the general reader may be the most useful part of the book. It is regrettable that no corresponding summary is made for the Valley.

Here agriculture varies with the natural factors of soil and water supply, and the human factors of population density, culture, transport and markets. The last three are only slightly mentioned. Animal husbandry is not mentioned either for dairy cows or work animals. The soils are all alluvial; but the differences among them in age and development, in composition and in relation to present flood levels are considerable. There is some description but no map of soil distribution. The one map given, on a scale of about 1 : 10,000,000 marks the limits of the area; but adds nothing else to the information available in most atlases. Large-scale maps of typical areas would add greatly to the value of the study, especially for such natural divisions as the Plain of Tirhut (see p. 129 *et al.*). Local terms for soils and crops are freely used, and there is need for a glossary to explain them.

The book contains a mass of detailed information, largely statistical, and evidence of the author's personal knowledge of some districts. It is difficult to read, but well worth the effort. It needs geographical arrangement, graphical illustration and maps. The good parts are so good that we hope the author will add to his technical equipment and raise the whole of the work to the level of its best parts, plus adequate illustrations.

C. B. FAWCETT.

Humus: Origin, Chemical Composition, and Importance in Nature. By SELMAN A. WAKSMAN. Second edition. [Pp. xiv + 526, with 44 figures.] (London: Baillière, Tindall & Cox, 1938. 30*s*. net.)

IT has been said that "All really grim gardeners should have a keen sense of humus." Agriculturists and horticulturists alike have long possessed a keen sense of humus, and at the same time a keen realisation of the inadequacy of the information available as to the nature of this almost magical constituent of the soil, which more than any one other single factor indirectly and directly affects fertility and productivity by its influence on the soil as an environment for plant roots as well as a storehouse of major and minor plant nutrients. However, as a branch of applied chemistry, the chemistry of humus must be admitted to be a dark page, obscured by fantastic excursions in the terminology of a series of "humic acid" fractions that represented merely variations in conditions of extraction, and clouded by theories of formation that called for the existence of a comparatively simple brown substance formed by purely chemical processes from carbohydrates. In the formulation and establishment of the modern biological theory of humification, Dr. S. A. Waksman has had a leading part. According to this concept humus is a complex consisting of the decomposing residues of plant materials and microbial tissue, and one which varies therefore according to the composition of the parent material and the conditions under which it underwent decomposition.

This book, which has reached its second edition in two years, reviews most courageously the formidable humus literature (Bibliography with 1608 titles) and discusses many aspects of the subject in detail. Particularly commendable is the inclusion in this edition of some of the more recent Russian contributions, largely inaccessible to the ordinary reader, dealing with the role of humus in soil-forming processes. A chapter on the relationship between humus and soil conservation has also been added.

Because of its detail, this is primarily a book for specialists, but they should find it an invaluable source of reference.

A. G. N.

Soils of the Lusitano-Iberian Peninsula (Spain and Portugal). By Emilio H. del Villar. International edition in Spanish and English. The English text (somewhat abridged) by G. W. Robinson. [Pp. 416, with 28 figures and separate map.] (Published by the author, Lista 64, Madrid, 1937; sole Publishing Agents for all countries except Spain: Thomas Murby & Co., London. 40*s*. net.)

The purpose of this book is threefold: to expound the author's system of soil classification, to describe the main soil types that occur in Spain and Portugal and to give a soil map of the Peninsula.

The method of soil classification is based on a threefold grouping; the soil series, the stage of maturity and the phase. He recognises eight soil series, of which only five are sufficiently widespread in the Peninsula to be mapped, namely saline, calcareous, siallitic, acid humus and alluvial; three stages of maturity, namely immature, mature and post-mature, and two main phases; the primary phase which is when the soil is carrying its natural vegetation (natural phase), forest (forest phase) or grassland (prairie phase); and the secondary phase when it is a normal arable soil (agropedic phase).

The main soil series and their complexes are discussed in considerable detail, the emphasis being more on their field characteristics than on their genesis. The descriptions of the soils are clear, and are often amplified by several tables of mechanical and chemical analyses and in some cases by photographs. The soil map is drawn on a scale of 1 : 1,500,000 and has about fifteen colours and eight types of shading. The map is, in the reviewer's opinion, a clear and very creditable production. It forms a most auspicious beginning to the projected soil map of the Mediterranean lands.

The arrangement of the book is that there are three sections to each chapter, the first section being the full Spanish text, the second the English text and the third the analytical tables. The translation reads very well, and the English text appears to be remarkably free from misprints. The book and the map are each bound in stiff paper covers, and the publishers state that the map can also be supplied rolled, suitable for hanging.

E. W. R.

ZOOLOGY

Outlines of Evolutionary Biology. By the late Arthur Dendy, D.Sc., F.R.S. Fourth edition, re-issued with a foreword by Maurice Burton, D.Sc. (Pp. xlii + 481, with 190 figures.] (London: Constable & Co., Ltd., 1938. 16*s*. net.)

This book by the late Prof. Arthur Denby has richly deserved its continued popularity and can still be read with enjoyment and profit by the professional biologist, student or layman interested in biological evolution.

Beginning with details of the structure and life history of the simplest animals and plants and gradually extending his observations to more complex organisms, their inter-relationships, resemblances and differences and adaptations to their environment, the author presents the reader with a massive body of facts which have to be taken into account in any conception of evolution. Then follows a clear discussion of the theory and evidence of organic evolution and of factors determining the course of evolution, if not causing it, and a careful account of the views of Buffon, Erasmus and Charles Darwin, Lamarck and others is included. There is a glossary and an index and numerous figures which have been selected with commendable discrimination.

It would be difficult to find a book of such compass as this which presents so fully, clearly, and with such sustaining interest the fundamental facts and principles of organic evolution. In the fairness with which he has presented the views of opposing schools of thought, the author carries out his own injunction, that "above all, we must avoid dogmatising on an insufficient basis of fact; in dealing with problems of this kind, a rational conservatism, with a mind always open to conviction, seems the only safe attitude to adopt."

In spite of the remarks of Dr. Burton in his foreword to this edition, it is to be regretted that no reference is made to the great volume of important and relevant work of more recent years. Our present knowledge of heteroploidy, species synthesis, recent work on sex determination and differentiation and on the pros and cons of the transmissibility of acquired characters for instance should surely have been mentioned. As it is, the book provides the finest background one could wish for, but gives no idea of modern illumination of the problems, and so is in danger of falling into the class of history books, though one forming a worthy memorial to a distinguished biologist.

F. H.

Methods and Materials for Teaching Biological Sciences. By David F. Miller and Glenn W. Blaydes. [Pp. xii + 435, with 146 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 21*s.* net.)

This is a new book which should be in the possession of all teachers of Biology or of anyone interested in the teaching of Biology.

It is perhaps now true to say that the teaching of Biology in the schools is beginning to assume its rightful prominence, but it is still unfortunately equally true to say that the subject is all too often regarded merely as another possible alternative in the long list of examination subjects and the refuge of students not considered bright enough for others. Here is a book which makes very clear the value *par excellence* of biological studies as a training in scientific method and as a cultural training whose value cannot be overemphasised.

Part I of this book not only makes these facts clear but describes in detail how these fundamental aims of biological teaching may and should be attained, from choosing a text to planning progressive courses and carrying them out.

Great stress is laid on the unavoidable necessity for teaching by the examination of material and by simple experimentation with plants and animals; it is clearly recognised that teaching Biology primarily from text-books should be strictly prohibited. Part II of this book gives excellent and full instructions for the "Preparation and Uses of Classroom Materials," covering their collection, culturing, preservation, laboratory aids and substitutes, preparation and use of reagents, etc., experiments requiring inexpensive apparatus and including very useful information of a very wide range of animals and plants. It is indeed difficult to avoid being too enthusiastic about the arrangement, scope and treatment of this section of the book.

The authors of this book are Americans—Associate Professors in Zoology and Botany respectively and supervisors of teacher training in the Ohio State University, and have primarily, and naturally, written for American teachers or teachers in training. In that lies the only drawback from the point of view of the non-American teacher. Many of the animals and plants described and recommended for culturing, etc., are not to be obtained in Britain, for instance, and furthermore the full lists given of supply agencies from whom materials or preparations may be obtained are all American. This unavoidable difficulty is not at all insuperable to the keen teacher in other countries and detracts little from the value of the book.

To each chapter are added useful problems and references for further guidance and study, and amongst these references selected books are starred as being suitable for inclusion in the school library.

F. H.

Textbook of General Zoology. By WINTERTON C. CURTIS and MARY J. GUTHRIE. Third edition. [Pp. xviii + 682, with 486 figures.] (New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1938. 18*s*. 6*d*. net.)

THE third edition of this book has been considerably modified from the previous edition and a chapter on the Chordata has been added. It starts with four chapters on the physiological activities of animal organisms, that on co-ordination being particularly good; then deals with heredity and variation, passing on to a well-illustrated review of the animal kingdom and finishing with a good account of organic evolution and the theories concerning this process.

The standard is somewhat below that required for a General Degree in Zoology of the University of London for example, and a comparison between it and English text-books of approximately the same standard indicates the wide divergence between American and English teaching of Zoology. Here much stress is laid on the principles of the subject and the morphology is, judged by English standards, lacking in detail, while the more philosophical side is dealt with early on instead of being superimposed upon a ground work of morphology. The book is well written and deals concisely with many sides of the subject not otherwise easily accessible to English students at this stage; hence they should find it a valuable supplement to their text-books. There is an index and a glossary.

N. H. HOWES.

An Introduction to the Vertebrates. By L. A. ADAMS. Second edition. [Pp. viii + 479, with 327 figures.] (New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1938. 17*s*. 6*d*. net.)

IT is a tribute to the usefulness of this book that a second edition has been published only five years after the publication of the first edition. As in the first edition the subject-matter has been divided into three parts, an outline classification, a long comparative section and a section on the various vertebrate groups, and at the end of each chapter there is a useful résumé. The second edition contains more than fifty additional pages, most of which have been added to the comparative section, which has also been to a great extent rewritten.

All the sections are well planned and equally balanced, and if any general criticism is to be made it might be said that most of the figures illustrating the classification are rather small and that the embryology is rather too abbreviated. Many of the mistakes of the first edition have been corrected, but as is inevitable in books of this kind many still remain. A few of the most outstanding may be mentioned. Although it is now over thirty years since Goodrich demonstrated that Polypterus and Calamoichthys are Actinopterygians they are still treated as Crossopterygians. This has led the author to make the unfortunate and misleading statement that "the lobe fin of the type found in Polypterus probably gave rise to the Tetrapod limb." It is also quite untrue to say that the nephridial ducts of Cephalochordata have ciliated openings, the nephrostomes. Similarly, although Watson's recent work on the Aphetohyoidea has been incorporated, other recent work such as the existence of the Ichthyostegids, a group of Devonian Amphibia, is not mentioned.

Like the first edition this book should prove to be a useful text-book for the student who wants a general account of the Vertebrata without including too much detail.

J. A. Moy-Thomas.

Animal Life in Fresh Water: A Guide to British Fresh-water Invertebrates. By Helen Mellanby, B.Sc., Ph.D. [Pp. viii + 296, with 211 figures.] (London: Methuen & Co., Ltd., 1938. 8*s.* 6*d.* net.)

This book is mainly a guide to the identification of fresh-water animals which can be studied with the naked eye or with a hand lens, although some account is given of Protozoa and Rotifers. The text is divided into chapters, each of which, apart from the introductory chapter, deals with one Phylum, with the exception that four chapters are devoted to the Arthropods.

The degree to which an animal can be classified, if this book alone be used, varies considerably from group to group, since the author has wisely used her discretion and has only dealt with species individually where the number of British forms is few and identification not difficult. On the other hand, where specialised knowledge is required and the number of species is large, data are only given for referring specimens to their orders.

The selection has been well made and a large amount of information is packed into the book. Further, the text and the numerous illustrations, most of which are original simple line drawings of the type usually made when observing an animal, do really make it possible to carry out the identifications as far as the author intends. References to more specialised literature on the groups are given at the end of each chapter, so that anyone wishing to carry the identifications further knows where to go for the necessary information.

This book adequately fills a serious gap which has long existed in British zoological literature and can be recommended to school teachers who have interested their pupils in natural history and are faced with identifying specimens brought in by enthusiastic disciples; to university students who wish to study and identify animals in the field; and to amateur naturalists. There are several lapses in style and in spelling in the text, and some of the more technical terms used might well be defined early in a work of this nature, but these faults do not materially detract from the value of the book. There is an index.

N. H. Howes.

The California Woodpecker and I. By William Emerson Ritter. [Pp. xvi + 340, with coloured frontispiece and 29 figures, including 1 plate.] (U.S.A.: University of California Press; Great Britain and Ireland: Cambridge University Press, 1938. 16*s*. net.)

In the first part of this book the author gives the results of several years' study of the remarkable food-storing habits of the Californian Woodpecker. It has long been known that the bird collects acorns, stores them in holes drilled in the bark of pine trees, in the crevices of oak trees, cracks in telegraph poles and even in weather-boarding of houses. Formerly ornithologists thought the acorns were harvested for the sake of the insects inside them, but later investigations proved that the woodpecker lives on acorns in winter and spring, and at other seasons on insects and fruits. This bird appears to have some power of discrimination and bores holes to suit different sizes of acorns. Pebbles are occasionally inserted in holes, like acorns, and the latter dropped into cavities where the bird cannot get at them; these habits the author explains as cases of "maladaptivity."

Another interesting fact is that this woodpecker lives in groups, or parties, each inhabiting a particular area and co-operating in gathering the food supplies and protecting them from jays and squirrels. There is still much to be learned about this communal system which appears to be extended even to the rearing of the young.

The remainder and greater part of the book is devoted to a philosophical discussion of the significance of the differences between the woodpecker, as the type of bird, and man as the type of a mammal. The ancestry, structure, mental equipment and other points are all dealt with at length. There is a useful bibliography and the work is well got up, and illustrated with a colour plate and several photographs.

N. B. K.

Special Pathology and Therapeutics of the Diseases of Domestic Animals. Vols. I, II, and III. By Dr. Franz Hutyra, Dr. Joseph Marek, and Dr. Rudolf Manninger. Fourth English edition, edited by J. Russell Greig, Ph.D., M.R.C.V.S., F.R.S.E., with the collaboration of J. R. Mohler, V.M.D., D.Sc., and Adolph Eichhorn, D.V.S. [Pp. xvi + 2430, with 23 plates and 724 figures.] (London: Baillière, Tindall & Cox, 1938. £6 6*s*. net.)

The first English edition of Hutyra and Marek appeared about twenty-five years ago. It and the two subsequent editions were edited by Dr. J. R. Mohler and Dr. A. Eichhorn of the United States Bureau of Animal Industry, who translated the teaching of Budapest into the most comprehensive text-book of veterinary medicine in the English language. The fourth English edition in three volumes is now published under the editorship of the Director of the Moredun Research Institute at Edinburgh. Many new chapters appear, and numerous revisions have been made.

In one important feature Hutyra and Marek retains its pre-eminence among text-books of veterinary medicine, namely the description and illustration of the symptoms and morbid anatomy (though not the bacteriology), particularly of the contagious infections. But despite its many admirable qualities this fourth edition retains far too many of the faults of its predecessors.

The grouping of subjects is a medley of the etiological and anatomical systems of classification, peculiar to Budapest. For instance, equine influenza is classified under acute infections whereas swine influenza is found among the diseases of the respiratory system. Rabies is in the first volume but Borna disease and some other specific forms of encephalomyelitis are in the third volume. Grass sickness is grouped with botulism, and three entirely unrelated infections of rabbits are tacked on to pasteurellosis. The causal agents of circling disease and fowl paralysis are cited as filtrable viruses, but someone appears to have had some misgiving, so both diseases are planted for safety in the third volume, and on p. 327 fowl paralysis becomes "a neoplasmatic affection," whatever that is ! ! Hutyra and Marek does not cater for rapid reference, even the initiated learn to rely on their memory and sense of direction ; there is no general index for the three volumes ; each volume has its own.

Other defects are a nomenclature, particularly bacteriological, which to English readers is either unfamiliar or archaic ; a waste of space on the reproduction of long-discarded notions of etiology ; and the citing of the names of authors unaccompanied by any reference to the date and source of their publication. In his own subjects, such as the chapter on milk fever, Prof. Russell Greig sets a standard of clear, authoritative and condensed writing which stands out in sharp contrast to the prolix continental style of the greater part of the book.

According to his preface the editor defends his retention of the continental posology, while claiming that Hutyra and Marek serves the needs of English-speaking veterinary practitioners. His mistake is not in retaining the continental therapeutics, but in failing to add the English posology. The English veterinary practitioner would claim, not that Hutyra and Marek's therapeutics should conform to, but that they should reflect much more of, the British system of teaching and practice.

In other important respects Hutyra and Marek pays little respect to English practice. The neglect of bovine streptococcal mastitis is a grave defect. Three other such important subjects in Great Britain as lamb dysentery, pulpy kidney disease and enterotoxæmia are squeezed into two pages, whereas malignant œdema receives 19 pages and 7 illustrations. Beyond a cursory mention of tumours which may be found in certain organs, the subject of neoplasia in animals and birds is neglected ; lipomatosis of cattle and carcinoma of the œsophagus of cats are not even mentioned. The subject of canine nephritis is a muddle ; it appears in the first volume as dog typhus or Stuttgart disease on the long-discarded hypothesis that it was caused by a spirochæte ; whereas in the second volume it is differentiated from typhus by the absence of gastro-enteritis and of spirochætes from the urine. Other sections requiring thorough revision are those dealing with endocarditis, congenital heart defects, goitre, intestinal obstruction and gall-stones ; and the entire section on so-called articular rheumatism might with advantage be rewritten in accordance with modern knowledge of rheumatic disease and the various forms of infectious arthritis.

The sections dealing with veterinary parasitology have been improved, at least from the systematist's point of view. The persistent use of the suffix *-osis* provides such novelties as ancylostomatosis, filariosis, schistosomosis and produces a mix-up with strongylidosis (p. 323), trichostrongylosis (p. 270) and trichostrongylidosis (p. 264). Somehow the familiar term *myiasis* (p. 255) was overlooked. Drugs are prescribed for the treatment of stomach worms in *wild* hares, without instructions on how first to catch your hare. The statement (Vol. III, p. 526) that "in foxes on fur-farms lungworms are normally present" is inaccurate; moreover, anyone familiar with fur-farms does not use the term "minks" for the plural of mink. This unfamiliarity with the practical side of parasitology may explain the inadequate advice for the prophylaxis of ancylostomiasis in foxes and dogs, and the neglect of the English work on *Oslerus osleri.* It is unnecessary to add the *s* for the plural of quail and ptarmigan.

This atmosphere of unreality and practical inexperience pervades much of the writing on disease of birds. The statement that pigeons "are seldom severely or fatally affected" by coccidiosis is the reverse of the actual facts. Obstruction of the crop of young pheasants is attributed to feeding on insect larvæ, which happen to be the bird's natural food. Palpation of the gizzard is advised for the diagnosis of grit deficiency, and glass, cinders, coal, fruit-stones, and brick dust are cited as "unsuitable substances" in the gizzard of the fowl!! The "wavy parrot" more widely known as the budgerigar, *Melopsittacus undulatus,* is mis-spelt on p. 331 together with *Pacheco and Bier* and *parrakeets.* Psittacosis is merely allotted one page as a sub-section of fowl pest, and along with another important subject, laryngo-tracheitis, requires revision. Coryza (p. 453, not 458 as in the index), should be separated from nutritional roup. Chondrodystrophy and disease of the oviduct appear to have been overlooked.

To make this fourth edition of Hutyra and Marek, Prof. Russell Greig has added his sparkling English vintage to the heavy continental wine in the old Budapest skins. The result is an affront to the English reader's palate. Also the edition does not fulfil the need of the times, it is neither a senior student's book nor an authoritative research manual. In the reviewer's opinion the time has arrived for the production of two books which will reflect the fact that during the past quarter of a century the veterinary science and practice of the English-speaking peoples has attained the lead in the world of veterinary ideas and study. The book on comparative pathology should be comparable in quality and mode of production with the M.R.C.'s System of Bacteriology. A brief summary of such a system of comparative pathology, added to the modern teaching and practice of veterinary therapeusis, would supply the second need—namely, a first-class text-book of veterinary medicine for the use and within the purchasing power of senior veterinary undergraduates. In order to exert a profound influence upon the veterinary practice of English-speaking peoples a book of veterinary medicine must appeal to and become the familiar companion of the student and young graduate. Hutyra and Marek fails to do this.

TOM HARE.

Domestic Pests: What They Are and How to Remove Them.

By L. HUNTER, M.Sc. [Pp. xii + 235, with 116 figures.] (London: John Bale, Sons, and Curnow, Ltd., 1938. 7*s.* 6*d.* net.)

THE preface states that this book is prepared "in the hope that it will be useful to housewives, students training as teachers, especially in domestic science, welfare workers; sanitary inspectors; adult education students and to members of the medical profession." The book includes chapters on the significance of pests, methods of control, and accounts of personal pests, pests in food, fabrics, wood, and pests of the house such as cockroaches, ants, rats and mice. Due acknowledgment is made of the many authorities consulted. There appears to be but little original matter in the book.

It is surprising that, prepared for such a public, the subject-matter should have been presented in so simplified a form and on the tedious plan of question and answer. In the process of simplification the author should have avoided general and popularised statements which have led to inexactitude and indeed to many serious errors. For example, a description of the three pairs of appendages which form the mouth parts of even so primitive an insect as the cockroach (which, by the way, is repeatedly called a black beetle) is evidently considered as too detailed for this book, so " the two mouth parts, the mandibles, " only are mentioned. But a large and not altogether accurate diagram of the highly modified mouth parts of the bed bug is allotted nearly a whole page although there is no explanation of the function, origin or homologies of the parts named in the legend.

One suspects that the author has little personal experience of some of the methods of control which she describes; for example, a well-baited cockroach trap will collect a couple of dozen specimens in a very short time, and this is a really effective method of tackling a serious infestation.

The glossary is inadequate and some of the definitions are inaccurate or, like many of the phrases in the text, ambiguous.

P. C. E.

PHYSIOLOGY AND MEDICINE

The Stuff We're Made Of. By W. O. KERMACK, M.A., D.Sc., LL.D., and P. EGGLETON, D.Sc. [Pp. viii + 341, with 8 plates and 55 figures.] (London: Edward Arnold & Co., 1938. 7*s*. 6*d*. net.)

THE authors state it to be their object to survey the region between the physical and biological sciences and to give the reader a reasonable description of the relation of the living to the non-living and, at the same time, to present an account of some of the remarkable advances in biochemistry and to indicate the significance which they possess for the future well-being of mankind. Under an apparently flippant title for the book the authors have disguised a really masterly account of the subject they are dealing with. In the first chapter, entitled " A Sense of Proportion," they give an account of the relative sizes of a number of well-known objects, beginning with the proton and ending with the universe, and use this as a means of introducing the reader by easy stages to the logarithmic scale of measurement. The second chapter contains, under the heading of " The Scientist takes a Sample," a very lucid account of statistical errors and errors of sampling and stress is laid on the fact that it is the aim of Science to provide statements which combine the highest possible significance with the greatest possible reliability. Under the title of " What is Truth ? " the reader is given as simple an account of the principle of indeterminacy as we have yet seen. A more distinctly chemical note is struck in the chapter devoted to Giant Molecules; though excellent in the general account it gives of highly polymerised compounds, it contains one rather ambiguous statement to the effect that " artificial silk is real silk built out of sugar instead of amino-acids." Much illuminating information is provided in the two following chapters on engines and energy and Nature's use of overdraft by a liberal use of simple analogies. The misleading title of " The Vitamin Racket " provides a very readable account of the discovery, isolation and synthesis of the various vitamins, and so far from belittling their significance, as the title would suggest, their important influence upon human and animal welfare is emphasised. In discussing enzymes under the heading of "Nature's Stepping Stones" the authors have thought fit to introduce a quasi-humorous drawing which by a curious error of judgment has been reproduced on the specimen page issued as a circular; to most casual observers this would act as a deterrent to reading a book which is, in point of fact, one of most outstanding merit. Space does not permit of referring to the many other points of interest; suffice it to say that in a free-and-easy style the authors have produced a most amusing and stimulating book which can be profitably read from cover to cover.

P. H.

The Living Body: A Text in Human Physiology. By CHARLES HERBERT BEST, M.A., M.D., D.Sc. F.R.S., and NORMAN BURKE TAYLOR, M.D., F.R.S.C. [Pp. xxiv + 563, with 15 coloured plates and 283 figures.] (London: Chapman & Hall, Ltd., 1939. 18*s*. net.)

As the authors state in their preface, "This book is addressed to those seeking an elementary knowledge of the actions and reactions, physical and chemical, occurring in the human body." The book is profusely illustrated with diagrams which are on the whole very good. The type is well spaced and easily read. The text is clear and concise and very readable. Sufficient anatomy has been included to make the functional aspect clear to students who have no knowledge of structure. There are many histological diagrams, some of which are rather fanciful. A certain amount of biochemistry has been included, some of it perhaps wrongly in an elementary book of this type, as for instance the chemical formulæ for vitamins. On the whole it is surprising how much the authors have condensed into the 538 pages of the book.

It is a pity that in dealing with blood groups the authors have used the Jansky classification. It is sure to lead to difficulties, if not danger, should this book be the sole guide for nurses in Great Britain. It would be advisable in future editions to refer at least to the fact that there are other systems of classification.

The subject-matter of the book is adequate for elementary students of the subject, such as nurses and dental students. The authors are to be congratulated on producing such a well-written and instructive work.

H. P. G.

Annual Review of Physiology. Vol. I. Edited by JAMES MURRAY LUCK and VICTOR E. HALL. [Pp. viii + 705.] (Stanford University, California: American Physiological Society and Annual Reviews, Inc., 1939. $5.)

THIS book presents critical accounts of the recent progress in a score or so different fields of physiological research. In these days of ever-increasing specialisation, it is becoming wellnigh impossible for the research worker to know "everything about something," much less "something about everything"; and any book which offers us a hope of achieving the latter part of this precept, as far as recent discoveries are concerned, is assured of a warm welcome.

As the editors wisely point out in the Preface, exhaustive treatments of selected topics are already available in *Pysiological Reviews* and the *Ergebnisse* *der Physiologie. The Annual Review of Physiology* is therefore designed to supplement such journals by providing critical and interpretative résumés covering year by year the entire field of physiology.

The topics included in this, the first volume, cover a wide range, including among many others, Permeability (M. H. Jacobs), Genetics (G. W. Beadle), Developmental Physiology (J. Needham), Peripheral Circulation (H. C. Bazett), Respiration (R. Gesell), Heart (J. A. E. Eyster), Reflex Action (J. J. Eccles), Kidney (Homer Smith), Endocrines (H. M. Evans); and the extensive bibliographies at the end of each chapter bear witness to the painstaking care with which the authors have performed their unenviable tasks.

In a few cases too much has been attempted, and the result is an uncritical cataloguing of papers which, difficult as it must be to avoid, makes heavy going for the average reader. However, the majority of the contributions are excellent, particularly those by J. J. Eccles and H. M. Evans, which might serve for future contributors as a model of what is required.

It can be said of few books that their usefulness will certainly increase with their age; but this is one of them, and no one who wishes to take an intelligent interest in the progress of modern physiology can afford to be without it.

R. A. G.

Laboratory Outlines in Comparative Physiology. By Charles Gardner Rogers, Ph.D., Sc.D. Second edition. [Pp. ix + 133.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 8*s*. 6*d*. net.)

Rogers's book, now in its second edition, provides useful directions for a course in animal physiology, with the emphasis laid on the physical and chemical aspects of the subject. The first section contains a very instructive choice of experiments related to Physical Chemistry of Cells. It is followed by a short chapter on "The General Physiological Properties of Protoplasm" and a longer one on "The Internal Medium," which also contains general protein reactions. "Circulation" is dealt with extensively in Section IV. It contains some experiments which are perhaps too elaborate for Zoology students. Section V on "Respiration" occupies 29 pages, the following on "Nutrition" only 7. It might be profitably enlarged in a future edition to include methods for the quantitative estimation of nutrients. The last chapters are on "Physiology of Movement," "Excretion" (1 p.) and "The Nervous System and Behaviour."

There are no diagrams illustrating the apparatus described in the text. This is particularly regrettable in instances where the technique suggested is not widely used. The usefulness of the book, especially for teachers, would be increased if directions were given for the preparation of certain reagents (Millon's, Benedict's solution, etc.). In the reviewer's opinion there are too many questions such as "What happens?", "Why?", "Result?", instead of explanations of the experimental results. Thus the directions very often assume the character of examination questions and make the use of the book too much dependent on the course of lectures running closely parallel with that of the practicals.

F. G.

Food and Health: An Introduction to the Science of Nutrition.

By A. Barbara Callow, M.A., M.Sc., M.S. Second edition. [Pp. viii + 168, with 9 figures, including 4 plates.] (Oxford: at the Clarendon Press; London: Humphrey Milford, 1938. 5*s*. net.)

In reading any modern book on diet there are three questions one asks oneself: " Is the book up-to-date ? "—" Is it easily readable from the lay point of view ? "—" Is it free from the taint of ' food faddism ' ? "

In the case of *Food and Health* all these three questions can be answered unhesitatingly in the affirmative. The book has another very good point; while the writer clearly knows her subject and its now very extensive literature, she would seem to have a flair for knowing what to leave out, often a more difficult matter to decide than that of what should be put in. In this latter connection, of what to put in, time after time when some important point came into one's mind, and the index was turned up to see if it had been dealt with, the reference was found and the text-matter proved to be adequate and satisfactory. In fact, it is not easy to find faults, though the section on constipation might perhaps have been handled rather differently. In this connection, the distinction between a spastic and a non-spastic colon is rather important, with the desirability of a non-roughage diet for the spastic type. Physicians to-day state that up to 50 per cent. of the constipation modern man suffers from is due to tonic contraction rather than to atonicity; in other words, to the spastic type of colon. Perhaps in a later edition this matter might be made somewhat clearer and brought more up to date. For the rest, the book seems to fulfil its avowed purpose admirably, and, perhaps a minor detail, the type and paper are unusually pleasant for a book of this moderate price.

V. H. M.

Vitamins and Vitamin Deficiencies. Vol. I: Introductory and Historical; Vitamin B_1 and Beri-Beri.

By Leslie J. Harris, Ph.D., Sc.D., D.Sc., F.I.C. [Pp. xiv + 204, with 50 figures.] (London: J. & A. Churchill, Ltd., 1938. 8*s*. 6*d*. net.)

This invaluable book has set for the other six projected volumes in the same series a standard that probably only its author could maintain. It is something less than a mere compilation in size, but something very much more in labour. It is a selection, and a critical selection, of all the information about Vitamin B_1, in presence and absence, likely to be of use to the practising physician or to the biochemical research worker. Dr. Harris's position as Director of the Nutrition Laboratory of the Medical Research Council and the University of Cambridge renders him uniquely fitting to undertake the task of preparing this set of monographs. His approach is fundamentally chemical, for as a chemist he was trained, though his work during the last two decades has brought him into close contact with medical problems, especially those arising from nutritional deficiencies.

That Dr. Harris's survey is comprehensive and integrative appears at once from his discussion of the chemical structure of aneurin and its relation with cocarboxylase and the complex enzyme systems involved in carbohydrate metabolism. That his outlook is also practical is witnessed by the critical consideration given to the various methods proposed for the biological estimation of Vitamin B_1 and the different chemical tests.

When the whole of the volumes in this series are available, they will constitute a complete library of reference for all those whose work lies among or near the vitamins, and a source of information whose permanence will be secured by Dr. Harris's intention and ability to carry out the necessary revisions from time to time.

A. L. Bacharach.

Laboratory Manual of Physiological Chemistry. By Meyer Bodansky and Marion Fay. Fourth edition. [Pp. viii + 295, with 9 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 10*s*. net.)

This book provides a sound basis for any course on practical physiological chemistry and sufficient experiments are quoted to give the student a good working knowledge of the subject. The details for performing the various experiments are more than adequate, and, in the case of the quantitative work, alternative methods are given wherever possible. The chapter on urine is very good in this respect.

In the reviewer's opinion the book suffers from a serious defect, and that is its failure to give due consideration to the principles on which the various methods are based. It is always difficult to strike a correct balance between theoretical and practical work in a laboratory manual, but the reviewer feels that in this case the authors have neglected the former. The minimum requirements are, that the principles of each method should be carefully explained before going on to the practical details, and in most cases this has not been done.

This is of little consequence if the book is to be used by qualified men who want practical details only, but if it is to be used by students, the reviewer is of the opinion that its value would be enhanced by devoting more space to general principles.

S. R. Elsden.

A Textbook of Biochemistry. By Roger J. Williams, Ph.D., D.Sc. [Pp. x + 525, with 19 figures.] (New York: D. van Nostrand Co., Inc.; London: Chapman & Hall, Ltd., 1938. 21*s*. net.)

This book which deals primarily with animal biochemistry is intended for medical students and others with similar interests; it is divided into five sections which, according to the author, seek to answer in order the following five questions: (1) How are biochemical materials constituted? (2) Of what are our body tissues composed? (3) Which of the chemical constituents of foods are necessary for nourishment? (4) What mechanisms over the body apply in the control of chemical transformations? (5) What are the details of the process whereby the food materials are transformed into tissues with their living activities? It will be seen that the plan adopted by the author is a logical one which should give the student a good grasp of the subject, but it is also an ambitious plan and in his desire to make the story as complete as possible the author has tended to condense his information so much as to make it difficult for the student to follow him; the experienced reader, on the other hand, will find much in the book that is to be commended, as it provides an excellent and up-to-date summary of our present knowledge of the subject. The book is remarkably free from misprints, but the ring oxygen atom in the cyclic formula for fructose has accidentally been omitted on p. 45.

P. H.

Outlines of Biochemistry. By Ross Aiken Gortner. Second edition. [Pp. xx + 1017, with 165 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 30*s.* net.)

The title of this text-book is perhaps a somewhat misleading one. The very size of the volume, over one thousand large pages containing about forty chapters, is alone sufficient to make one suspect that the outlines have at least in places been fairly well filled in. But this is not an ordinary biochemical text-book. It deals mainly with the basic facts and principles on which the science of biochemistry rests, rather than with the systematic, detailed application of these to the explanation of particular processes in the tissues of animal or plant. These facts and principles belong in large measure to the sciences of physical, colloid, and organic chemistry. Consequently this book gives a wide survey of this fundamental groundwork. The thorough discussion of the physical chemistry underlying cell activity is especially valuable, for in most books on biochemistry it tends to be treated somewhat summarily. The organic chemistry part of the book is also very well written. The volume is the more readable because the author treats many interesting problems, and practical applications of the facts and principles he is discussing. Almost every page contains many references to the original literature, whilst the subject index alone occupies 38 pages. In this new edition the contents of the book have been brought up-to-date, and it is an interesting sidelight on the rapidity of the progress of the science to notice how much is included that has been discovered during the nine years that have elapsed since the first edition was published. Undoubtedly this is a book that should be in every biochemical library.

W. O. K.

Biochemistry for Medical Students. By W. V. Thorpe, M.A., Ph.D. [Pp. viii + 457, with 37 figures, including 4 plates.] (London: J. & A. Churchill, Ltd., 1938. 12*s.* 6*d.* net.)

This is a more advanced book than the title suggests, but this should not deter medical students from reading it, for all the topics are dealt with in a simple fashion, and it merely means that the book will enjoy a much wider circulation.

There is very little repetition, the reader being referred to the page on which the information is to be found. It is a good scheme, serving to teach the student how to use references and, what is more important, how to use a library. This latter object is furthered by a carefully chosen set of references and an adequate index.

Another pleasing feature is Dr. Thorpe's effort to give, wherever possible, the experimental evidence on which the existing theories are based, and the assumptions made in the interpretation of this evidence. This point is particularly well illustrated in the chapter on muscle chemistry, where the author emphasises that most of the evidence for the present theory of glycolysis has been obtained by the use of muscle extracts and breis.

A possible criticism is that certain chapters are unbalanced. This is most marked in the chapter on biological oxidations, in which the theory of redox potentials is discussed for two pages, but no mention is made of the important respiratory coenzymes, and their relation to the anti-pellagra vitamin, nicotinic acid amide. Further, the reviewer feels that the average medical student would appreciate some information on stereo and geometrical isomerism, more especially when he comes to the competent chapters on carbohydrates and sterols.

Taken as a whole, this is a good book and Dr. Thorpe is to be congratulated. Praise is also due to the publishers for the way in which the book is set out and the excellent treatment of the illustrations and the numerous formulæ.

S. R. Elsden.

A Textbook of Biochemistry for Students of Medicine and Science. By A. T. Cameron, M.A., D.Sc. (Edin.), F.I.C., F.R.S.C. Fifth edition. [Pp. viii + 414, with 3 plates and 25 figures.] (London: J. & A. Churchill, Ltd., 1938. 15*s*. net.)

That no less than five editions should have been called for in the space of ten years is in itself sufficient testimony to the merit of this book. In the present edition, however, the book has been almost completely re-written in the light of the many recent advances in biochemistry which have enabled a clear insight to be obtained into the interrelationships between various groups of compounds. This is specially noticeable in the close connection between enzymes, hormones and vitamins, since it is known that at least one vitamin united to a protein forms an important enzyme, while another combined with a phosphate acts as a co-enzyme. In the treatment of his subject the author has assumed the reader to have had a sound training in organic chemistry, but not making the same assumption concerning the reader's training in physical chemistry he has devoted an early chapter to the subject of "Physical Chemical Concepts of Importance in Biochemistry." The information here given is of necessity very condensed, since it attempts to cover a large field in some 30 pages; incidentally by a curious oversight the name of Thomas Graham is given as Thomas Hunter on p. 31. The subject of enzymes is very well treated in the third chapter, as is also that of vitamins and, with a view to emphasising the real existence of these substances, a number of photomicrographs of them have been provided. Then follow a series of chapters dealing with carbohydrates, lipids, proteins and in which not only their chemistry but also their digestion, absorption and utilisation is described. The remainder of the book treats the subject in a very praiseworthy manner from the human physiological point of view, bringing chemistry closer into relation with the body fluid and body tissues, and good accounts are also given of extra- and intra-cellular respiration involving of course a detailed discussion of the intricacies of oxidation-reduction systems. Novel features are the inclusion of such subjects as Bacterial actions and Detoxication mechanisms and a Biochemical Introduction to Pharmacology which includes clear accounts of immunochemistry and the present state of knowledge regarding Filtrable Viruses. In addition, there are two valuable chapters on Quantitative Metabolism and Diet, both of which contain much useful information which is not quite easy to find without a good deal of search in scattered literature. Students of medicine can hardly be expected to remember all that is contained in the closely printed pages of this excellent book, but as a book of reference it may be highly recommended to them as well as to students of chemistry desirous of obtaining information on biochemistry in relation to human physiology.

P. H.

Laboratory Methods of Biochemistry. By A. BERTHO and W. GRASSMANN. Translated by W. McCARTNEY, Ph.D., F.I.C. [Pp. xiv + 281, with 33 figures and 1 folding plate.] (London: Macmillan & Co., Ltd., 1938. 18*s.* net.)

THIS admirable book will be found invaluable in the teaching of practical biochemistry, and is also recommended to trained organic chemists who wish to gain familiarity with biochemical technique. The general plan is based on that of the well-known Gattermann's *Laboratory Methods of Organic Chemistry,* and after chapters devoted to introductory matter and to general descriptions of apparatus and experimental technique, the main subject-matter is classified under the three headings of "Substrates," "Enzymic Hydrolyses," and "Respiration and Fermentation." This is in no sense a book of recipes. The theoretical treatment is excellent, and a valuable feature is the abundance of references to the original literature, and especially to review articles, which will be much appreciated by the reader who desires fuller information on restricted topics. Methods of isolation, purification, and analysis are given, and of the determination of physical constants such as p_H and oxidation-reduction potentials. In a work covering so wide a field as modern biochemistry it would be easy to cite omissions. Thus, there appears to be no reference to electrophoresis, and a special section on micro-technique would be welcome. The general reaction, however, is one of admiration that such a wealth of material should be attractively presented in so small a compass. There are descriptions of the preparation and properties of enzymes, of the study of cell metabolism by Warburg's method, and an extensive discussion of the mechanism of fermentation and respiration, to select only three courses from a rich repast.

J. W. C.

A Text-Book of Pharmacognosy. By G. E. TREASE, B.Pharm., Ph.C., A.I.C., F.L.S. Third edition. [Pp. x + 740, with 233 figures and 10 maps.] (London: Baillière, Tindall & Cox, 1938. 21*s.* net.)

THIS text-book is designed to meet the requirements of students who are reading for a degree in Pharmacy, or a similar professional qualification. The text has been revised, and brought up-to-date, and new chapters and figures have been added. Several specialists have contributed to a treatise which presents a difficult subject in its varied aspects in a way which the student will find interesting and instructive.

The work is divided into five sections. Part I brings together such subjects as the general history and commerce of crude drugs, plant principles and their extraction, plant enzymes, cultivation, collection, drying, storage, insect and other pests. Part II treats of the technique of microscopy and its application in pharmacognosy. Part III is the main section of the book. In it the drug-yielding plants are arranged according to their families, following the system of Engler. The drugs are described more or less fully according to their relative importance, and information is given as to geographical source, methods of cultivation, collection, preparation for the market, packing and storage. Details of histological structure are given for those drugs used in powder form, and descriptions of possible adulterants and substitutes. Part IV is concerned with drugs of animal origin, and Part V deals with chemical and physical methods of drug analysis. Here we have such modern developments as fluorescence analysis described.

An important feature of the text-book is the wealth of illustration. Many photographs and blocks from other works are reproduced, and numerous illustrations have been adapted for reproduction. Some of these latter tend to detract from the otherwise general excellence of the book. Their lines are too thick and the shading too heavy (*e.g.* Fig. 48, p. 131) or, they are not sufficiently critical (*e.g.* Fig. 133, p. 394).

Apart from these details, both Mr. Trease and the publishers are to be congratulated on the production of a most useful and efficient text-book which will undoubtedly have a wide appeal.

W. O. Howarth.

The Microscopical Study of Drugs. By Lilian A. Kay, B.Pharm., Ph.C. [Pp. viii + 228, with 47 plates and 10 figures.] (London: Baillière, Tindall & Cox, 1938. 10*s.* 6*d.* net.)

This book presents a scheme of practical work on the microscopy of the vegetable drugs included in the syllabus for the Pharmaceutical Chemist Qualifying Examination and the Bachelor of Pharmacy Examination of the University of London. The scheme comprises a series of 40 schedules of work, each designed to be completed in approximately three hours. While these schedules do not differ essentially from those available in other text-books, the author has produced a useful volume by incorporating a comprehensive series of original drawings.

Each schedule contains instructions for the preparation and examination of sections, isolated tissues and powder of the drug and concludes with a summary of the diagnostic microscopical characters. Accompanying each is a plate showing a plan of the section and detailed drawings of sections, isolated tissues and powder. In addition to schedules dealing with thirty important vegetable drugs, chapters are included also on the drawing and measurement of microscopical objects, the use of reagents, the examination of fibres and fabrics used for surgical dressings and quantitative microscopical analysis by the lycopodium spore method.

Miss Kay's drawings are, on the whole, quite good, although the accuracy of some of the finer details is open to criticism and a considerable error is shown in the arrangement of the " perimedullary phloem " of the Solanaceous leaves.

Pharmaceutical students and others taking up the study of drug microscopy should find this book useful both as a practical guide and as a work of reference.

E. T.

Medical Entomology. By William A. Riley, Ph.D., Sc.D., and Oskar A. Johannsen, Ph.D. Second edition. McGraw-Hill Publications in the Zoological Sciences. [Pp. xiv + 483, with frontispiece and 184 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 25*s.* net.)

It is not easy to classify the applications of biology to practical affairs. The author of a work on weeds of field and garden might divide his subject taxonomically, grouping together those weeds which belong to particular families; alternatively, he might divide his subject into the weeds of root crops, of cereals, or of the kitchen garden. The classification of medical entomology is even more difficult. Some insects, such as the bed bug, are included simply because they are nuisances. Many others, such as fleas and mosquitoes, transmit organisms which cause disease. In other cases, closely related organisms are transmitted by unrelated hosts; for instance, the spirochætes of different types of relapsing fever are transmitted by lice or by certain ticks. As a further complication, the lice are not only involved with relapsing fever, but also with two diseases of the typhus group. It follows that if the subject is classified by diseases, it will appear muddled to a zoologist; but zoological classification sunders things which the medical man would wish to bring together. As the reviewer holds that the object of medical entomology is to throw light on the epidemiology of certain diseases, he would prefer the former classification.

The authors of the present book treat the subject as a branch of zoology. They introduce the Arthropoda, work through the Arachnida, and then pass to those Hexapoda which have medical importance, reviewing them order by order. This arrangement is clear and the authors have produced a balanced statement on the medical entomology of the world. Their second edition brings up to date a convenient manual, which is already known. But the medical reader will not find the zoological method of presentation very convenient. He will notice that chapters on beetles and moths (quite trivial insects to him, and only included because a few are poisonous or irritating) are inserted between the lice and the flies; moreover, if he wants a clear picture of the medical entomology of the typhus diseases, he must hunt it up in three places, under lice, mites and fleas, and put it together for himself.

P. A. BUXTON.

Introduction to Physiological Optics. By J. P. C. SOUTHALL. [Pp. x + 426, with 135 figures, including 3 plates.] (London: Oxford University Press, 1937. 21*s*. net.)

HELMHOLTZ published the first edition of his *Physiological Optics* in 1867. The second edition appeared in 1896, whilst the third appeared in 1909–10, fifteen years after his death. The book was so far ahead of its time that it was still worth while to publish an English translation in 1924. Ignoring the contemporary additions, this can still be used as a text-book, although its style is a little old-fashioned. Very few of the statements of fact have been shown to be untrue, and only one fresh principle has been discovered. This is the mechanism by which sensory impulses are conveyed to the brain: as a result of this, sensory physiology has made a leap forwards. We are therefore astonished to find that Prof. Southall has ignored this work. Like the third English edition of Helmholtz, the part which is Helmholtz is good, but neither work is improved by the additions of a few, uncritically selected, modern American authors.

The chapters on the optical system of the eye are well done. Some of the data cannot be found in other English books without much searching. Those who have a knowledge of optics will enjoy reading these chapters. Prof. Southall opens the optical section by saying that he is going to give a brief outline of the elementary theory of optical imagery. In the next sentence he takes it for granted that the reader is acquainted with the fundamental principles of geometrical optics. There follow a few sentences which do not matter either way and then we find ourselves anticipating a lucid account of the geometrical optics of thick lenses, but we learn that the constructions are "so familiar that they hardly require any explanation." They certainly do not get it.

On the whole the clinical methods described are not those used in this country. It is stated that "the most useful clinical methods of ascertaining the nature and degree of ocular astigmatism" are Placido's disk, Gullstrand's keratoscope, the ophthalmometer, the skiascope and subjective tests. In a book on physiological optics we might expect a full account of the best and most generally accepted method for estimating astigmatism, namely that of retinoscopy. We look in vain, however.

Prof. Southall is obviously much interested in the history of his subject and the text is full of quotations from old works and literary illusions. He has a real feeling for the past. It is a pity that he is not more sympathetic to the present.

R. J. L.

Chronic Diseases of the Abdomen: A Diagnostic System. By C. Jennings Marshall, M.S., M.D., F.R.C.S. [Pp. xvi + 247, with 131 figures, including 4 coloured and 78 black and white plates.] (London: Chapman & Hall, Ltd., 1938. 25*s*. net.)

This book is not only an exceptionally useful work of reference, but it is a delight to read.

As a reference-book for the busy practitioner, it is arranged under headings of methods of examination (Part I) and symptomatology (Part II). Under most symptom-headings a complete differential diagnosis is given. As a book to read it gives a consecutive and clear account of the known chronic abdominal diseases. Mr. Marshall does not fail to quote the orthodox, but equally does not hesitate to give his own very experienced opinion when it is contrary to the orthodox. That his style is racy and his comments often interpolated with dry humour add to the value of the book as well as to the pleasure of reading it. The book is illustrated by radiograms, photos and graphs, all of which are clear and illustrate definite points in the text.

E. P. S.

HISTORY OF SCIENCE AND PHILOSOPHY

Galileo and the Freedom of Thought. By F. Sherwood Taylor, Ph.D., M.A., B.Sc. The Library of Science and Culture. [Pp. xvi + 212, with frontispiece and 14 figures, including 4 plates.] (London: C. A. Watts & Co., Ltd., 1938. 7*s*. 6*d*. net.)

The scope of this book is wider than its title might suggest. It is a complete sketch of Galilei's life and labours intended for the general reader, to whom it may be confidently recommended.

Dr. Taylor rightly stresses Galilei's positive achievements as a discoverer, and as a pioneer of the scientific method, rather than his wrongs as a victim of the Inquisition. He includes, however, a careful and objective account of Galilei's conflict with the Church, as it must have appeared to the participants, and he quotes extensively from the fundamental documents. He is of the opinion that Galilei did not suffer torture, but that his condemnation for heresy was technically unsound, since the Copernican doctrine had not been pronounced heretical by the Pope, or by a General Council of the Church. It is a familiar story; but the growth of restrictions upon intellectual freedom in the world to-day gives it a new significance.

The book is based upon an independent study of original materials; and Dr. Taylor gives a straightforward and competent exposition of Galilei's principal discoveries and arguments. If one small criticism may be made, he seems to have misunderstood the bearing of Galilei's discovery of the phases of Venus upon the Copernican controversy (p. 67). The occurrence of such phases, it is true, did not prove the Earth to be in motion; but it showed, firstly, that the planet Venus was a dark body like the Earth, receiving its light from the Sun, and, secondly, that Venus was revolving about the Sun, as required by the Copernican, but not by the Ptolemaic, theory.

The illustrations include three portraits of Galilei, together with reproductions from his works, and explanatory diagrams. There is also a select bibliography of seventy-two books by, or relating to, Galilei.

A. A.

Copernicus: the Founder of Modern Astronomy. By Angus Armitage, M.Sc. [Pp. 183, with 2 plates and 37 figures.] (London: George Allen & Unwin, Ltd., 1938. 10*s*. net.)

This book is a worthy and much-needed addition to the History of Science Library. It is not merely a biography of Copernicus—that has already been done adequately enough by Prowe and by Birkenmajer: it traces the history of hypotheses of the planetary system from antiquity through the Middle Ages to the final triumph in the seventeenth century of the heliocentric theory propounded by Copernicus in the sixteenth.

The main part of the book is devoted to a detailed analysis of the *De Revolutionibus*, which is studied comparatively with Ptolemy's *Almagest*. An epilogue describes the vicissitudes of the heliocentric theory after Copernicus' death. In these days, when the theory is so universally accepted as to seem almost axiomatic to the general public as well as to astronomers, it is fascinating to read of its early struggles against clerical and political prejudice, and indeed against established authority of every kind.

The treatment will appeal not only to the specialist but also to that wider circle of readers whose interest in Copernicus is more as a pioneer in the evolution of modern thought than as the founder of modern non-sidereal astronomy.

A. H.

The Collected Works of George Abram Miller. Vol. II. [Pp. xii + 537, with frontispiece.] (Urbana: University of Illinois Press, 1938. $7.50.)

The period 1900–07 covered by the second volume of Prof. Miller's collected works was one in which the number of articles on group theory published in English considerably exceeded those in all other languages together. In America it was a time of ever-growing vitality, symbolised among other incidents by the founding of the "Transactions of the American Mathematical Society." The schools working under the direction and inspiration of L. E. Dickson and G. A. Miller developed the pioneer work of the French, German and Italian mathematicians of the previous generation; in Europe, W. Burnside was the outstanding and most active figure.

The volume opens with an eighteen-page "Note on the history of group theory during the period covered by this volume," specially contributed by Prof. Miller. Two problems bequeathed by the nineteenth century to the twentieth were solved early in the period in question: the first that if a transitive group of degree n involves exactly $n-1$ substitutions of this degree, then these substitutions together with the identity always constitute a group; and the second that every group whose order is not divisible by more than two distinct prime numbers is solvable. The influence of both these problems is reflected in several of the papers reprinted in this volume. A third problem did not reach its solution until the end of the period, namely that the enumeration of all finite groups of collineations in three variables, which was begun by C. Jordan in 1878 and later supplemented by F. Klein and H. Valentiner, is actually complete. This was finally established by H. F. Blichfeldt in 1907.

During the period represented by this volume, Prof. Miller published nearly one hundred and fifty mathematical papers, including articles and reviews, in American, English, French, German, Italian and Polish periodicals, and in almost as many languages. One hundred and seven of these papers are reprinted here in full, the remainder are recorded by title. A mere half-dozen of the reprinted papers are of an elementary or popularising standard; the remainder represent definite contributions to group theory. The volume ends with a sketch of " Primary facts in the history of mathematics " (36 pp.) specially written to emphasise the facts that inspired Prof. Miller's writings on historical topics.

Again we congratulate and thank the University of Illinois, and look forward to the third (and how many more ?) volumes.

E. L. I.

A History of Science, Technology, and Philosophy in the Eighteenth Century. By A. Wolf. History of Science Library. [Pp. 814, with 345 figures, including 128 plates.] (London: George Allen & Unwin, Ltd., 1938. 25*s*. net.)

The first volume of Prof. Wolf's history of science appeared in 1935, and the present volume carries the story into the succeeding century. Those who have used the first volume will be familiar with the general plan of the work, but it may be mentioned that it deals with topics, starting with the " exact " sciences of mathematics and astronomy, passing through physics and chemistry to the natural sciences of botany, zoology, geology and geography, to medicine and technology, and concludes with philosophy. This plan has obvious advantages and is perhaps the best for a large work, but it has the disadvantage that the work of several scientists must be divided. In some cases there is overlapping, as when the biography of Wilcke appears twice. It is no doubt difficult for an editor to avoid this before an index has been drawn up. The biographies appear in certain sections and it would seem desirable in future volumes to mark these by heavy type in the index. There are twenty references to Black, for example, and nine of them must be turned up before the biography is found. The book is amply illustrated by portraits.

A work of this size would be of little value unless extensive use had been made of original sources in its preparation, and there can be no doubt that this has been done. In most cases references are given, but some sections are not so well documented as others, and the book would have been improved if the bibliography had been fuller.

The author has provided useful short summaries and appreciations in many places. The book is commendably free from that type of scientific journalism which uses a half-baked knowledge of the history of science to expound some political or economic fallacy. In his general introduction Prof. Wolf tells us that the eighteenth century was notable for its rationalism, humanism and internationalism, and he at least knows what he is talking about and his opinions command respect, although we may not always agree with them.

The sections on mathematics, mechanics and astronomy have no great discoveries to chronicle, as the period was one of consolidation and systematisation rather than originality. The sections on astronomical and marine instruments are very interesting, although here, as in some other places, the illustrations are not sufficiently explained ; the lettering, for example, is not mentioned in the text. The chapters on physics, classified under the headings of light, sound, heat, electricity and magnetism, are among the best, and since important discoveries were made in these sciences in the eighteenth century, they are of uncommon interest. Particular attention is directed to measuring instruments, such as thermometers, calorimeters, electrometers and photometers. Other kinds of measuring instruments are described in the sections on meteorology.

The sections on chemistry are very well written and, in view of the important developments in that science made in the century, might have been longer, some other less important matters (*e.g.* the history of fireplaces) being curtailed. The natural sciences come in for interesting and well illustrated treatment.

The eighteenth century was a period of great activity in the development of technology, and the sections on this subject are very readable and useful, that on the steam-engine being especially noteworthy. The fact that many inventors were shamefully exploited by business interests, especially in England, although not specially emphasised, emerges clearly. This part of the work includes sections on mining and metallurgy, industrial chemistry, instruments, calculating apparatus and telegraphy. More references to sources would have been welcome in many parts.

The concluding part of the book gives a concise but informative survey of the social sciences, psychology and philosophy, and is noteworthy for the many quotations from original sources.

Prof. Wolf's second volume is a serious contribution to scholarship and has been written with care and accuracy. It deserves to be widely read and it is to be welcomed as a genuine historical work, written by specialists. Every science teacher and student should find it helpful and suggestive in his work. Many parts of it will be found intelligible and interesting by the general reader.

J. R. P.

A Short History of the Steam Engine. By H. W. Dickinson. [Pp. xviii + 255, with 11 plates and 78 figures.] (Cambridge: at the University Press, 1939. 15*s*. net.)

Those familiar with Dr. Dickinson's studies of James Watt and Matthew Boulton will look forward to opening his latest work. Divided into two sections, the first dealing with the history and development of the reciprocating engine, and the second with the steam turbine, this historical study provides a useful book of reference to the engineer, and, for its comparatively short length of 250 pages, includes a remarkable wealth of detail.

The possible criticism that insufficient attention has been paid to the locomotive and the marine engine is unwarranted, though perhaps the title might have been better chosen to stress the fact that these two extensively existing types of steam engine had not been included within the survey.

The book serves to bring out in true perspective the debts which civilisation owes to Watt and Trevithick for their revolutionary inventions in carrying the development of the steam engine many strides further during their respective lifetimes. The inventive genius of Thomas Savery, who set up the first steam engine workshop in the world, the work of Newcomen with his movement of the valves by beam motion, and the part played by Smeaton are set out in clear and connected form. Watt's addition of the condenser and as originator of engine performance tests and the centrifugal governor, not to speak of the expansive working of steam, form an attractive section of the study, whilst the story of Trevithick's realisation of the value of pressures, his steam carriage, steam locomotive and the use of a fusible plug follows in due rotation.

The benefits derived from Whitworth's machine table, Murdoch's slide valve, Belliss' forced lubrication and the Babcock & Wilcox water-tube boiler, all fit neatly and easily into this well-threaded lineage of steam-engine development. Admirably told is the romantic history of the work of Sir Charles Parsons, his energies and his difficulties.

Replete with a large number of reproductions and concluded with a valuable index, this work redounds to the credit of the author and very greatly to that of the Cambridge University Press. The type is admirably set, and the illustrations clearly spaced and well delineated.

C. E. R. Sherrington.

The Marxist Philosophy and the Sciences. By J. B. S. Haldane, F.R.S. [Pp. 183.] (London: George Allen & Unwin, Ltd., 1938. 5s. net.)

This little book is almost too full of interest and, as the author implies, it provokes controversy—most of the questions asked resemble the relation between brain and mind, which in principle is not an insoluble problem, yet cannot be solved except in the most summary manner until we know a very great deal more about the brain. What is Marxism is especially difficult to answer. It has been defined as a complete Theoretical system—but in the same paragraph we are told this is not true. It is not a system, and only secondarily Theoretical. It is not complete because it is alive and growing and lays no claim to finality. It is a method, and although Theory is essential to Marxism, Marx "proclaimed the primary of practice over Theory."

This book is based upon the Muirhead lectures on political philosophy delivered in the University of Birmingham in January and February 1938 and addressed to scientific workers and students.

Prof. Haldane is a new adherent to Marxism. His reason for publishing the lectures is chiefly that he thinks Marxism is true and in view of the general importance of the subject he hoped to interest a wider audience. It will appeal especially to students of philosophy, biology, psychology, and sociology—but the reader must be prepared for much effort of concentration as the style is in many places not easy, especially in the sections on psychology and quantum theory—and must expect disappointment if he anticipates a clear picture of the relations of Marxism to Science.

P. J.

MISCELLANEOUS

Van Nostrand's Scientific Encyclopedia. [Pp. viii + 1234, illustrated.] (New York : D. van Nostrand, Inc. ; London : Chapman & Hall, Ltd., 1938. 50*s*. net.)

The name of Van Nostrand is famous in America in connection with scientific publications ; the name of Chapman & Hall is famous here amongst publishers : this volume, therefore, attempts to make the best of both hemispheres.

The inevitable " sulfur " and " fall," and the unexpected legacy of the wild and woolly west in " corral " for " gather together," many examples of geology, certain ordinances and specifications, and a very good article on " Water Supply," make little appeal to us, as being too American. But apart from these examples and some others there is justification for almost unstinted praise. Twenty-one prominent Professors of American Universities have controlled over 10,000 scientific terms in a volume that is intended to be of value to both the layman and the technician. It covers chemistry, physics, mineralogy, geology, botany, astronomy and mathematics, with navigation, medicine, and civil, mechanical and electrical engineering, as well as aeronautics which takes a prominent place.

Some irregularity in the handling of subjects is almost inevitable. We find the " Lamarckian Theory " but not the " Darwinian Theory " ; " Scrofula " but not " Lupus." And " Concrete " contains no explanation of the mystic figures (to the Layman) of " 1 : 2 : 3 " and the like. There is some repetition in " Spleen," and " Water Wall " leaves the reader a little puzzled as to its purpose.

A welcome feature are the short but informative biographies of Bessell, Newton, Galileo, Kepler, Laplace, Newton and Ptolemy (the Geographer).

It is remarkable that only one misprint has been found, perhaps it is actually the only one in the book, and that is trivial and obvious—" bitumin " for " bitumen."

This Scientific Encyclopedia is without doubt a most useful and attractive volume for those seeking information in a condensed and " human " form, with an excellent impartiality when dealing with controversial matters.

Percy E. Spielmann.

Das Mikroskop. By Dr. Phil. A. Ehringhaus. Second edition. [Pp. vi + 156, with 83 figures.] (Leipzig and Berlin : B. G. Teubner, 1938. Price abroad, RM. 2.70.)

Dr. Ehringhaus has produced a concise treatment of the general principles and applications of the microscope. The two opening chapters deal with image formation by lens systems including lens aberrations, leading up to hand magnifiers. The compound instrument is dealt with at some length, including a tabulated description of the procedure involved in the correct setting-up of the microscope—a valuable asset. Methods for the determination of the optical constants of objectives and eyepieces are given, and resolving power tests are discussed. The generally known methods of illumination for transparent and opaque objects are quoted together with dark-ground illumination and the ultra-microscope. A section is devoted to the measurement of microscopic objects dealing with length, thickness, and angle measurement. The mounting of objects is also included. Brief treatment of the application of polarised light to the microscope, the use of ultra-violet light with the microscope, and the so-called electron microscope, is given; but these subjects are treated too briefly. In a book of this size it would have been helpful if a list of references had been given in connection with the last two named modern aspects of microscopy; and indeed this remark applies to all sections throughout the book. Apart from this defect, the book will be found useful to any serious student of microscopy.

B. K. JOHNSON.

The Mind of Primitive Man. By FRANZ BOAS. Second edition. [Pp. x + 285, with 3 figures.] (New York and London: Macmillan & Co., Ltd., 1938. 12*s.* 6*d.* net.)

IT is very much to be doubted if on the first appearance of this book some twenty-eight years ago (first edition 1911) any reviewer would have felt that the keynote of the whole thesis was the strong undercurrent of common sense. Many realised that the work of Franz Boas was original and daring: there were few prepared to see in his writings, regarded at the time as somewhat speculative, that background of common sense that seems so conspicuous to the reader of to-day. It is probable that when the main thesis was delivered as an address in 1895 the audience were impressed far more by its daring than by an orthodoxy that now seems so apparent. Herein lies a great tribute to Franz Boas: and it is a personal satisfaction to the reviewer that to-day he may dismiss as almost commonplace the views against which popular prejudice rebelled on their first presentation. In brief, Boas, in his original address, called in question the popular thesis of the fundamental importance of racial type and the doctrine of the connection between hypothetical racial type and the possession of definite mental or psychological attributes. In 1911 no political atmosphere lent any added importance to the multitude of facts adduced by Boas in support of his main thesis. But in 1939 there is a difference in atmosphere which Boas himself notes in his preface as "the subjugation of science to ignorant prejudice in the countries controlled by dictators." In these days when wholly false doctrines of "race" and of racial superiority are freely broadcast through the world, the reissue of this eminently sane book is peculiarly welcome. It is very much to be hoped that the doctrines of Franz Boas will gain wide currency in a world troubled by false teachings concerning the questions of race and racial attributes. It would be a good thing if his dictum that "a close connection between race and personality has never been established" could become the slogan of a newer propaganda.

F. W. J.

Ascaris: The Biologist's Story of Life. By Dr. RICHARD GOLDSCHMIDT. [Pp. viii + 312, with 78 figures.] (London: The English Universities Press, Ltd., 1938. 10*s.* 6*d.* net.)

THE author of this interesting book—a professor of Zoology of University of California—has accomplished successfully a most useful form of science primer which, when the demand for biological training is so urgent, will be a valuable help to secondary-school teachers and those who may be responsible for introducing the elements of biology to the young and uninstructed.

The book starts with the consideration of the simple parasitic intestinal worm—"Ascaris"—and proceeds by way of respiration, circulation, body fluids, muscle and nerve structure, sense organ, etc.—and after touching on

Metabolism and renal excretion concludes with chapters dealing with heredity, mendelism and development. This is an easily written book and the nursery style is persistently maintained, but it is probable that most readers will tire of the author's zeal and wish for a less-comprehensive treatment of the subject. A dissertation on hæmophilia seems to be unasked for—and "Metabolism" is surely a subject for more advanced students than those this little book purports to attract.

P. J.

Wild Country: A Highland Naturalist's Notes and Pictures. By F. Fraser Darling. [Pp. viii + 104, with 82 illustrations.] (Cambridge: at the University Press, 1938. 10*s.* 6*d.* net.)

The miniature camera has won a sure place as a means of presenting to the people aspects of nature which lie outside their experience, and Dr. Darling has made good use of his Leica in amassing the miscellany of 82 photographs in this slim book. I mention the photographs first because obviously they form the backbone round which the text has been built. Some show a real feeling for beauty, like the English willows, sea-pink, the young gulls on p. 19, and the "fairies of the lochan"—a weird picture, but all illustrate features of scenery or characteristics of birds and mammals to which the text makes apposite reference. Although the photographs came first in time, the text makes a very good second. It discusses subjects which range from the Scottish highlands and islands and their inhabitants, to the fallow deer of English parks, and always it is charming in the ease of its style. It has the further merit of adding to the description of characters and habits suggestions of the deeper interests of animal life, such as protective coloration, infant protection and mortality, defence reactions. An odd mistake is the statement (p. 78) that the upper jaw of the crocodile can move independently of the head.

J. R.

The Cambridge Region. Edited by H. C. Darby. [Pp. xiv + 234, with 58 figures.] (Cambridge: at the University Press, 1938. 6*s.* net.)

In 1904 when the British Association for the Advancement of Science came to Cambridge the *Natural History of Cambridgeshire* was published under the joint editorship of Marr and Shipley. This work served as text-book and reference-book on the Natural History of the county for many generations of undergraduates and others. Now, after some 34 years, the Association has again met in Cambridge and naturally a new handbook was needed for the meeting.

In Dr. H. C. Darby they found a most competent editor. The book more or less follows the classical lines of a British Association Handbook, but some sections have been enlarged upon more than others.

This is particularly the case in the more geographical aspects of the region, for, whilst the first 80 pages are devoted to the various aspects of the Geology, Botany and Zoology of the district, the remaining 150 pages deal with the History, Agriculture, Industries, Fen drainage, etc., from the earliest times up to the present day. In this respect it covers a much wider field than its predecessor.

The book has some 34 contributors, mostly members of the University and each an expert in the branch on which he writes; we thus have an up-to-date summary of each subject with all the latest research included.

There are numerous references to the more detailed papers on which the articles are based, so that further information on any point can easily be looked up in the original paper; some information and certain maps are published for the first time.

The work will remain the most accessible and most easily assimilated source of information on the region for many years to come.

The numerous maps are clear and the general printing is of that high standard which is expected in a book printed at the Cambridge University Press.

W. B. R. K.

The Science of Production Organisation. By E. H. Anderson, Ph.D., and G. T. Schwenning, Ph.D. [Pp. x + 282.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 17*s.* 6*d.* net.)

This book consists of an outline and description of the fundamentals underlying the process of organisation for effective production. The writers hold that there *is* a science of organisation and that the science is the product of evolution rather than of a single theory, having been evolved by aggregation, by combining and blending the ideas and practices of many people rather than by reasoning from the premises of a few.

From the point of view of the student of these problems a noteworthy feature of the work is the lengthy and verbatim quotations from the relevant authors. The terms used are clearly defined, the need for which is well brought out by a consideration of the varied meanings attached to such words as organisation, administration, management. The evolution of the theory of organisation from Adam Smith to Taylor and his followers is discussed; also the organisation of work, including a reasonably detailed account of the Gilbreth system, types of organisation and a consideration of the advantages and disadvantages of each, and finally the fundamental principles and laws underlying organisation.

Organisation problems are studied from the point of view of the work to be done and of the personnel who do it.

The goal of scientific organisation is expressed as the making out of a group of individuals, a perfect organisation for service that will operate with the maximum efficiency and harmony.

M. S.

Statistical Methods for Research Workers. By R. A. Fisher, Sc.D., F.R.S. Seventh edition. Biological Monographs and Manuals, No. V. [Pp. xvi + 356, with 12 figures.] (Edinburgh and London: Oliver & Boyd, 1938. 15*s.* net.)

This edition of the well-known work appears only two years after the issue of the previous edition, a statistical fact which is indicative both of the popularity of the book and of the increasing importance of statistical methods in scientific work.

The section dealing with the fitting of curved regression lines has been expanded. The method is that of successively finding the straight line equation, then that of the parabola, then that of the cubic, and so on. The practical aspect of the problem is, of course, that which is emphasised here, as elsewhere throughout the book.

A new section has been introduced. It relates to the discrimination of groups by means of multiple measurements. The problem is to find the best possible linear compound of a set of measurements which distinguishes between two groups. The necessary technique is given here with appropriate illustrations.

E. C. R.

An Orientation in Science. By C. W. Watkeys and associates. [Pp. x + 560, with 281 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 21*s*. net.)

In their preface the authors describe this book as "a survey of natural science as developed during the past ten years at the University of Rochester." It is really a small but comprehensive encyclopædia of scientific knowledge devised; after strict limitation of methods and technical detail, to supply the need rather of the lay reader than the student. It is not possible for a single reviewer to express an expert opinion on all the parts of so vast and various a field as that comprised in this volume, but after reading several sections it must be admitted that there will surely be few readers who can fail to derive both instruction and pleasure from these pages.

It is a drawback that the introduction of illustrations necessitates the use of surface paper which makes for a book too heavy to hold with comfort.

P. J.

An Approach to a Philosophy of Education. By T. H. Eaton, A.B., M.A., Ph.D. [Pp. x + 273.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 12*s*. 6*d*. net.)

In the United States it is customary to use the term philosophy of education where a British writer—through fear of seeming to claim too much—would prefer to speak of principles of education. It must, however, be admitted that the character as well as the competence of Dr. Eaton's treatment of his subject would justify its appearing anywhere under the loftier title. The book is divided into two parts: the first dealing mainly with the backgrounds of the aim of education, the second with the implications of the theories advanced in the first. Part I, after defining the concepts of "environment" and social behaviour, proceeds to examine the values inherent in human life studied as a biological, a psychological and a social phenomenon. From the biological point of view success is the master-idea; but, in considering the inner life, the dominant notion is experience. The elements that emerge in social life and its institutions are to be valued by reference to the same two notions; and Dr. Eaton's clear and penetrating analysis makes the chapter devoted to this part of his theme especially valuable. In applying his theories to education, Dr. Eaton acknowledges the guidance of the late Sir John Adams, taking from him the concept of "bi-polarity" and its implications. In the main, however, the applications follow directly from the arguments of Part I. Some are striking, perhaps even disturbing. For instance, the theory of biological success turns out to justify mass-production by the most hard-cased educational "go-getter," and a type of school in which the teacher is merely an artisan; but, in developing the values inherent in experience, utterly different methods are called for, and teachers become artists clothed with spiritual authority. How these contradictions are to be harmonised is not made clear, but they leave an extremely interesting and important problem for the reader to reflect upon. The same may be said of the proposals with regard to the correlation and integration of studies with which this suggestive book ends.

T. P. N.

Creative Teaching: Industrial Arts and Vocational Education. By F. T. Struck, Ph.D. [Pp. xxvi + 623, with 60 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1938. 17*s*. 6*d*. net.)

This is truly an imposing work, its six hundred pages being close-packed with an immense amount of information. The author is very fond of tabulating the findings of various investigators for the sake of better organising his condensed material. The documentation and references throughout are outstandingly complete within the limits of being rather exclusively American, and the numerous photographs are well chosen. The "practical suggestions for teaching," as well as questions for discussion by the student, and recommendations for supplemental reading which follow the chapters in the manner of American text-books, honour a custom which is highly useful.

The pursuit of thoroughness leads sometimes to slight naïveté, as where a long "rank-list of desirable teacher-traits" such as "breadth of interest," "self-control," etc., is described at a length of one paragraph each; and the same is done more briefly respecting "things for which the ideal teacher stands."

About a page and a half is devoted to the "sources of motives." In a work of four hundred times this length and of such general completeness of treatment, such a handling of so large a topic is not even comic relief and would much better have been omitted entirely, as it is not very relevant. In contrast to this, the twenty pages on "teaching pupils how to study," including useful hints on the psychology of the subject, constitute one of the author's best worked-out chapters; and it is supplemented by others on "the art of questioning," "visual sensory aids to instruction," etc.

The above facts constitute for the book an extremely broad approach to its topic, so realising Dr. Struck's expressed intention that it should appeal to students majoring in general education.

The work is, nevertheless, chiefly "written for teachers of industrial arts and vocational industrial education." Sometimes the text appears to wander away from this aim somewhat and to be more concerned with the general than with the specific field. Concrete applications are, happily, taken from the teaching of industrial processes, and so, even more especially, are most of the 25 tabulations and all of the collection of 60 splendid illustrations.

Pryns Hopkins.

Educating for Democracy. Edited by J. I. Cohen, M.A., and R. M. W. Travers, B.Sc. [Pp. xxx + 458, with 4 plates.] (London: Macmillan & Co., Ltd., 1939. 10*s*. 6*d*. net.)

This book is a collection of essays by various authors expressing their views on the relation of education and training of children, adolescents and adults to the present urgent necessity for preserving democracy through its difficult and stormy journey. Like all collections, the value of the individual essays makes variable appeal owing to the degree of expertness and experience of the author and to an inevitable selection bias.

On the whole, the editors have planned well and have written a most useful and interesting editorial introduction. Essays by Sir P. Hartog—"The teaching of the mother tongue and the formation of the citizen" and "The place of examination in the Social system"—are excellent and informative, and contrast with several others in which the authors suffer from a nervous addiction to adjectival redundance and a false modesty which avoids committal of any useful conclusion.

As in the preceding volume, *Human Affairs*, arranged by the same editors, there is a tendency now prevailing among young authors to hurry "Psychology" and "Social Science," both of them immature pursuits, into positions to which they are not entitled.

Wrapping ingenious theory in a smoke-cloud of words does not make a science.

P. J.

English for Students in Applied Sciences. By S. A. Harbarger, W. R. Dumble, W. H. Hildreth, and Bert Emsley. [Pp. xiv + 260, with 11 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1938. 12*s*. net.)

How far do teachers of science make any serious attempt to ensure that their students write reasonably clear and intelligent English? To the present reviewer (who is not a scientist, but who has made some contacts with science students through examining the essays of scholarship candidates), it has often seemed that they have been treated far too leniently in the matter of setting down their ideas. There can be no excuse for a man expressing himself in a slovenly fashion because his subject is scientific; indeed, if he has been trained to think accurately it is almost impossible that he should do so. One is not asking the science student to produce "literature," but only to set down his thoughts tidily and efficiently; and one can hardly help suspecting that if incompetent writing were frowned upon more severely by his teachers it would become far less common than it is.

The four authors of *English for Students in Applied Sciences* are aware of the student's needs, and have much sensible advice to offer him on the use of words, on making outlines, on writing reports. Unfortunately their own English is sometimes that of a committee; and the reader who opens their book at the Preface is not likely to feel tempted to proceed any further. "This text for beginning college composition courses deals with English for students in colleges of applied sciences." With those ambiguous words the text-book is launched upon the literary market. "Text" may be good American for "text-book." But what part of speech is "beginning"? It is only fair to add that when the book gets under way it improves on this standard of writing; and if allowance is made for a number of words like "subscript" which are not likely to be familiar to the English reader, it should offer some useful training in literary method to the science student who finds difficulty in ordering and expressing his ideas.

J. R. S.

The Subject Index to Periodicals, 1938. [Pp. xxix + 544 columns.] (London: The Library Association, 1939. £3 10*s*. net.)

The scientific worker who desires—as who does not?—to keep pace with the publications of others interested in the same field, has cause to bless those who make it their business to abstract, index, and arrange the vast flood of material which pours annually from the printing presses of the world. The diligent individual probably misses little to which reference is made in the recognised bibliographies of his subject, but he is not likely to examine regularly the many journals of a general nature, any of which may from time to time include an article of importance to his work. Just for this reason should his attention be drawn to the excellent service which the Library Association continues to render each year with a punctuality, comprehensiveness, and accuracy which deserve all praise. The Subject Index to Periodicals deliberately excludes reference to papers appearing in journals indexed by such well-known publications as *Engineering Abstracts, Index Medicus, Science Abstracts,* and the like indexing or abstracting periodicals of established authority, while devoting its attention to 580 periodicals covering a very wide range of subjects which are here arranged in a single alphabetical list. Quite general periodicals, like *Science,* specialised journals, such as *The Journal of the Royal Aeronautical Society,* and the publications of local societies, of which *The Proceedings of the Dorset Natural History Society* is an example, are all indexed with equal care by the energetic team over which Mr. T. Rowland Powel presides as General Editor.

J. W.

Cataloguing in Special Libraries: A Survey of Methods. By JOHN L. THORNTON, A.L.A. [Pp. xii + 268.] (London: Grafton & Co., 1938. 10*s*. 6*d*. net.)

THIS book is the result of an enquiry into the methods of cataloguing in use in special libraries. The term "special" is used in its broader sense to include all libraries other than public and county libraries.

The cataloguing of university libraries, medical and scientific libraries, the libraries of learned societies and institutions, government libraries, business, commercial and technical libraries, libraries of national importance, school and polytechnic libraries and other types of special libraries, is discussed. In every case typical examples of such libraries in the British Isles are chosen for illustration, and their methods of cataloguing and problems are discussed.

A chapter is devoted to the cataloguing of serial publications and another to the difficulties of alphabetisation.

Statistics are given concerning the cataloguing codes used, the forms and types of catalogue provided and the schemes of classification in use in different libraries.

In conclusion, the author deplores the lack of uniformity in cataloguing methods in this country, but points out that the requirements of readers using the different types of libraries vary greatly, and that their convenience should have the first consideration when choosing the cataloguing system to be used.

The book is primarily of interest to the trained librarian, and serves as a record of the methods of cataloguing in use in this country.

M. S.

BOOKS RECEIVED

(Publishers are requested to notify prices.)

Calculus. By Frederic H. Miller, Assistant Professor of Mathematics, Cooper Union Institute of Technology. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1939. (Pp. xiv + 419, with 118 figures.) 15*s.* net.

An Introduction to Modern Geometry. By Levi S. Shively, Ph.D., Associate Professor of Mathematics, Ball State Teachers College. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1939. (Pp. xii + 167, with 122 figures.) 10*s.* net.

Mathematical Recreations and Essays. By W. W. Rouse Ball, late Fellow of Trinity College, Cambridge. Revised by H. S. M. Coxeter, past Fellow of Trinity College, Cambridge. Eleventh edition. London: Macmillan & Co., Ltd., 1939. (Pp. xvi + 418, with 2 plates and numerous figures.) 10*s.* 6*d.* net.

Astronomy. A Textbook for University and College Students. By Robert H. Baker, Ph.D., Professor of Astronomy in the University of Illinois. Third edition—second printing. New York: D. van Nostrand Co., Inc.; London: Macmillan & Co., Ltd., 1939. (Pp. x + 527, with frontispiece and 293 figures.) 16*s.* net.

An Introduction to Astronomical Navigation. By P. H. Legg, B.Sc. London: Chapman & Hall, Ltd., 1939. (Pp. xiv + 102, with 67 figures.) 10*s.* 6*d.* net.

Introduction to Contemporary Physics. By Karl K. Darrow, Ph.D., Research Physicist, Bell Telephone Laboratories. Second edition. New York: D. van Nostrand Co., Inc.; London: Macmillan & Co., Ltd., 1939. (Pp. xii + 648, with 123 figures, including 1 plate.) 25*s.* net.

The Evolution of Physics. By Albert Einstein and Leopold Infeld. London: The Scientific Book Club, 1939. (Pp. x + 319, with 3 plates and 75 figures.) 2*s.* 6*d.* to members.

Elementary Survey of Physics. A Non-Mathematical Presentation with a Special Supplement for Pre-Medical Students. By Arthur E. Haas, Ph.D., Professor of Physics at the University of Notre Dame. With the collaboration of Ira M. Freeman, Ph.D., Associate Professor of Physics, Central College, Chicago. London: Constable & Co., Ltd., 1939. (Pp. xii + 203, with 75 figures.) 7*s.* 6*d.* net.

A Text-Book of Heat. Part I. By H. S. Allen, M.A., D.Sc., F.R.S., Professor of Natural Philosophy in the University of St. Andrews, and R. S. Maxwell, M.A., B.Sc. London: Macmillan & Co., Ltd., 1939. (Pp. x + 544, with 200 figures.) 10*s.*

Proceedings of the Sixth Summer Conference on Spectroscopy and its Application. Held at the Massachusetts Institute of Technology, Cambridge, Massachusetts, July 18–20, 1938. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1939. (Pp. viii + 172, with numerous figures.) 15*s.* net.

Theory and Practice of Electron Diffraction. By G. P. Thomson, M.A., F.R.S., Professor of Physics in the University of London at the Imperial College of Science and Technology, and W. Cochrane, M.A., B.Sc., Ph.D., Assistant Lecturer in Physics at the Imperial College of Science and Technology. London: Macmillan & Co., Ltd., 1939. (Pp. xii + 334, with 10 plates and 109 figures.) 18*s*. net.

Proceedings of the Fifth International Congress for Applied Mechanics. Held at Harvard University and the Massachusetts Institute of Technology, Cambridge, Massachusetts, September 12–16, 1938. Edited by J. P. den Hartog and H. Peters. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1939. (Pp. xxxii + 748, illustrated.) 30*s*. net.

Introductory Quantum Mechanics. By V. Rojansky, Ph.D., Professor of Physics, Union College, Schenectady, N.Y. London and Glasgow: Blackie & Son, Ltd., 1939. (Pp. xii + 544, with 112 figures.) 22*s*. 6*d*. net.

Elementary Mechanics, with Hydrostatics. By D. Humphrey, B.A., B.Sc., Director of Education, The Polytechnic, Regent Street, and E. A. Baggott, M.Sc., Ph.D., Senior Mathematical Master, Archbishop Tenison's Grammar School, London. London, New York, Toronto: Longmans, Green & Co., 1939. (Pp. xvi + 628, with frontispiece and 103 figures.) 8*s*.

Applied Aerodynamics. By Leonard Bairstow, C.B.E., F.R.S., D.Sc., Zaharoff Professor of Aviation, University of London, and Director of the School of Aeronautics, Imperial College of Science and Technology. Second edition. London, New York, Toronto: Longmans, Green & Co., 1939. (Pp. xviii + 808, with 27 plates and 416 figures.) 63*s*. net.

Elements of Practical Aerodynamics. By Bradley Jones, M.S., Professor of Aeronautics, University of Cincinnati. Second edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1939. (Pp. viii + 436, with 123 figures.) 18*s*. 6*d*. net.

Aircraft Design. Vol. II: Aerostructures. By C. H. Latimer Needham, M.Sc.(Eng.), B.Sc., F.R.Ae.S., M.I.Ae.E., Lecturer in Aeroplane Design, Battersea Polytechnic. London: Chapman & Hall, Ltd., 1939. (Pp. xiv + 308, with 160 figures, including 10 plates.) 16*s*. net.

Aeroplanes and Aero Engines. By Captain P. H. Sumner, late R.A.F. Third edition. London: The Technical Press, Ltd., 1939. (Pp. xvi + 285, with frontispiece and 236 figures.) 15*s*. net.

Building Design and Construction. With Reference to the New L.C.C. Regulations. Vol. I. By Dipl. Ing. Felix James Samuely, Lecturer on Advanced Design in Steel Construction at the Architectural Association, London, and Conrad Wilson Hamann. With a Foreword by H. Berry. London: Chapman & Hall, Ltd., 1939. (Pp. xii + 350, with 408 figures.) 25*s*. net.

Direct-Current Machinery. By Hempstead S. Bull, Assistant Professor of Electrical Engineering, University of Michigan. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1939. (Pp. vi + 318, with 193 figures.) 15*s*. net.

High-Frequency Alternating Currents. By Knox McIlwain and J. G. Brainerd, both of the Moore School of Electrical Engineering of the University of Pennsylvania. Second edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1939. (Pp. xvi + 530, with 226 figures.) 30*s*. net.

The Fundamental Theory of Arc Convertors. A Theoretical Study of the Principles Underlying the Design and Operation of Arc Convertor

Circuits. By H. Rissik, Hons. B.Sc., A.M.I.E.E., M.A.M.E.E. With a Foreword by C. L. Fortescue, O.B.E., M.A., M.Inst.C.E., M.I.E.E. Vol. VIII of a Series of Monographs on Electrical Engineering under the Editorship of H. P. Young. London: Chapman & Hall, Ltd., 1939. (Pp. xvi + 287, with 79 figures.) 18*s.* net.

Theory and Design of Valve Oscillators for Radio and Other Frequencies. By H. A. Thomas, D.Sc., M.I.E.E. With a Foreword by E. V. Appleton, M.A., D.Sc., LL.D., F.R.S. Vol. VII of a Series of Monographs on Electrical Engineering under the Editorship of H. P. Young. London: Chapman & Hall, Ltd., 1939. (Pp. xviii + 270, with 103 figures.) 18*s.* net.

Electrolytic Condensers. Their Properties, Design, and Practical Uses. By Philip R. Coursey, B.Sc., M.I.E.E., F.Inst.P., Technical Director, Dubilier Condenser Co., Ltd., London. Second edition. London: Chapman & Hall, Ltd., 1939. (Pp. xii + 190, with 112 figures, including 12 plates.) 10*s.* 6*d.* net.

A Practical Manual of Chemical Engineering. By Harold Tongue, M.I.Mech.E., Mem.A.S.M.E., A.M.I.Chem.E., Principal Assistant, The Chief Engineer's Dept., The London County Council. With a Foreword by Sir Gilbert Morgan, O.B.E., D.Sc., Sc.D., LL.D., F.R.S. London: Chapman & Hall, Ltd., 1939. (Pp. xvi + 560, with 305 figures, including 61 plates.) 36*s.* net.

Colour in Theory and Practice. Vol. I: General Theory. By H. D. Murray, M.A., F.I.C., and D. A. Spencer, D.I.C., Ph.D., Hon. F.R.P.S., F.I.C. London: Chapman & Hall, Ltd., 1939. (Pp. xvi + 176, with 3 coloured plates and 109 figures.) 25*s.* net.

Colour and Colours. By Matthew Luckiesh, D.Sc., D.E., Director, Lighting Research Laboratory, General Electric Company, Cleveland. New York: D. van Nostrand Co., Inc.; London: Chapman & Hall, Ltd., 1939. (Pp. xii + 205, with frontispiece.) 15*s.* net.

General Science for Schools (Part III). By F. Sherwood Taylor, Ph.D., M.A., B.Sc. London: William Heinemann, Ltd., 1939. (Pp. xii + 271, with 8 plates and 152 figures.) 3*s.*

Mellor's Modern Inorganic Chemistry. Revised and edited by G. D. Parkes, M.A., D.Phil., Fellow of Keble College, Oxford, in collaboration with J. W. Mellor, D.Sc. London, New York, Toronto: Longmans, Green & Co., 1939. (Pp. xx + 915, with 241 figures.) 12*s.* 6*d.*

Inorganic Chemistry. By F. A. Philbrick, M.A., Rugby School. London: G. Bell & Sons, Ltd., 1939. (Pp. viii + 396, with 181 figures.) 6*s.*

Factual Tests in Inorganic Chemistry. (Post School Certificate Standard.) By F. A. Philbrick, M.A., Rugby School. London: G. Bell & Sons, Ltd., 1939. (Pp. 80.) 1*s.* Answers issued separately 6*d.* net.

Inorganic Syntheses. Vol. I. Editor-in-Chief, Harold Simmons Booth, Western Reserve University, Cleveland, Ohio. New York and London: McGraw-Hill Publishing Co., Ltd., 1939. (Pp. xiv + 197, with 31 figures.) 18*s.* net.

An Introduction to the Principles of Physical Chemistry. By O. Maass, Macdonald Professor of Physical Chemistry, and E. W. R. Steacie, Associate Professor of Chemistry, McGill University. Second edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1939. (Pp. x + 395, with 102 figures.) 15*s.* net.

Luminescence. A General Discussion held by the Faraday Society, September, 1938. London: Gurney & Jackson, for the Society, 1939. (Pp. ii + 240, with 8 plates and numerous figures.) 12*s.* 6*d.* net.

The Chemistry of Organic Compounds. A Year's Course in Organic Chemistry. By James Bryant Conant, President of Harvard University. Revised with the assistance of Max Tishler, Ph.D., Research Chemist, Merck & Co. New York and London: Macmillan & Co., Ltd., 1939. (Pp. xii + 658, with 35 figures.) 18*s.* net.

A Concise Organic Chemistry. By N. F. Newbury, M.A., M.Sc., A.I.C., Chemistry Master, Oldershaw Grammar School, Wallasey. London: George G. Harrap & Co., Ltd., 1939. (Pp. 190.) 3*s.* 6*d.*

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SCIENCE PROGRESS

ATOMIC ENERGY[1]

By Sir CHARLES DARWIN, K.B.E., M.A., Sc.D., F.R.S.
Director of the National Physical Laboratory

The purpose of this paper is to give some account of the theories which have led, from stage to stage, to the discovery of means whereby it is possible to utilise atomic energy. Something will be said about the final stages, but the main purpose is not so much to discuss them as to use the whole story as an illustration of the development of a range of scientific thought. Most extending developments of thought borrow ideas from such a wide field that it is difficult to explain them without expanding out over an ever-widening field of knowledge; my present subject is rather more compact, so that it will be possible to present it as a whole, without having to branch out too widely into remote fields.

I would like to make a point of personal explanation before going further. It was my good fortune to be in Rutherford's laboratory in the earlier days of some of the work I am going to describe, so that I was in close touch with it; of all the later work that has been much less the case, and I can claim nothing but derived knowledge of its later phases, though I have been enabled to follow them fairly closely through the chance that I have had the privilege of serving as a member of one of the committees connected with the developments that led to the atomic bomb.

At the end of the nineteenth century physical theory had reached a rather definite stage, and seemed to many to have arrived at a certain completeness which left little for the future beyond an improvement in accuracy of measurement, and the dotting of a few i's and crossing of t's. The atomic theory of matter was quite firmly established—mainly from chemical evidence—but atoms were still regarded as rather hypothetical quantities, because no one had seen them, and it was felt that no one ever would. To illustrate this vagueness it may be mentioned that there was still much doubt concerning the size of the atoms or, to put the matter more precisely, as to how many atoms there are in a cubic centimetre of water. There had been some quite good guesses at this, but also some considerable divergencies, and if anyone had had grounds to put forward a value of this number, say double that of most of the previous ones, no one would have condemned it out of hand. To show the progress made since then, if someone now produces a value differing from the rest by more than one part in a thousand he will have hard work to convince his colleagues of its correctness.

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[1] Based on a lecture delivered to the Birmingham and Midland Institute, October 1945.

All this was changed in a few years by the discovery of radioactivity in 1896 by Becquerel and the Curies. The first thing this showed was that atoms, previously regarded as immutable, did in certain rare cases have the capacity of changing, so that a piece of the element uranium would slowly turn itself into lead. The property of radioactivity which led to its discovery was the capacity for fogging a photographic plate; this led to a study of what the radiations really were which were emitted, a matter worked out largely by Rutherford, and as it is the very heart of our subject I must spend a little time describing them. They fall into three quite different classes which he called α, β and γ. The α-rays are very intense, but have little penetrating power. They can only go through an inch or two of air, or a thin sheet of India paper, but in conditions when they are not blocked out by obstacles they produce effects much greater than the β- or γ-rays. The α-rays were shown to be helium atoms projected at a very high speed and carrying strong positive electric charges; they are the first examples known of atomic projectiles, of which we shall come across other types later. The β-rays are much less intense, but have much more penetrating power; a β-ray would go through perhaps a board a quarter of an inch thick. They are simply electrons, the very light particles with a negative charge which play such an important part in wireless valves, but many of them come out of the radium at a speed approaching that of light. The γ-rays are in fact extremely penetrating X-rays, and so are, physically speaking, the same thing as light.

In dealing with the transformations of the atom we shall continually be considering whether the change is connected with something of the γ or β or α type, that is with the emission of radiation, or of electrons, or of atomic particles, and we shall finally see that it was the discovery of yet a fourth type of behaviour that led to the new developments.

It was Rutherford more than anyone else who made these discoveries, and it was he who perceived that these projectiles would provide probes for finding out what had hitherto been a complete mystery, the structure of the atom itself. Here begins our actual tale. The α-particles were shot through thin films of gold foil and a study was made of what happened to them in the foil. The two things that can be studied are their loss of speed and their changes of direction or scattering. The loss of speed is the easiest to observe, but as it happens, for technical mathematical reasons, it is much harder to work out its theory and more definite information could in the first place be derived from the scattering. The experiment consisted in this. A narrow beam of α-particles passed through a gold foil and on the other side it fell on a suitable screen which showed a faint scintillation whenever an α-particle struck it. For the experimenter the work is very tedious since he has to adapt his eye to see the scintillations; and this means sitting in the dark for at least half an hour before commencing work, and remaining there till the experiment is over. If there were no gold foil in front of the screen scintillations would be seen at a place in the line of the narrow beam, but none outside, but when the foil is there a few are seen scattered a little outside the original line of the beam. Rutherford assumed that this scattering was due to the collisions of the α-particles with electrons, as it was already known that there were a good many of these in the atoms of all matter. The measurements then gave an answer to the question of how many such electrons there were, and the answer for gold was roughly a hundred in each atom. For foils of aluminium a similar experiment gave an answer of about ten or fifteen, and the general rule seemed to be that the number of electrons in any atom is something like half the atomic weight.

This was an important if not very accurate result, but it was not this itself that led to the really exciting consequences. In the course of their experiments the observers had naturally looked at their screen a good long way off the original beam to see whether any would be scattered so strongly, and they saw a few scintillations there too. There were not many of them, but there were far more than there should be if they were to be attributed to scattering by the electrons. Indeed, a few were thrown right back by the foil. Rutherford made the guess that it was not possible to regard these as due to a cumulative effect coming from a lot of electrons, but that there must be some other obstacle which would on rare occasions by a single act turn the path of an α-particle into quite a new direction. He made the hypothesis that each atom had a heavy nucleus, which was very small so that it was rarely hit by the α-particles, but when it was hit the α-particle would bounce off it. He worked out the theory; it was duly verified in all its details and gave the structure of the atom which has ever since been accepted as correct.

Here is the result. The atom of each chemical element is composed of a very small heavy nucleus carrying practically its whole mass and a large electric charge. The charge provides the force which pushes the α-particle off, and the mass the stiffness which prevents the α-particle from simply brushing the atom aside. It is the charge that determines the chemical character of the element, and measured in the appropriate units they run from 1 for hydrogen, 2 for helium, 6 for carbon and so on up to 92 for uranium. The corresponding weights, also measured in units of the weight of hydrogen, start at 1 for hydrogen, 4 for helium and end at 238 for uranium. The α-particle is itself simply the nucleus of a helium atom. It is these two characters, the atomic number (that is the electric charge) and the atomic weight, that will be our concern, but before passing on I ought perhaps to complete the picture by saying a word about the rest of the atom. The very strong positive charge of the nucleus attracts round it a very strong neutralising charge, but this has quite a different character, for it is made up of a cloud of electrons which carry negative charges, but which differ from the nucleus in being much lighter; in an atom only about a three-thousandth part of the mass is carried by the electrons. There is a great body of theory about the behaviour of these electrons which was first understood about twenty years ago. On the whole, the discovery of this theory, the quantum theory, marked an even more exciting advance in physics than the subject of this essay, but for the most part it is not needed for our present purposes. It will suffice to say that there is a thin cloud of negative electricity surrounding an intensely charged positive nucleus and cushioning it against the outside world, but that the protection is not strong enough to hold out a bare nucleus such as an α-particle.

The early work gave no indication of the size of the nucleus, since it could be regarded as a point, but anticipating later results it may be said that the diameter is about a ten-thousandth part of that of the whole atom, so that if you take the tip of your little finger for the nucleus the atom would be a hundred yards in diameter. As it takes about a million atoms in a row to make a length just visible to the naked eye, it will be realised that we are dealing with very small quantities.

The next puzzle about the nucleus was this. The study of the collisions clearly showed that the nucleus had a charge which was an exact multiple of the charge of the hydrogen nucleus—or *proton* as it came to be called—but chemical work definitely showed that for many of the elements the mass was not a multiple of the mass of the proton. There was a strong indication that it ought to be so, because in an unexpectedly large number of the elements the ratio did approach a whole number quite closely—certainly far oftener than the laws of probability would permit. It is not necessary here to describe all the various lines of argument that elucidated the problem. It was mainly solved by Dr. Aston, who showed that what had been regarded as single elements were really mixtures of substances of the same atomic number but of different atomic weights, *isotopes* as they are called. The weight of each isotope is a multiple of the weight of the proton, but their abundance is very various. For example, chlorine is the seventeenth element, but it has weight 35½ on the hydrogen scale. This is because it is a mixture of two substances of weights 35 and 37 of which the first is about three times as abundant. So we now say that chlorine, which has atomic number 17, has two isotopes 35 and 37.

There has naturally been a great deal of work done to find out what isotopes exist, and by now the field is fully explored, though there are probably a few which have been overlooked because there are not many atoms of their types. There are no exact rules to tell us how many isotopes there may be for any chemical element; some have only one, some have two, and some quite a lot. But in all elements the isotopes have weights not very different from one another; for example, tin, which has atomic number 50, has 10 isotopes ranging in weights between 112 and 124, and this element has more isotopes than any other. The general rule is that for the lighter elements the isotopes have atomic weights about twice the atomic number, but that for the heavier ones the weight is a good deal more. Thus the last element uranium is No. 92, and its principal isotope is 238 in weight, which is a good deal more than double 92. This is much the commonest type of uranium isotope, but there is another one of weight 235, which though not nearly so common is very important for our present purpose, and there is another of weight 234 which is rarer still.

Since ordinary chemical processes depend on atomic number and not on atomic weight the isotopes of an element are always found in the same relative abundance, and that is why their existence was never detected by any of the older methods. More recently, with the expenditure of much labour, in a few instances methods have been found for separating isotopes. This is easiest for hydrogen, which has an isotope of weight 2, known as heavy hydrogen, present in very small quantities. It is the easiest because here one isotope is proportionately so much heavier than the other that there are perceptible differences in their rates of evaporation and in similar phenomena. With the expenditure of much power, quite large quantities of heavy hydrogen have been separated, and by burning have been turned into heavy water, a substance of use in some of the matters to be discussed later. The isotopes of a few other elements, such as nitrogen, have also been separated with great difficulty, and as we shall see so also have the isotopes of uranium.

Though the weights of the various isotopes are unmistakably very near whole numbers on the hydrogen scale, they are not exactly so. Very important deductions can be made from this, because according to the theory of relativity, mass is essentially the same as energy, and so by finding the mass of an atom one can estimate the energy of its formation. For example, two hydrogen nuclei are perceptibly heavier than a nucleus of the heavy hydrogen isotope, and this tells us that if the two could be joined together energy would be given off. There is an attractive theory, which is now rather generally accepted, that the heat of the stars is derived by the gradual "burning" of hydrogen 1 into hydrogen 2 and then by a further reaction into helium 4. Of course this nuclear reaction is not to be confused with an ordinary chemical one; it only happens at temperatures of millions of degrees, and then very slowly by a sort of catalysis in which a nitrogen atom takes part. Similarly, though the language of chemistry will be used in describing other nuclear reactions, it must be understood that they are quite different from ordinary chemical processes.

By measuring then the masses of all the atoms we can tell their energies of formation, and can say which are the most stable by their having less mass and therefore less energy. With a view to finding out this the same method of weighing the atoms was used as had been used to discover the isotopes. But whereas in the first work, rough results would do, in that it was only necessary to recognise whether a number was, say, 30 or 31, now much more precise measurement was needed, because it was necessary to know whether the number which was unmistakably 31 was one-thousandth or two-thousandths of a unit below it. These measurements gave the general result that the elements in the middle of the table were the most stable, and that therefore energy could be won either by combining together the very light ones or else by splitting the heaviest ones. It is this last process that occurs in the atomic bomb.

For the next item in my history I must turn again to Rutherford. In his first work on the collisions of α-particles with the nucleus he had been content to study the scattering collisions where the α-particle did not go very close, that is to say it had bounced off in consequence of the general electric repulsive force. But he now began to look for more detail and saw that in some cases it might go so near the nucleus that if the nucleus itself had any structure it would be revealed. He had to work with light atoms, because their electric charge would be small so as to give him a chance of producing this close penetration. I have been talking rather light-heartedly as though one could shoot one atom at another. Of course this is only a symbolic way of putting it—the process is not like a rifle, but like a shot-gun, where there are so many pellets that a few will hit by the laws of chance. It was not very long before he discovered that penetration of a nitrogen nucleus by an α-particle caused the nucleus itself to break up giving off a proton in place of the α-particle that had entered it. In this way he achieved for the first time in history the artificial transmutation of the elements, for the remaining nucleus, having gained in atomic number 2 from the α-particle and lost 1 from the proton would end as oxygen. An intensive investigation of the new phenomenon showed that it occurred with other elements, and by the technical process known as Wilson's cloud chamber the event could actually be photographed. Later, when Cockcroft and Walton had developed a method of getting very high voltages, protons were used as the bombarding particles instead of α-particles. In this way the process was made completely artificial, whereas it might before have been protested that the process had only been attained with the use of the naturally occurring radium. Space will not permit further description of the great work in this field; it gave a good deal of knowledge of the forces that the nucleus exerts, but it was limited to the lighter atoms, because for the heavier ones the electric forces were so great that the projectile, whether an α-particle or a speeded-up proton, could never get close enough to the nucleus to show its structure.

It was this field of work that led to the next discovery which is of primary importance for our subject, the discovery of the *neutron*. In the various disintegrations that had been studied there had been emitted not only α-particles and protons, but β-particles and γ-rays, that is electrons and a very penetrating radiation. This radiation was much studied because it gave information about the energies involved in the various transformations, and in the course of the investigation a radiation was discovered which had a penetrating power much greater than anything observed before. This was first found by workers in both Germany and France. Following up the work Chadwick discovered that the extraordinary penetration was due to the "radiation" having an essentially different character from that originally supposed. It consisted in fact of neutrons, that is to say corpuscles like the α-rays but with the mass of the proton and no electric charge at all. Through having no charge it could penetrate easily through matter because it was indifferent to the electrons or nuclei it was passing, and was only affected when it actually met a nucleus on the direct line of its path. It was naturally exceedingly hard to detect for this reason—indeed, I remember shortly after its discovery taking part in a conversation where we considered whether we might not be completely surrounded by a thick cloud of neutrons passing through us in all directions all the time, but not affecting our own atoms in any way and therefore quite harmless. But neutrons are not like that. They go very easily through matter, but not for ever. The difference arising from the absence of electric charge is that whereas an α-particle can barely get through a thin sheet of paper, a neutron may go through inches of solid before it has even a single collision to upset its straight line path. The difference can be described by saying that whereas an α-particle or a proton cannot go through more than about an inch of air, a neutron may go about three hundred yards. The effect, moreover, could be made visible by sending a beam of neutrons—in every sense invisible—through a chamber containing water vapour. Here and there in it one of the neutrons would find a hydrogen nucleus exactly in its path, and would bat it on, giving up its own energy so that it came to rest, while the hydrogen nucleus would travel on at a high speed. This nucleus would not be immune in the same way as the neutron had been, but could, by means of a suitable mechanism, be made to show the track of its path.

There are quite a variety of methods by which neutrons can be obtained. One of the earliest consisted in mixing radium with the element beryllium, since an α-particle striking the beryllium nucleus makes it give off a neutron; this method is much used when only weak sources are required because it takes very little room and once set up goes on continuously. When larger sources are needed, atoms of heavy hydrogen are given a high speed and allowed to collide with atoms of the same substance, with copious emission. There are now many ways of producing high-speed neutrons and of studying their effects; for example, by means of the cyclotron.

The neutrons are free to enter any nucleus, and when one does enter it may stick and transform the atom. But it was soon discovered that the condition for sticking depended rather exactly on the neutron's speed; for example, there might be a narrow range of speeds for which it would stick, whereas either faster or slower ones might not be nearly so effective. The culminating point in this work was reached by Professor Fermi of Rome. He had the idea that if the neutrons could be made to travel quite slowly, they would stick much more easily. Now the way to slow neutrons down is to make them collide with atoms; they can often collide without sticking and then they share their energy, so that the neutron loses speed. It is easy to see that the most efficient atoms for this purpose are the lightest, and Fermi therefore projected his neutrons through a thick layer of paraffin wax or water since these substances contain a lot of hydrogen. Because they have been slowed down in water these neutrons are sometimes called, colloquially, wet neutrons; they are extremely efficient in entering the nuclei of almost all atoms. The result is that nearly every element can be transformed by them, and a whole new field of nuclear chemistry was opened up. We may consider the sort of thing that happens. Suppose that we expose a piece of copper to slow neutrons. Copper is element number 29, which is the charge on its nucleus. Normal copper has two isotopes, one of atomic weight 63 and the other 65. Consider an atom of the 65 isotope. If a neutron enters, it adds 1 to the weight, but has no effect on the charge. As the charge is unchanged it is still a copper atom, but it now has weight 66. This proves to be too big a weight for the charge, and the atom must therefore either lose weight or gain charge. In the present case the charge is increased by the emission of an electron from the nucleus. By this emission the nucleus has lost one negative charge, and therefore gained a positive one, and so its atomic number is now 30, while its weight is still 66. We therefore have obtained an isotope of zinc, which is element number 30. This is a normal or stable zinc isotope and therefore the process stops, but it might be—and in some other cases is—that the nucleus is still too heavy for its charge, and then it would emit another electron, at an appropriate speed and time, and turn into the next element. There are very many similar examples that could be given, and we shall later return to the important one of uranium, but for the present this will suffice.

The discovery of the neutron produced an important philosophic revolution in our ideas about the nucleus. Earlier there had been very great difficulties in understanding how the heavier atoms could be built. We knew about protons and electrons, and thought that there were no other elementary particles. Also, as it happens in those days, we knew little about heavy hydrogen. How then was a helium atom to be constructed out of protons and electrons, as it undoubtedly was ? Its weight is four, so it must have four protons, its charge two, so two of these protons must be neutralised by two electrons : that is to say there would be six particles in all. It seemed very unlikely indeed that anywhere in space there could be simultaneously a collision of six bodies with such speeds that they could stick together. With the discovery of heavy hydrogen with two protons and one electron the process became a good deal more credible, but it would still demand a collision of three bodies simultaneously, which would be quite a rare event.

All this is changed with the discovery of the neutron, if we regard this as one of the ultimate elements, for now we can imagine that a heavy hydrogen atom is formed by a suitable collision of one proton with one neutron, so that they stick together, and then again that a helium atom is formed by two heavy hydrogens sticking together. But the idea that has guided the theorists in working out all this goes much deeper and is more abstract. The idea is that in essence a neutron and a proton are two aspects of the same thing, and that the same particle—sometimes called a nucleon—can change to and fro between the two states. For example, in a nucleus of heavy hydrogen there is a proton and a neutron, but one could not say which is which. But when, as in the example of copper there would be too many neutrons in the nucleus, one of them changes into a proton, and as a sort of secondary, though absolutely necessary, effect, the change is regulated by the emission of an electron. There is certainly a great deal of deep theory behind this, not yet all completely understood, but for the present purpose it suffices to forget about the electrons. A nucleus is composed of the two primary particles, protons and neutrons. For a stable element they are roughly equal in numbers. If there are too many neutrons, one changes into a proton by the emission of an electron. If there are too few an electron may fall in to change a proton into a neutron, but sometimes a much more exciting thing happens. This is hardly part of our present story, but it should be mentioned—an electron is emitted which has opposite charge to the ordinary one ; sooner or later this positron, as it is called, meets an ordinary electron and the two annihilate one another, emitting in the process a very intense γ-ray.

Returning to the structure of the nucleus, it has been pointed out that in it there are about equal numbers of neutrons and protons. It is easy, by just looking at the table of atomic weights and numbers to verify how far this is true. There is first a notable exception in ordinary hydrogen with one proton and no neutrons at all, and it is this that suggests that there is available a large source of energy in the " burning " of hydrogen, first into heavy hydrogen and then into helium and heavier elements. For the general run of the lighter elements, the atomic weight is almost exactly twice the number, but for the heavier ones it becomes gradually larger. For example, aluminium is number 13, and has only one isotope 27, that is to say there are 13 protons and 14 neutrons, whereas iodine has weight 127, but number only 53, so that there are 74 neutrons against 53 protons. In the last element uranium, the isotope 238 has 92 protons and 146 neutrons, and so if a uranium atom were to be split in two, the resulting two nuclei would have a good many more neutrons than they could carry. This is one of the most important features of the phenomena which make the atomic bomb work.

We now come to the final stage of our history. In the course of the experiments which added neutrons to atoms, it was natural to include work on uranium. It was found that the atom would accept a neutron, and so become uranium 239 instead of 238. The new atom then at leisure emitted an electron, and so raised its number from 92 to 93, and then another electron and so came to number 94. After this further changes occur and it was at first thought that it turned into the still higher elements 95 and 96, but this proved to be wrong. The subject was revolutionised by Professor Hahn of Berlin who recognised among the products some nuclei of the much lighter element barium through certain radiations that are characteristic of it. Further examination showed that though the formation of 94—plutonium as it is now called—was established, the source of all the other radiations was quite different. They arose from the splitting of the uranium atom into two roughly equal parts. Moreover, it was established from a rather deeper theory that it was not only plutonium that gave this fission, but also the rare uranium isotope, of weight 235, that is mixed with the ordinary one to the extent of rather less than 1 per cent.

This emerged from a general theory of the nucleus which was developed by Niels Bohr of Copenhagen, the man who perhaps more than any other has contributed to all the revolutions of our ideas of fundamental physics during the past thirty years, and I propose to glance at his general theory of the nucleus, in particular of the heavy nuclei of the atoms of large atomic number. As we have seen, the nucleus is composed of a mass of nucleons, that is protons and neutrons, stuck very close together by forces about which frankly we do not know very much. But even without this knowledge we can say that in a nucleus there are strong attractive forces holding it together, and that these forces give effects that make it in many ways very like a drop of water. For example, it shows a surface tension which keeps it spherical and it can show a property roughly analogous to the evaporation of water.

Using this analogy, consider what happens when a high-speed neutron strikes the drop. A composite nucleus will be formed by the union, and this composite nucleus will have a good deal of extra energy which has to be got rid of. There are three competing ways of doing so. Firstly, it may emit the energy as radiation in the form of γ-rays which correspond to ordinary heat radiation from the water-drop; if this happens the result will be a stable atom of weight one more than the original atom. Secondly, it may cool off by evaporation, that is to say it may give off at leisure one or more neutrons, or perhaps protons or α-particles, in exchange for the one it has received, just as the water can cool by evaporating some of its molecules. Thirdly, the added neutron may set up a violent oscillation and put the "drop" so much out of shape that it breaks into two. In this sort of violent motion we shall not expect any exact division into equal parts, but rather that the two parts should occur in any proportions, though with a rather strong preference for being not very unequal.

These three alternative events may occur with any atom, but they are, so to speak, competitive, in that there are different chances of the compound atom getting rid of its energy in one or other of the three ways, and what happens will depend on the relative probabilities for the various alternatives. The chances are very different for the various types of atom. Excepting for uranium and one or two others of the heaviest atoms the chance of fission is so much less than the other two that it can be left out of account. Even in the case of ordinary uranium it is a small chance, which occurs only when the entering neutron has a rather high and accurately adjusted speed; it is much more likely that the neutron will stick and give an atom of weight 239. But the matter is quite different for the less common isotope of weight 235, for this has a good chance of catching neutrons of any speed, and when it does there is a good chance of fission. There is also a good chance of fission from the element plutonium when this is hit by a neutron.

What now are the conditions that can exploit the very great energy given by these fissions ? We may compare it to a much more ordinary thing, a fire, either a common fire or the very rapid combustion that we call an explosion. The necessary condition for starting a fire is that the combustion of one molecule should provide enough heat to set off its neighbours ; there has got to be a multiplying effect, and even gunpowder will not go off if it is spread so thin that the heat of the first grain is wasted before it heats up the next one. So here, too. A neutron may be enough to produce a fission of one atom, but that will not make a real explosion unless there is a multiplying effect, so that the atom starts a train which will cause the fission of a few of the others, which will themselves each again start off more of them in rapidly increasing numbers. Now when a 235 bursts, its two parts have too many neutrons to balance their protons, because the proportion of neutrons to protons is a good deal higher for the heavy elements than for the middle ones. So the two atoms have got too many neutrons—perhaps 8 or 10 too many—and must get rid of them. Perhaps some of them turn into protons, but there are so many that there is no time for them all to change, and therefore some, perhaps three or so, are thrown out and are therefore capable of attacking the other uranium atoms. The conditions are exactly right for multiplication, which if it is only slight will be like a fire, or if more violent will be an explosion.

The stage is set. If we can get enough atoms which will by their fission produce a multiplication of neutrons, we can make an explosion which because of the great energy and the rapidity of its propagation will far exceed any hitherto produced. From its tendency to keep the neutron without bursting the ordinary uranium 238 is not much direct help, and we are therefore forced into one of two alternatives ; one of them is to separate the 235, the other the slow process of producing plutonium and then bursting the plutonium from uranium 238. Both of these are technical problems of chemical and physical engineering which are probably the most difficult ever undertaken, but both have been successfully solved. Two different processes have been used to separate the 235. In one of them a gaseous compound of uranium has been produced, and the separation in effect depends on the fact that in the gas the average speed of the molecules containing 238 is a little less than that of the 235 ones. The other method depends on the mass spectrograph which separates the atoms by deflecting them in a magnetic field. In both these processes the real problem is one of quantity. It is comparatively speaking easy to get small quantities, such as a millionth of a gram, but pounds are needed and one simply cannot manufacture a hundred million of what is already a fairly complicated apparatus. In fact, the difference of quantity is so great that it almost becomes a difference of quality, but still it is substantially the same method, and I shall not describe it. Working on these lines very considerable quantities of 235 have been separated.

We have now got a stock of explosible material, and the question arises how it is to be set off, and still more pertinent is the question of why it does not go off as soon as it is made. The reason for this is the same as the reason why you can burn a piece of paper without setting fire to everything around it, that the heat gets away so quickly that no multiplication of the effect occurs. We must remember how very transparent matter is to neutrons. If then we take a small lump of 235 and start with a neutron, though one atom may burst, the resulting neutrons will usually go some distance before they hit another nucleus. If the lump is small most of them will have got outside it without hitting one, and they are then useless, and so the multiplication will fail and there will be no explosion. It takes a definite bulk of the material before it can go off. The trick of controlling the explosion is therefore to have two pieces, each of them just too small to explode, and then suddenly to bring them together. As soon as contact is made, if there is even a single neutron in the middle part—and there always will be—then an atom will burst there, and most of the neutrons from it are certain to hit another atom before they escape, and so the explosion can multiply. Even so it is a very wasteful process, because many of the neutrons produced among the bursting outer atoms of the bomb will escape, so that probably only about 10 per cent. of the bomb really explodes; but this is quite enough for all practical purposes. The crucial question in the early days was how big the critical size for explosion would be. It was certain that it was larger than ounces and likely that it was smaller than tons, but it made a big difference when it was known it would be pounds and not hundredweights, for if it had been hundredweights the total amounts would have been unattainable in practice.

The manufacture of plutonium requires an entirely different procedure. Here it is necessary to slow down the process of multiplication so as to keep only a small excess multiplication of neutrons which will steadily convert the isotope 238 into plutonium. Large quantities of heat are produced and have to be dissipated, for if they were not the machine would continually rise in temperature till it got out of control. The components of the machine would get red hot and melt, and the whole would be destroyed, though whether it should be called a fire or an explosion I do not know; anyhow, if it is an explosion it would be of the tame type associated with such things as dynamite which is no use to anybody. In this connection I may explain one of the events in the history of the war, the commando raids on the Norwegian power station, Norsk Hydro. As I have just said, to produce plutonium it is necessary to damp down the fire, and there are not many suitable damping materials. One of the most effective is heavy water, and as there is no other very important use for it, the fact that the Germans were demanding it from Norway showed that they were at work on the same subject. Now at all stages of the war we were much further on than they were. Also we had practically the whole existing stock of heavy water in the world, though it was not a great deal; it had been got out of France at the last possible moment. It seemed worth while to prevent the enemy carrying their experiments to a further stage, even though it would not have been a very advanced one. So the whole dramatic story came from the determination to prevent the enemy from climbing on to a step of the ladder that we had already passed some time before.

The other, and much the most important, feature about plutonium is that all along it has received close consideration in connection with the benevolent side of atomic energy, the production of power for industrial purposes. For the military purpose the heat which had to be dissipated was a by-product in the production of the plutonium, but we look forward to the time when the heat is the important thing. It will be a major problem of practical engineering to turn this heat into power, calling as it will not only for the solution of all the practical problems of heat-resisting materials and so on, but also for the additional complication that in view of the intense radiations emitted by the furnace everything will have to be worked by distant controls. To return to the bomb, plutonium is a chemical element quite different from uranium, so that it can be separated out by more or less ordinary chemical processes, a fundamentally easier problem than the laborious separation of isotopes. Once obtained, the technique of making the explosion is very much the same for both.

As you know it was decided four years ago that the material should be produced in America. A large staff from this country went there to help, but we must give full credit to the ability and enterprise of American scientists and engineers for bringing into fruition in such a short time what must be easily the most difficult problem of chemical engineering ever undertaken. At the end of July, 1945, everything was ready, and the trial explosion was made in New Mexico, with complete success. In any scientific project there are so many factors to be considered, that there is always a little doubt that one of them, judged unimportant, may not prove to be capable of spoiling the prediction. In the present case naturally every conceivable difficulty of this kind was examined with the closest attention, but there might still lurk a doubt. At the end one could say that the chances were immensely favourable, but still it was not a matter of certainty. The project cost something like £500,000,000, and I suppose this is easily the largest bet ever made—and won.

My purpose has been to describe the broad principles governing this development of which we have only seen the beginning, but the matter cannot rest there, and though I have no more right to express opinions than the next man, I cannot leave the subject without touching on its general implications. In the first place, there is no going back, and even if it were possible to back-pedal on scientific development, to do so would entail infinitely worse consequences for humanity than the most dreadful things we may fear. After all, quite apart from atomic bombs, the destructive power of humanity has increased considerably of late, but it has not increased nearly as rapidly as the creative and the recuperative power. Even in warfare we build now faster than we destroy. We can knock down a bridge in half the time required a century ago, but we can build one, or for the matter of that a harbour, in a tenth of the time, even in the face of the enemy. And we can keep in good health in the field armies which a century ago would have all died of starvation even before they had had time to die of the inevitable epidemics. So I maintain we are better off now in the balance between creation and destruction than ever before, even in war-time; how much more in peace.

Accepting then the existing state of affairs, let me try and draw up a balance sheet of how we stand with regard to atomic energy. On the debit side we have the obvious military menace. It is a weapon of quite a new kind, because unlike other weapons it is impossible to use it on the small scale; as we have seen a small bomb cannot explode. This means the weapon cannot easily be used for police purposes, because it is hard to imagine a state of affairs when the authorities would decide on such violence as a purely preventive measure. Moreover, it is rather hard to see how it could be used against an army in the field because of the dispersal of the troops, the necessity of having one's own troops well away at the time of the explosion, and the uselessness of occupying a piece of devastated country. Its natural use is against the large towns and seaports, and to judge by the conduct of the gangsters who started this war it would most likely be used treacherously to start a war. For the present the process of manufacture is so elaborate that there is little danger of its being concealed, and this will probably be so for some time, but we cannot feel sure that it will always be so, and the problem of a world-wide organisation of inspection to prevent it seems very difficult. Moreover, there is a special danger that will be very hard to avoid. Every power-station developing atomic energy will produce plutonium as a by-product—not very much of it, but there it will be—and so if we are to get any benefit from atomic energy we cannot help accumulating explosive material.

On the credit side we must put the immediate enormous saving of life that has been achieved. Japan was already inevitably beaten, but it would have cost our armies perhaps a million more lives to finish it off. And I would defend the view that we have saved a very large number of Japanese lives also by convincing them so much more quickly that resistance was useless. Then there is the future development of power from atomic energy, not very far off. It is not easy to evaluate how important to the world a large power-station really is, but certainly it is at present one of the essentials of civilisation. We can now feel free of the threat of the consequence of the exhaustion of our coal resources for at any rate centuries to come. It may appear to you comparatively trivial, but I must add a third item to the credit side which is of great present interest to the scientist, which will undoubtedly produce benefit to humanity in due course. By the use of neutrons it is possible, so to speak, to label atoms so that they are later recognisable through their radioactivity. These are a very powerful new weapon in all sorts of research; for example, in biological research it is easy to watch the progress of atoms through the various tissues of the body so as to discover much more fully than ever before the chemical reactions that go to constitute life.

There is the balance sheet, a great menace and a great opportunity. We have got to accept it as an accomplished fact. It is for all of us to consider the new situation, and to show how to deal with it. Let us hope that mankind can rise to the level of the new responsibility.

THE VARIATION IN REPRODUCTIVE CAPACITY OF PLANTS

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THE normal rate at which a plant or animal reproduces its kind is perhaps as much a characteristic of the species as most of those we recognise. Like all characteristics it is subject to variation and more so than many others. Because the range in number of potential offspring is often wide, the fact that their total fluctuates about a mode that is specifically characteristic is apt to be overlooked, although extreme examples emphasise the remarkable differences between organisms in this respect. The small number of offspring and slow breeding rate of the Elephant, which was estimated by Darwin to produce offspring at the potential rate of 1 in 10 years after the thirtieth year, is one extreme, whilst the potential offspring of a Codfish has been estimated as 9 millions. The vegetable counterpart of the elephant is the Double Coconut, which does not fruit till its thirtieth year, and can subsequently produce 4 to 5 fruits that take 10 years to develop. At the other end of the scale a large specimen of the Giant Puffball has been estimated to produce some 7 billion spores and a large Vanilla Orchid can give rise to many million seeds in a single season. Between such extremes there is every gradation, and the observant naturalist naturally asks himself why species differ so greatly from one another in the size of the potential families which ensure their continued survival. The facile teleologists of the last century found a plausible answer to this question based on *a priori* reasoning, almost wholly unsupported, however, by data the collection of which is both arduous and time consuming. But, to do our predecessors justice, it is doubtful if this line of enquiry could have been profitably pursued until the study of variability and its statistical examination had passed into its modern phase. The general attitude of mind so far as concerned plants was that the capacity to produce offspring varied so much from individual to individual that no useful purpose could be served by its examination. Nevertheless, the rather contradictory assumption was made that the magnitude of the capacity to produce offspring was an insurance premium against the inevitable risks of mortality. If this were true, then clearly the normal seed output would be a measure of the risks of non-survival and a biological integration of the highest value, capable of statistical definition.

That no organism could survive where rate of multiplication was inadequate to meet normal risks is obvious, and that the two are closely related might appear from the fact that species, for the most part, occur with similar abundance so long as the conditions in which they grow remain more or less unaltered. If the average number of offspring is increased it is manifest that the risks of mortality increase also. If the view held by Herbert Spencer, and accepted by most others since, be true, the size of the family and the risks are proportional, but it is evident that increased size of family may increase the chance of survival more than it increases the risks of mortality which such an augmented family entails, although the law of diminishing returns will inevitably obtain. What the answer to that question is cannot be settled by *a priori* reasoning, but only by the accumulation of as precise data as circumstances permit. The writer of this article has over a period of years amassed information concerning the seed production of a considerable number of wild British plants, sufficient both in variety and magnitude to permit of formulating certain generalisations (*cf. The Reproductive Capacity of Plants*, Bell & Sons, London, 1943).

The seeming stability of populations of particular species has been referred to above, but this is largely illusory, since the impression of stability is often due to lack of observation and unconscious averaging over long periods which masks the fact that though particular kinds of plants do not commonly exhibit a steady increase or decrease, and so become either ubiquitous or extinct, yet in reality they often show striking fluctuations, particularly in relation to environmental variations. The abundance which has sometimes been observed of the Helleborine (*Epipactis ensifolia*) and the Shepherd's Rod (*Dipsacus pilosus*) can be related to the cutting of the undergrowth of the woods where these species normally occur, but the amazing abundance sometimes presented by the Green-winged Orchis (*Orchis morio*), followed by great scarcity or complete absence, is a phenomenon which does not accompany any obvious alteration in the environment, though it may well be that some subtle change, such as perhaps the exhaustion of a particular nutrient source or some trace element in readily available form, is in fact involved. The abundance of the Rosebay Willowherb and the Oxford Ragwort on the bombed sites of our cities is as much a tribute to the efficient dispersal of their seeds and fruits as to the profusion of their production but it is also a witness to the fact that the bombing has changed sterile areas of bricks and mortar and stone paving into places where plants can grow. Bracken too can be seen in many of the exposed cellars. So that one can visualise the countless millions of spores, fruits and seeds that have been shed from the air for centuries only to perish until the blitz rendered London habitable to them.

One important generalisation that emerges from the examination of a number of species is that it is amongst those which are characteristic of habitats which afford the opportunity of rapid colonisation that the large seed output is most obviously developed. This places high rates of reproduction in quite a different light to that imagined by Herbert Spencer. Far from being merely an insurance premium against extinction, the large output enables such species to take advantage of the opportunities that intermittently accrue for extensive colonisation.

A good example is furnished by the Canadian Fleabane, so abundant on many bombed sites and, in its native country, and elsewhere, characteristic of waste ground generally whether created by human agency or nature. Its tiny flower-heads make up in number what they lack in size. Examination of 50 fruiting heads yielded a range of from 16 to 72 fruits per head, with an average of 45 ± 1·2. A random sample of 21 plants from a bombed area was examined for the number of fruiting capitula they produced. These ranged in number from 121 on the smallest plant to 7175 on the largest, the average number being 1153. Thus the average production of fruits from a single plant would be well over 50,000, whilst the largest plant examined must have produced some 322,000 fruits. Each fruit is furnished with a parachute of hairs and takes rather more than 15 seconds to fall 10 feet in still air. So that the species is admirably equipped to colonise any bare area that may become available. It is of interest to compare the Canadian species with our native member of the same genus, the Fleabane (*Erigeron acre*) of our chalk downs. This latter is a plant of short turf and dry places, which, though it may colonise waste ground, is naturally a plant of situations much more clothed with vegetation where the opportunities for colonisation are much fewer and more restricted than in the habitats occupied by the alien species just considered. The average output for *E. acre* was found to be 2147 ± 182 fruits per plant (*cf. Reproductive Capacity* *of Plants*, p. 153). In other words, the average output of the Canadian Fleabane is more than 23 times that of its relative of more closed habitats.

In the work cited, data are furnished for a number of plants from another type of intermittent habitat, namely drying mud on which all the most common and characteristic species possess the capacity of producing large numbers of seeds. An additional example is the Mud Speedwell *Veronica Anagallis.* Its capsules contain an average of 61 ± 2·1 seeds, and a normal-sized plant will bear about 3000 capsules so that the seed production would be about 183,000. What a contrast this offers to such a species as *Veronica officinalis,* usually associated with situations more or less occupied by other competing plants, the output of which is under 9000 seeds a year.

But, if it is particularly the opportunist species which are most prolific, it must not be thought that all kinds of plants, which grow in situations where opportunities for colonisation frequently recur, have developed the capacity to take advantage of this fact. A few species of exposed mud, for example, are anything but prolific, and it is noteworthy that though normally rare, the relatively small number of their potential offspring has not led to their extinction although in fact the risks of mortality are greater than for the more prolific types, since the conditions these species demand are more restricted. Two examples shall suffice to illustrate this fact, which even alone would be enough to make us doubt the generalisation of Herbert Spencer. The Star Fruit, *Damasonium stellatum,* has an average output of only 231 ± 15 seeds (*loc. cit.*, p. 199), and, though an extremely rare species of drying mud, nevertheless reappears, often at long intervals, amounting, it may be, to half a century, when the mud is again exposed. Our second mud species is likewise a rarity. It is the composite, *Pulicaria vulgaris.* Ninety-eight plants examined had an average of 5·76 fruiting heads, each producing an average of 129 fruits. So that the seed production may be estimated as under 750, a figure that though high for so small a plant is ridiculously small in comparison with the common species of the same environment. The seeds for the latter germinate freely, and their seedling mortality, apart from competition amongst themselves, is low, whereas the seeds of the rare species cited often exhibit poor germination and are liable to high seedling mortality, so that if the number of potential offspring were in any appreciable degree related to the risk of mortality, it is these and not the common species which should produce high seed outputs.

Another interesting comparison can be furnished in this connection by the two common British Saxifrages. The Wall Saxifrage (*S. tridactylites*) and the Meadow Saxifrage (*S. granulata*). The former is much the smaller plant associated with open habitats, where competition is often slight and therefore the risks of mortality likely to be low. The meadow saxifrage grows in damp grass meadows where competition for space is severe and seedling mortality likely to be high. A total of 115 plants of the Meadow Saxifrage which were examined bore from 1 to 23 fruits with an average of 6·1 ± 0·24 capsules per plant. Each capsule contained from 99 to 478 seeds with an average of 296 ± 21. Thus the average production of seeds per plant is between 1600 and 2000. On the basis of mortality risks we should expect the output of seeds of the Wall Saxifrage to be much lower whilst on the basis of opportunism we shall not be surprised if it had evolved the capacity for rapid colonisation in which event the output might be expected to be higher. Actually the average seed production is 1106 ± 98. The output of *S. granulata* is thus absolutely higher, but in fact having regard to the relative size of the two species and their respective assimilating areas it can be said that the output of *S. tridactylites* is relatively the higher.

The capacity of a plant to produce seeds will clearly depend on the surplus food material available for the purpose, and thus in large part upon the size of the leaf surface by which the food is manufactured. So a valid comparison can only be made between species of comparable size. Bearing this in mind it is true to say that the largest potential progenies are usually to be found amongst the weeds of waste ground, the plants of woodland clearings or those of recently exposed mud. At the other extreme we have the plants of more or less "closed" types of vegetation such as thickets and woodlands occupying ground that is carpeted with vegetation. The seedlings of such plants have to endure competition, which is sometimes severe, and frequently must grow for some time at the expense of food stored in the seed before they can sufficiently emerge out of the shade of their neighbours to obtain adequate light to enable them to manufacture food more rapidly than they utilise it. The plants of such habitats normally have small outputs of seed, but the individual seeds are large. Such then we can regard as having become specialised to a greater or smaller degree in two respects, both of which have obvious "survival value" and which are complementary in character. Such "closed" habitats offer few opportunities for extensive colonisation so that we can appreciate a small output will suffice, but this in turn permits of the larger provision in respect to each that is requisite to support the seedling in a shaded habitat. Thus we find that in general the woodland species has a large and heavy seed or fruit, whilst its open habitat relative may possess a light one. We say 'may,' because on sand-dunes and shingle beaches the large seed may also have a high survival value, and here too, as in the Sea Convolvulus, we can find number sacrificed to weight.

Whilst a lavish provision for offspring is obviously advantageous as giving them a good start in life, such specialisation brings with it a handicap in dispersal to the point where reproduction by seed, apart from possible variation, has little merit and certain disadvantages compared with vegetative propagation. It is, in fact, true that vegetative modes of propagation are especially characteristic of the herbaceous woodland species sometimes even to the exclusion of reproduction by seed. One striking exception must, however, be noted, namely, the parasitic species and many mycorrhizal plants where the special modes of nutrition circumvent the normal disabilities consequent upon minute seeds: disabilities which are of considerable significance, unless the species be associated with open habitats.

All gradations are encountered from species with minute and numerous seeds to those producing few of large size, and if groups of species be considered, not individuals, the size of the seed tends to increase with the density of the plant community which is the normal habitat of the species. However, within a particular type of habitat, there is considerable range of seed size but even more of seed output, and the positive value of the latter is attested by the fact that the species which produce the largest number of seeds are usually the commonest. If, for instance, we consider the plants characteristic of coppiced areas in woodlands, Foxgloves and Mulleins are both widespread and abundant, the former especially on the more acid and the latter on the more basic types of soil. The Figwort and Wood Groundsel are both less frequent and occur in smaller numbers, whilst the Wood Cudweed and square-stemmed Willowherb are both much less frequent. All the plants mentioned agree in having light seeds or fruits. The following table shows their seed outputs, roughly, to parallel their frequency:

TABLE I

Species.	Approximate Average Output of Seeds per Plant.
Mullein (*Verbascum Thapsus*)	136,000
Foxglove (*Digitalis purpurea*)	85,000
Figwort (*Scrophularia nodosa*)	45,000
Wood Groundsel (*Senecio sylvaticus*)	20,000
Wood Cudweed (*Gnaphalium sylvaticum*)	9,000
Willowherb (*Epilobium tetragonum*)	7,000

If we examine a large population of individuals of any one kind of plant and collect them in groups according to the number of fruits that each individual bears we should expect to find that a small proportion are starved plants bearing very few fruits and that also there will be a few luxuriant specimens bearing an exceptionally large number. That as the number of fruits characterising each group of plants approaches the mean for the species the number of individuals in the group might be expected to increase in a similar degree whether the number of fruits for the group be above that characteristic of the species or below it. Actually an approximation to such a normal variation curve is sometimes realised as, for example, in the number of fruits produced by plants of the Cuckoo Pint (*Arum maculatum*). An examination of more than 300 plants (*loc. cit.*, p. 176) showed that 23 fruits was the most frequent condition representing about 6 per cent. of the entire population whilst approximately 50 per cent. bore more than the modal number, some as many as 58 fruits, and 44 per cent. of the plants produced

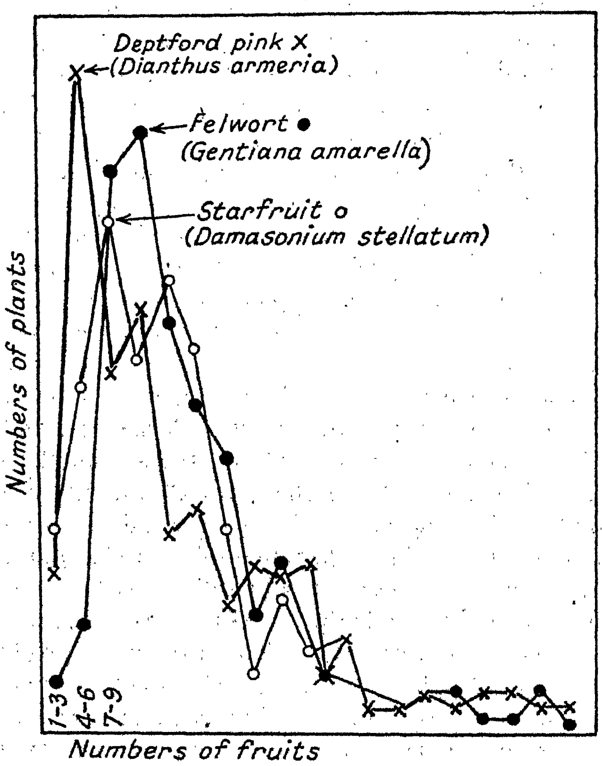

FIG. 1.—Variation in number of fruits showing asymmetrical "curve." Group classes differing by three units.

from 5 to 22 fruits. Actually the variation curve for this species was almost symmetrical and the average number of fruits per plant for the entire population was nearly the same as the " mode," namely 24 instead of the modal 23.

Far more frequently however the " curve " is asymmetrical in form, the number of individuals and range of variation above the mode being much greater than below it. Three examples are shown in Fig. 1. They represent data from wild populations of the Felwort (*Gentiana amarella*) a common plant of chalk downs, the Starfruit (*Damasonium stellatum*), the rare species of exposed mud referred to above and the Deptford Pink (*Dianthus armeria*), a plant of dry banks and open scrub. They thus exemplify diverse habitats. In general form the curves are very similar with a relatively small proportion of individuals which bear less fruits than the mode but an appreciable number of plants very much in excess of the mode. As a consequence, the *average* fruit production for plants of these species is very different from the mode. Thus for the Starwort there are more plants bearing between 7 and 15 fruits than any other number, but the average for all the plants examined was 19.

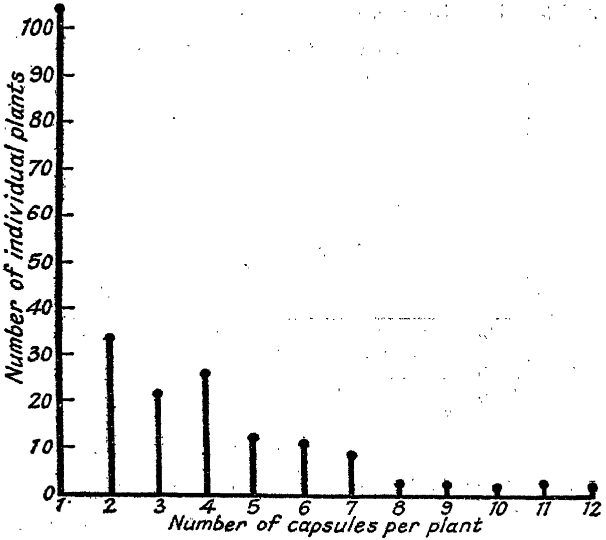

FIG. 2.—Variation in capsule number on plants of *Mœnchia erecta*.

For the Felwort the mode is 9 capsules per plant, but the average is 17; whilst more plants of the Deptford Pink bear 6 capsules than any other number, whereas the average is 23 capsules per plant. An extreme example is furnished by the little heath plant *Mœnchia* *erecta* (Fig. 2), where more than 40 per cent. of the population of individuals only produce a single capsule although the average capsule production is 2·8.

All the fruits of these plants do not contain the same number of seeds but the variation in this respect is largely independent of the number of fruits which can be taken as an approximation of the seed output, except for the very small depauperate plants. If the actual seed numbers could have been determined for each plant the asymmetry of the curves would tend to have been accentuated rather than diminished. The Starfruit, however, is peculiar in that the fruits almost invariably consist of six compartments, containing two seeds each, so that the " curve " for actual seed numbers would have been identical in form with that illustrated.

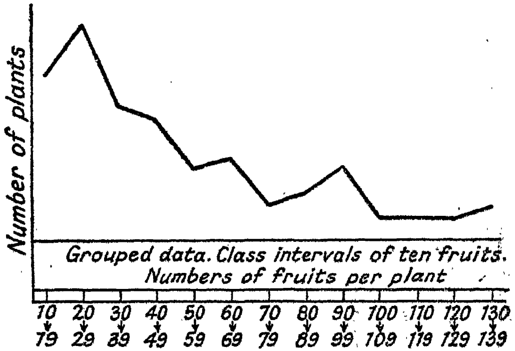

FIG. 3.—*Circæa lutetiana.*

When the mode of fruit production is low it is obvious that the possibility of variation in the direction of lower seed production is very limited whereas there is no such definite restriction in the upward direction, except in so far as the physiological make-up of the species sets such a limit. Whilst this factor must manifestly operate in some degree it is far from being the major cause of such asymmetry. For if this were so and we could regard the variation of fruit and seed production as conforming to a normal form, subject to curtailment in the downward direction, then the higher the mode of fruit production the more symmetrical the form of the " curve " should be. But though it is true that approximations to the symmetrical form of variation " curve " are to be found where the mode has a larger value the asymmetrical form is sometimes very pronounced when the modal value is high. This is well exemplified by the " curve " for the Enchanter's Nightshade (*Circæa lutetiana*) illustrated in Fig. 3. The indehiscent fruit of this species contains two seeds with considerable constancy so that fruit production and seed output are parallel. For convenience of representation the plants have been grouped into classes, the individuals in each of which have a range of fruit production of not more than 10, this, however, does not affect the general form of the "curve" which is markedly asymmetrical despite the fact that the mode is situated between 20 and 29 fruits.

The explanation of asymmetry is probably to be found mainly in the competition factor. The writer has shown (*loc. cit.*, Chap. VI) that fruit and seed production is very strongly influenced by the competition, both of individuals of the same or of other species. Thus with species subject to such competition there will be a preponderance of individuals with low fruit production and low seed output, but others which by reason of circumstances, such as earlier germination or more favourable situation, are better able than their neighbours to develop their potentialities. Owing to the manner in which plants obtain and manufacture their food any slight initial advantage to a particular individual not only leads to a progressive increase of size but automatically tends towards the suppression of its neighbours so that the small advantage is cumulative and

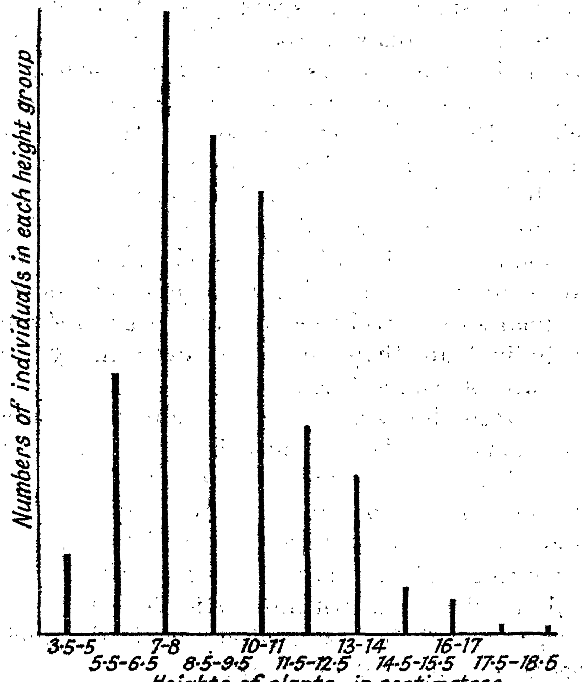

FIG. 4.—Variation in height of plants of Saltwort (*Salicornia annua*).

leads to large ultimate differences. For this reason, doubtless, populations of social species often show such asymmetry in a striking degree. The pure stands of Saltwort (*Salicornia annua*) which grow on mud flats show the same type of variation if we estimate the relative vigour of the individual plants by measuring their height as presented, for a random sample of 250 plants, in Fig. 4. Reference has already been made to the nearly symmetrical variation " curve " exhibited by the fruit production of the Cuckoo Pint. The circumstances of this species are somewhat peculiar. The individuals usually occur sparsely scattered in groups under hedgerows or in woodlands, their chief competitors being not individuals of the same species but the social plants of the vernal woodland flora. Since however these are mostly shallow-rooted types, such as Dog's Mercury and Wood Anemones whereas the Cuckoo Pint has its corm and root system at an appreciably lower level, it is obvious that the competition factor here plays a much more minor rôle. Since we find that even quite small depauperate specimens of annual species will often produce a single fruit, whereas depauperate perennial plants often die before they flower, it follows that the annual species will tend to show a greater preponderance of smaller individuals than perennials even though subjected to similar pressure of competition.

The more recondite aspects of reproductive capacity involve complex relations of balanced nutrition, the intensity and mode of incidence of radiant energy, and the correlations between seed production on the one hand and vegetative propagation on the other. Such factors are fundamental to a comprehension of the broader aspects of distribution and may play a major rôle as between one habitat and another, but within particular surroundings simple observations, provided they be accurate, could do much to enlarge our knowledge in this field of study.

PHYSICAL STATES OF AGGREGATION

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Two opposing factors govern all physical or chemical equilibria, one relating to *energy* and the other to *statistical probability.* The latter factor has its origin in the molecular structure of matter, and because systems ordinarily examined consist of vast numbers of molecules. The thermodynamic expression is :

$$-\Delta F = T\Delta S - \Delta U \doteq \Delta Q - \Delta U$$

where ΔF = change of free energy when a system changes from one state to another, T = absolute temperature, ΔS = change of entropy, ΔU = potential energy change, and ΔQ = heat change. When ΔF has a negative value the relevant change in the system occurs *spontaneously* ; if it is positive the change is reversed, and if it is zero the two forms of the system can exist side by side with no tendency to change over, as ice and water at 0° C.

The molecular picture of the two factors $T\Delta S$ and ΔU is as follows. Imagine a system consisting of a great number of molecules, all attracting each other. There will be one particular way in which all the molecules can settle down so that their mutual attractions are satisfied to the greatest degree, and this must be an *ordered* arrangement, like a three-dimensional wall-paper pattern or a pile of cannon balls. ΔU represents the energy change due to mutual attraction and associated with the passage from a *disordered* to the *ordered* state. The tendency to assume this static state, however, is opposed by the restless molecular motion expressible as the *temperature.* This brings in the laws of probability relating to large numbers. Since there are many possible disordered states and only one or few ordered states the probability that the system will be disordered is greater than that it will be ordered. Entropy of disorder is the logarithm of the probability, and the $T\Delta S$ term represents the effect on the free energy of the system due to molecular statistics. At low temperatures the ΔU term predominates, and molecular systems order themselves into crystalline forms; at high temperatures the $T\Delta S$ term is the greater, and disordered systems of liquids and gases are found.

It was noted by early physical chemists that changes from crystals to liquid, or liquid to gas, occurred at remarkably sharp temperatures. From the above equation the change-over temperature T_c is given by: $T_c\Delta S = \Delta U$, and either term equals ΔQ, the *latent heat* of the change. The expression *phase* was employed as a general name for a state of aggregation, and *phase changes* were realised to occur at *fixed temperatures* and to be accompanied by the liberation or absorption of *latent heat.* The simple phase changes solid ⟷ liquid ⟷ gas were supplemented by solid ⟷ solid changes, enantiotropic (with realisable transition-point, *e.g.* rhombic → monoclinic sulphur) or monotropic (transition-point above the melting-point, *e.g.* yellow and red phosphorus), where the same considerations apply. Different crystalline modifications correspond to different modes of molecular packing of different energies and entropies.

The above concepts of *phase* lead to the idea of molecular systems of like molecules existing wholly in one phase or another except at certain *transition-points* where two phases can exist together; as the transition-point is crossed the molecules are imagined to tumble entirely from one state of order into another. The translation of the simple thermodynamical equation into molecular statistics, however, is complicated by the fact that variations of ΔS and ΔU occur with volume and temperature, manifesting themselves in thermal expansion and in specific heat changes. The possibility exists therefore that the equation $T\Delta S = \Delta U$ might not define a sharp temperature, so that under some conditions melting, for example, would take place over a range of temperature and the phase change be gradual instead of abrupt. To form a molecular picture of this possibility it is necessary to try and visualise the effect of temperature on crystalline solids and liquids.

In the first place, crystalline solids cannot be regarded as possessing *perfect* regularity of molecular arrangement. Owing to chance accumulations of high thermal energy on certain molecules displacements from lattice positions may occur. There are two possibilities: the molecule may temporarily take up an *interlattice* position, or a "hole" may appear in the crystal with a corresponding new site occupied at the surface. These two possibilities are illustrated in Fig. 1. The fraction of such displacements in equilibrium at any temperature can be shown thermodynamically to be given by $e^{-E/RT}$ where E is a displacement energy. The reality of this effect has been shown clearly in the last few years, particularly for crystalline salts at high temperatures, though even near the melting-point the experimental values for the displaced fraction are as low as 10^{-5}. If continued in imagination until the bulk of the molecules were displaced a disordered system resembling a compressed gas or a liquid would be arrived at. Secondly, considerations based on X-ray reflections from liquids, on the similarities of volumes and of specific heats of liquid and crystalline forms, on the expansion of water below 4° C. and also on freezing, etc., indicate that liquids are as closely akin to crystals as they have long been assumed to be to gases, and that some degree of orientation must be present among the molecules even in a fluid liquid.

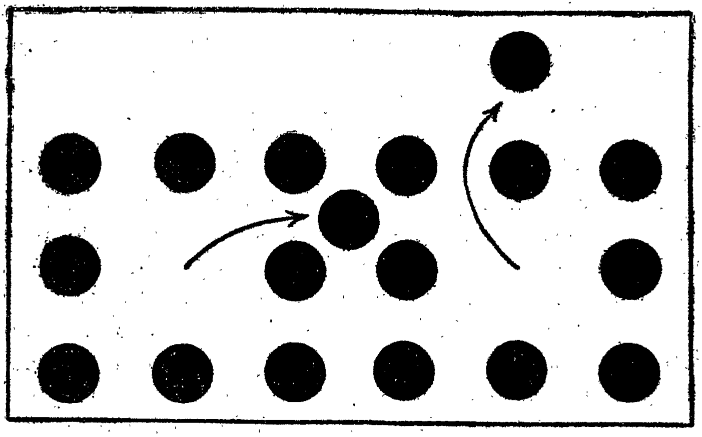


FIG. 1.

A second way of imagining a crystal turned into a liquid then is to visualise a large crystal gradually reduced to a polycrystalline state with larger and larger numbers of differently oriented micro-crystals. At low temperatures the balance of the terms $T\Delta S$ and ΔU leads to an equilibrium with the minimum of polycrystallinity, *i.e.* to single crystallisation, and at high temperatures to a limit where the boundaries between the micro-crystals become so numerous and so mobile that rigidity is lost and the system is liquid. On both these views transition from solid to liquid is *continuous*, and the melting-point will be a temperature *range*, possibly too small to measure but nevertheless finite in theory. As is shown later, such melting-point ranges are found experimentally, but the range only becomes marked in certain types of molecular aggregations.

Similar to solid ⟷ liquid transitions are solid ⟷ solid changes. The commonest type are the numerous examples of polymorphism, where the molecules can build themselves into more than one kind of crystal lattice which are not energetically very different, though geometrically dissimilar. Sometimes the different arrangements merely differ in the thermal motions of the molecules. The best-known example is ammonium nitrate. At high temperatures the NH_4^+ and NO_3^- groups in the crystal rotate freely and behave as spherical ions, but on cooling the system passes through transition points at certain temperatures where the rotations cease to be maintained against the adverse effect of lowered thermal energy. The rotations pass over into oscillations to and fro, and the entropy change associated with this causes latent heat development at the transition temperatures. Similar phenomena are shown by crystals of long-chain fatty acids and other substances.

Of greater subtility than the above forms of polymorphism are the order-disorder changes observed in alloys. An example is the alloy Cu_3Au. The crystal structure is face-centred cubic. At high temperatures the copper and gold atoms occupy indifferently all the lattice sites. At low temperatures (after suitable annealing to secure equilibrium) the copper atoms occupy the centres of the faces of the unit cubes of the structure and the gold atoms occupy the corners. No change in gross crystal structure occurs, but the transition from the ordered low-temperature form to the less-ordered high-temperature one is made manifest by changes in the X-ray reflections, by specific heat anomalies and by peculiarities in the electrical conductivity. The change occurs over a temperature range, and the ratio of order to disorder is given by a term $e^{-E/RT}$ where E is the energy required to produce N pairs of disordered atoms. In other instances a continuous change of crystal symmetry may accompany the order-disorder transition, as from cubic to tetragonal. These matters have been the subject of considerable mathematical consideration, and the results are in reasonable agreement with observations.

The commercial development of plastic materials in late years has greatly stimulated work on order and disorder among molecules. It is in this field that the older apparently clear-cut concept of "phase" becomes blurred, sharp transition points are no longer observed, and the distinction between solid and liquid cannot be maintained. An ideal liquid is supposed to yield at the smallest force; when a graph of applied force is plotted against rate of shear a line is obtained, normally straight, but curved for liquids with anisometric particles which are oriented by flow, and always passing through the origin. An ideal solid is *elastic*; after distortion by a transient force it returns to its original shape. The change from one state to the other is caused by change of temperature; if the solid is a well-oriented crystal there is a sharp melting-point, but among plastics the transition is spread over a wide temperature interval. The intermediate forms between solid and liquid show a variety of peculiarities, including the qualities of *plasticity, reversible extensibility,* very high *viscosity, toughness, flexibility,* and *setting properties.* As one or more of these qualities are sought for in particular plastic preparations their dependence on molecular structure is a matter of great importance.

A substance shows *liquid* or *viscous* properties when the forces between the molecules are small and the temperature high. The smallest steady force causes the liquid to yield because it inexorably takes advantage of the continual "letting go" of each other by the molecules. An *elastic solid,* however, has tightly bound molecules, and ideally does not flow, though under great stress it may fracture. Below the yield-point solids show *elasticity,* but the extent of the deformation they can stand is very small. Under a force acting so rapidly that few molecules release one another in the time a liquid will show solid-like behaviour, as well shown by pitch or by "belly-landings" in bathing. Three classes of chemical interaction may be distinguished (neglecting the metallic state), (*a*) van der Waal's forces, *i.e.* weak interactions between chemically saturated molecules; (*b*) ionic forces, which are strong, and are found in salts; and (*c*) chemical valency forces between atoms in molecules, which are the strongest of all. Systems with the first type of force only, *e.g.* methane or benzene, have low melting-points, low viscosity as liquid and little hardness in the crystalline form. Somewhat stronger intermolecular forces give rise to higher viscosities, as with glycerol. Systems with (*b*) and (*c*) type forces, as sodium chloride or quartz, have very high melting-points and very great mechanical hardness. Chemical valency forces are directed in nature (ionic forces are not), and when the molten forms of systems of type (*c*) are cooled rather quickly the molecules may not have time to take up a perfect crystalline regularity. With decreasing temperature the small molecular aggregates increase in size and geometrical factors compel them to form a very irregular linked structure, rigid but with no crystalline order, *i.e.* to form a "glass" with no sharp melting-point. The liquid passes to glass from a mobile to a highly viscous, then plastic, and finally a rigid state. The intermediate *plastic* state is ill-defined and defies clear specification according to scientific methods, yet the property of plasticity is so important technically that it cannot be ignored. Roughly speaking, a system is plastic when it behaves like an elastic solid up to a certain force (the "yield-point"), while under higher forces it flows like a viscous liquid, *i.e.* the force-shear curve does not pass through the origin. The system may be visualised as having neither all weak nor all strong intermolecular forces, but to have many weak ones and a few strong ones. Application of a force is unresisted by the very weak ones (which are continually "letting go"), but puts a severe strain on the few strong ones. If the applied force is high enough these yield one by one. It is characteristic of plastic materials to yield and flow not smoothly but in a series of little jerks, due to the slow accumulation by chance of unsupportable strains on the few strong linkages in the system, so that they snap successively. In most systems the linkages heal themselves by the molecules finding new partners after a breakage, so that the jerky nature of the flow is maintained; if no healing occurs the result is fracture.

The precise physical chemist has in the past regarded plastic systems with disgust, owing to their unconventional behaviour, but their very difficulties are now exciting special interest. They cannot be characterised by sharp temperatures of phase change, while their "yield-points" and viscosities not only depend on the rate of action of the external force but even on the previous history of the material. The clear-cut ideas of stable states here have to give way to statistical considerations relating to many ill-defined states of nearly equal probability. The complications of plastic behaviour are shown in an aggravated form by two phase systems. Not only do the flow properties of plastics vary with rate of shear, an effect attributable to particle orientation during flow, but their mechanical behaviour may show such peculiarities as *thixotropic* or *rheopectic* effects. Mixtures of the colloidal clay bentonite with water, for example, are *thixotropic.* On standing the mass sets to a stiff mass, but on stirring the systems turn to a wet slurry of low viscosity. The effect is indefinitely reversible, and clearly makes an assessment of the viscosity or yield-point of the mixture a difficult matter. *Rheopexy* is almost an opposite effect. When stirred gently the system is fluid, or relatively so, but rather more violent treatment such as turbulent stirring or tapping brings about a considerable stiffening of the mass, sometimes accompanied by an expansion of volume ("dilatancy"). This behaviour is shown by starch pastes or by wet sand when trodden on. Thixotropic properties are shown by systems with anisotropic particles. On standing the particles slowly aggregate by localised interattraction forces into chains or networks whose criss-crossings through the surrounding liquid create a high viscosity or stiffness. Stirring disrupts this tenuous structure and reduces the viscosity. Rheopexy may arise when similar systems have a high concentration of such particles ; gentle movement here allows the particles to orient and slide over one another fairly freely, while turbulence destroys the mutual accommodation of particles to one another and causes them to pile up and lock together in a chaotic mass like shattered boulders on a mountain. Systems of this kind are almost intractable in detail by thermodynamics.

Plastic materials contain very anisometric particles ; the organic plastics have very long chain molecules with enormous molecular weights, produced by polymerising simple molecules together. The process of polymerisation cannot be controlled to give systems of equal-sized particles (" monodisperse "). All such plastics contain a mixture of large and small molecules, and therefore, although apparently a single phase, has two (or more) phase properties in the sense that the material may be regarded as a mixture of the large particles with the smaller. The thermodynamic concept of phase, however, loses its clarity here because the bigger particles, though large, are still of sub-microscopic dimensions, and because of the continuous range of particle sizes. The mechanical properties of the material will clearly be dependent on (*a*) the relation between the proportions of long and short molecules, and their absolute lengths ; (*b*) the degree of orientation of the molecular chains ; (*c*) whether cross-linking between chains occurs ; and (*d*) the freedom of movement of the chains to slide past each other ; and (*e*) other possible movements of the chains. The behaviour of these matters may be illustrated with examples.

The longer the molecules (or the more extensive the degree of polymerisation) the more will the plastic assume " solid " characteristics ; admixture with smaller molecules will soften the plastic, ultimately giving a liquid of high viscosity due to entanglements of the longer chains. Cellulose acetate forms a solid plastic ; acetone converts it to a viscous liquid which dries out to the plastic again. Dibutyl phthalate also softens it by separating the chains, and being non-volatile has a permanent effect, namely, acts as a " plasticiser." For a molecule to act as a plasticiser it must obviously have sufficient attraction for the polymer molecules to overcome the interattractions of the latter and to force them apart and overcome the rigidity of the polymer mass.

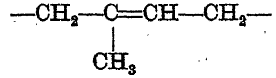
The influence of orientation is well shown by nylon, a polymer with long chains containing the linkages

$$—CH_2—NH—CO—CH_2— \quad . \ . \ . \quad —CH_2—CO—NH—CH_2—$$

The long polymer molecules after preparation are all tangled up in irregular shapes, and the material is not specially strong. When the substance is stretched it draws down to a finer and finer fibre up to a certain point; it then stops with a jerk. At this stage the long chains have been straightened out by the stretching until they have been brought practically parallel to each other and close together. The strong chemical attractions of NH for CO on adjacent chains are thereby brought into play and the chains no longer relax into coiled forms nor slide further over each other's lengths. A fibre of oriented particles and of exceedingly great mechanical strength and resistance to kinking and solvents is so produced. Had the chains been solely of hydrocarbon type, with small interattraction forces, tension would have produced indefinite sliding over each other of the chains, and some recoiling on release of tension. The plastic polyethylene, however, has such long chains that their mere mechanical entanglement gives the material good tensile properties.

Cross-linking between chains favours rigidity of the polymer. Bakelite, the phenol-formaldehyde plastic, has a cross-linked structure and is a rigid tough solid. Polystyrene and polymethyl methacrylate resins are similar, except that the cross-links are of van der Waal's type and easily broken on warming, so that the materials are "thermoplastic." Partly polymerised bakelite, on the other hand, is "thermosetting," for heating develops further chemical linkages which need such heat to break them again that the whole plastic is decomposed.

The most valuable of all the numerous properties of plastics, and the one which has excited the greatest theoretical interest, is that of "reversible extensibility" or rubber-like nature. Because of the need for synthetic rubbers much work has been directed in late years towards an understanding of the molecular conditions in a system which enable it to be used as a rubber band or check-ropes on an aircraft carrier. The thermodynamics of the elasticity of rubber prove to differ fundamentally from those of the elasticity of ordinary hard solids such as steel. When a spring is stretched it remains at constant temperature and the energy is stored as potential energy. Molecular linkages are twisted by the stretch, and the stretching energy is converted into work done against the action of intermolecular forces, *i.e.* is the ΔU term of the beginning of this article. Rubber on the other hand heats on stretching, and when allowance is made for "heat of crystallisation," discussed below, it is found experimentally that all the work of stretching is converted into heat energy and "lost." The stretched rubber, however, is still capable of snapping back when released. The term affected by stretching in the equation: Work $= T\Delta S - \Delta U$ must here be the entropy term $T\Delta S (= \Delta Q)$. On release of tension the rubber must fly back, not because its molecular linkages had been bent or twisted, but because the shortened state is "more probable" than the stretched form. The system must have close analogies with that of a gas. A compressed gas thermodynamically resembles stretched rubber; both have evolved heat equal to the energy of compression or stretching, yet both have a tendency to release the energy again intact later (cooling in the process). The entropy term for a gas arises from the fact that a large volume is a "more probable" state for an assemblage of free gas molecules than a small volume. It only remained for this concept to be suitably transferred to the rubber molecule to have an explanation of a "rubber." Natural rubber contains very long chains of polymerised isoprene:



$$-CH_2-\underset{\displaystyle CH_3}{\underset{|}{C}}=CH-CH_2-$$

In the raw state it is imperfectly elastic; the stretched form "creeping" owing to the chains slipping past each other. When heated with sulphur the substance is "vulcanised." This treatment ultimately yields the hard rigid material known as ebonite; the sulphur reacting with the double-bonds of the chains and bringing about extensive cross-linking between them so that they are locked together. More moderate vulcanisation, however, gives a rubber in its optimum stretchable form. The amount of sulphur for this purpose produces cross-links only here and there, enough to prevent viscous flow, but leaving long intervening lengths of chain unaffected. Now the valency angle C—C—C is 109·5°. If we consider a chain of single links and start from the first, regarded as fixed in position, the second link can lie randomly at any position on a cone of vertical angle 109·5°. From any of these positions the third link in the chain can lie at any position on another similar cone, and so on. An almost unlimited number of molecular shapes is thus possible, some elongated, some very tangled up. The distribution of overall molecular lengths can be calculated statistically just as Maxwell was able to calculate the distribution of velocities among molecules in a gas. The overall length is given by the expression (probability of a length r) $= k_1 r^2 e^{-k_2 r^2}$ where k_1 and k_2 are constants. This curve is well-known in probability theory and shows on plotting or differentiation that the most probable value of r is $1/k_2$. Chains, *i.e.* those portions of the molecules between the few cross-links, will favour the coiled-up forms this length corresponds to if left alone. On stretching, however, the chains are forced to take up elongated forms. When the pull is released, the material contracts not because linkages have been twisted against intermolecular attractions, since no force is required to bring about the valency bond rotations here considered, but because the elongated form is improbable, *i.e.* has a low *entropy*, and seeks to return to the state where the entropy is a maximum. In just the same way a compressed gas expands when the pressure is released, as it is more probable that the molecules will occupy all the new space available instead of remaining isolated in a small part of the total volume.

The orientation of the chains by stretching is easily shown by X-ray reflections. The approximately parallel molecular arrangements so produced can be regarded as an imperfect type of crystallisation. The artificial forcing of molecules alongside each other by stretching leads to the liberation of "latent heat" caused by the effect of intermolecular attraction forces. This is superposed on the entropy heat mentioned above. The "crystallisation" is very difficult to define precisely. X-ray results show that the average sizes of the well-oriented regions are much less than the effective chain length of the substance. Instead of the "crystal" being greater than the "molecule" we have here molecules longer than the "crystals." Fig. 2 shows diagrammatically the chains in stretched and unstretched rubber; in the stretched state each chain passes through several "crystalline" and several "liquid" regions.

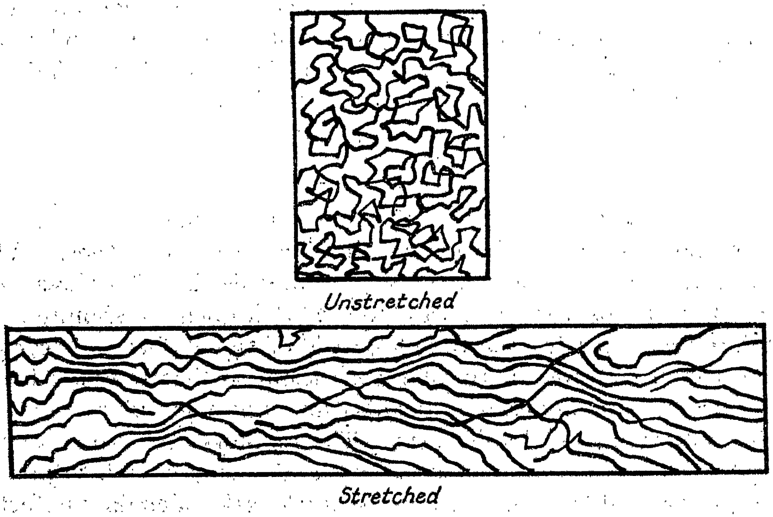
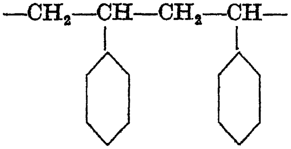


Fig. 2.

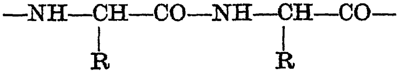
A number of chain-polymers of very diverse types show rubber-like properties, *e.g.*

Plastic sulphur $—S—S—S—S—S—$

Polystyrene



Gelatine (when swollen)



$$—NH—\underset{\displaystyle R}{\underset{|}{CH}}—CO—NH—\underset{\displaystyle R}{\underset{|}{CH}}—CO—$$

and in spite of its rather indefinite character on the "phase" concept the rubber-like state must be recognised as an intermediate form between liquid and solid. The intermediate form differs from a "glass" in that it is composed effectively of "solid" and "liquid" regions separated by hazy boundaries. Rubber-like properties exist only within a temperature range, not definable of course by sharp limits. At low temperatures the molecular chains have insufficient thermal energy to vibrate freely; they are locked to each other by van der Waal's forces and the material is rigid; at high temperatures the inter-molecular forces are unable to withstand a sustained stress and the material shows viscous fluid properties. Only between these temperatures can rubber-like properties be developed, and then only when the intermolecular forces are just of the right character, not too powerful as in nylon or dry gelatine, nor too weak as in hot plastics, and having long relatively freely moving chains interconnected by strong valency forces at a few suitable points. The development of a rubber-like state is clearly very sensitive to inter-chain attraction forces; hence its critical dependence on the nature of side-chains. Interpolymers, where more than one type of molecule is introduced into the chains (as in the buna rubbers), may give better rubbers than the component substances, and plasticisers (as swollen gelatine plasticised by water) often favour these properties by modifying the chain attractions in just the right way. A vast field still awaits exploration here. The general principles are fairly clear, but the detailed application can only be solved by widely spread laboratory work.

SOME ASPECTS OF THE GEOLOGY OF WATER SUPPLY

By Emeritus-Professor P. G. H. BOSWELL, D.Sc., F.R.S., F.G.S.

Recent legislation, long overdue, has resulted in the placing of important Water Acts on the Statute Book. When, sixty years ago, the pioneer work of the Franklands caused substantial improvement in the purity of public water supplies we passed one milestone on the road to national health. Now, since the new Acts are designed to secure, in due course, an adequate supply of pure water for every inhabitant of Great Britain, we may congratulate ourselves on reaching, even if tardily, another milestone.

The new Acts are concerned, among other matters, with the provision of water for rural as well as urban communities; with protection of sources from pollution; with conservation of supplies in areas where there is risk of over-development; and with the compulsory recording of geological and other data obtained when new wells and boreholes are sunk or old ones extended. Although the Acts apply to Great Britain, the principles underlying their provisions are necessarily of wide application: moreover, the information that will be gathered and the experience that will be gained as schemes are promoted under the new charter will prove of great value to those whose business it is to provide water for domestic or industrial use, no matter where they may be called on to dam lakes and rivers or bore the neighbourhood.

It is with the scientific repercussions of the new legislation, especially as regards underground waters, that we are here more particularly concerned: and in this connection it is appropriate to review the progress during recent years of the application of hydro-geological principles to the exploitation of water supplies.

Hitherto, returns of information about the geological strata met with in wells and boreholes, the depth at which water was found, the quantities yielded on pumping, and similar matters, have not been obligatory. Now, however, the new Acts require that persons abstracting water from any source shall record and forward the desired data; and they lay down certain requirements as to notices, inspection and returns which must be complied with in future by those who sink wells and boreholes in search of water of a depth of more than 50 feet below the surface. With these statutory obligations, it will probably not be so easy (if we may recall the pawky humour of a Scottish geologist) to be unwearied in well-doing.

Many enlightened well-sinkers (with the permission of their clients, either private consumers or industrialists) have in the past made a practice of forwarding details gratuitously to the appropriate Government department (namely, the Geological Survey of Great Britain). Unfortunately, however, many wells and boreholes have been sunk, and some in due course even abandoned, without the information being made available for the benefit of the community as a whole. Here, it should be emphasised that, in operations involving geology, even if water is unobtainable, failure is a good second-best to success. Moreover, experience has shown that most of the records submitted lack completeness and accuracy; occasionally, this arises from a fear that the information may help a rival, but in many instances it has been the result of ignorance of the nature of the records required. There is now no excuse for inadequacy of detail, since, following the passing of the Acts, the appropriate authority (the Department of Scientific and Industrial Research) has issued a memorandum setting out clearly the matters to which attention should be directed in making the returns.

In this document persons concerned with the sinking of wells and boreholes are required to keep a journal of the progress of the work; to record the precise site (preferably marked on a 6-inch Ordnance Survey map); to supply information about surface level, depth and diameter of the well or boring; to record the lengths, diameters, perforations, etc., of lining tubes; to supply details of strata met with; and to record the levels and flow of water, the results of test-pumping and the time of recovery of water-level after cessation of pumping. The Director of the Geological Survey must be notified of the operations so that a competent officer can visit the site, inspect the records and see the cores or other geological specimens obtained. Provision is made for keeping the information confidential if so desired.

So much for the legislation: now to consider the scientific aspects. At the outset, we should remind ourselves that the provision of an adequate water supply for every inhabitant of the kingdom is, fortunately for us, only a matter of £ *s. d.*, since the rainfall of Great Britain is more than sufficient to satisfy all the country's needs many times over. But, although the rain falls indiscriminately upon "the just and unjust fella," it is usually more abundant in sparsely populated regions than in dense urban areas where consumption, both domestic and industrial, is highest. This irregular distribution and demand calls for schemes which often involve the transport of water for long distances (100 miles or more); and cost naturally rises with storage capacity and length of pipe-line. At present, the greater part of the population relies on surface supplies of water, *i.e.* from springs, rivers, lakes and impounded reservoirs. Their exploitation is a task for engineers, although geological advice is sought when questions arise of the character of the ground and structure of the rocks. For the recovery of underground supplies, on which about 30 per cent. of the population are dependent, the active co-operation of geologists and water engineers is desirable and, indeed, general.

In Britain, conditions are exceptionally favourable for the study of underground water. In the first place, for example, we have many excellent aquifers (*i.e.* water-bearing formations) of different types and, in the second place, the country has been riddled with borings. Centuries of mining for coal and other useful minerals, apart from the demand for water in a terrain of relatively dense population, have placed us in a scientifically advantageous position in this respect. In few countries of similar size can the earth's surface layers have been so repeatedly pierced; and, in particular, in the London and Greater London area (occupying about 2700 square miles) the wells and boreholes amount to many thousands. Consequently, our knowledge of the hydro-geology of this area is extensive and peculiar, and serves admirably for illustrating and extending the well-known principles of water supply. Indeed, we must have all possible information here, since the inhabitants number almost one-quarter of the total population of Great Britain, and Professor Sir Patrick Abercrombie's fascinating dispersal scheme raises many water-supply difficulties.

With few exceptions, the ultimate source of underground water is rain. A certain proportion of the rainfall is lost by evaporation from the soil and the remainder either flows off the surface as rivulets and streams or sinks through the soil and subsoil, the respective routes depending on whether the underlying geological strata are impervious or permeable.

It is familiar to all that in country occupied by thick clay deposits there can be no appreciable percolation underground, and in districts where such rocks as limestone (including chalk), sandstones, sands and gravels crop out there may be practically no surface run-off. In such permeable ground 20 to 60 per cent. of the rainfall may pass underground, to be stored in the pores or fissures of the rocks. If the permeable beds are thin, they necessarily store little water: therefore, we turn to the thick formations for large-scale supplies. The saturation of a porous and fissured rock has led to the use of the rather loose expression "underground reservoir," for want of a better term, but it ought not to be allowed to convey the idea of a vast underground cavern in the rock, since such a feature is very rare in the earth's crust. In fissured rocks such a reservoir is often a congregation of tiny rivulets.

The level to which water rises in a permeable formation is measurable in shafts and boreholes. As is well known, it is subject to seasonal fluctuations, rising after rainfall and sinking during times of drought. Obviously, therefore, this reservoir of water-saturated rock should not be envisaged as one of still water. There is, indeed, a moving body of water: were it not so, the interstices of the rocks would long ago have become filled up and water would now be perpetually overflowing at the surface. Locally, of course, it does become filled up, in which case we see swamps and water-logged areas. Generally speaking, however, the water is in constant motion, finding its own level locally, so that the surface appears horizontal, but actually working its way slowly downwards, its level and gradient determined by the situation of its outlets, as will appear hereafter.

To state that underground water is in motion is, of course, a commonplace. Most of us are familiar with the results of experiments designed to determine the direction and rate of flow. The common method of determining such movement is that of putting harmless salts or dyes into wells and boreholes and watching for the appearance of the doctored water in other wells or springs and rivers. From these experiments it is evident that the course of water underground may in places be very sinuous. At times it reappears in the least expected spots. In the great Carboniferous Limestone area of Yorkshire, for example, the wayward behaviour of water near the head of the River Aire has long aroused interest. The stream from Malham Tarn sinks underground about half a mile below the lake and leaves the valley of the Aire dry for nearly a mile. The water which then emerges as springs at Malham Cove proves to be a different stream, however, which has sunk underground at Smelt Mill Sike, some distance towards the north-west; also, the Malham Tarn water is found to emerge farther southwards as the Aire Head springs. Thus there is evidence of separate subterranean courses, which may cross and recross, but with no com-mingling of the water, for each stream apparently keeps to its own fissure-system.

Again, similar experiments in the Chalk area of the London Basin show that water which passes underground by way of the Mimms swallow-holes in the upper part of the valley of the River Colne near Watford reappears in the Lee valley near Amwell. It thus travels north-eastwards, taking 2 to 3 days for a journey of 10 to 12 miles (Report of the Director of Water Examination, *Annual Report of the Metropolitan Water Board*, 1937). Not the least interesting feature of this example is that the water takes a course very different from that of its normal flow, which we know from other evidence to be south-south-eastwards or southwards. Evidently, part of the water which sinks underground takes one course and part another, although journeying to the same goal, the central and deeper part of the London Basin.

Fortunately for geologists (who are expected to be prophetic in these matters) such instances of erratic behaviour of underground water are exceptional. In the main, the problem remains simple, provided that the data on which we work are reasonably numerous. These examples do serve to emphasise, however, that it is not safe to assume that the drainage areas and watersheds of the underground water system correspond accurately with the catchment areas and watersheds of the surface (or overground) drainage.

It is therefore important that as much information as possible about the levels and character of underground water should be obtained. In porous rocks the various levels (determined by measuring the depth of water in wells and boreholes, or observed in shafts, tunnels and adits) indicate the position of the surface of saturation, or "water-table," as it is often termed. This surface may be almost plane in limited areas, with a more or less gentle gradient, or it may prove, on being plotted on a map, to be rolling, much like a subdued landscape, with smooth hills and hollows, well-defined valleys and sometimes relatively steep scarps.

In the case of fissured rocks like chalk, the rock material is either of low porosity or contains pores too small for water to travel freely. The water then rises and falls in fissures, still endeavouring (but sometimes less successfully than in porous rocks) to find its own level. Nevertheless, one observes that when cavities such as wells, boreholes, adits, etc., are made in such rocks, the levels of the water surface tend to lie on a similar plane. Thus, we can still speak of the water-table, even if it becomes a rather theoretical conception, in fissured ground which has not been riddled by borings. This leads to the conviction that, in the study of water supply, one of the most promising methods of the past fifty years has been the construction of maps showing the contoured surface of the water-table. Such maps have been termed *isopiestic* maps, because they furnish a clue to the varying pressure, due to hydrostatic head, at different localities. The use of these maps in the past has been hampered by the scanty nature of the records in certain areas, but as more information has accumulated, the cartographic method of analysing and interpreting them has proved of increasing value. Hence, it is impossible to exaggerate the importance of the provisions of the new Water Acts which relate to systematic recording of information. More records and more accuracy in the data will result in greater precision being given to predictions.

The floor of the underground reservoir is formed by a bed of impervious rock (for example, clay, mudstone or crystalline rock such as granite) underlying the permeable water-bearing stratum. Since the geological succession in the earth's crust often consists of alternations of permeable and impervious strata, several distinct reservoirs can be present, each with its water-table. A well-known case in the Greater London area is that of the upward succession Weald Clay, Lower Greensand, Gault Clay, Chalk, London Clay and associated beds; in which the aquifers are the Lower Greensand and the Chalk. They are separated by a thick bed of Gault clay, and each has its water-table which may be tapped by making borings of appropriate depth.

Since the form of the water-table is influenced by capillary action, it rises under hills and falls under valleys in such a way that its shape is in general similar to that of the surface topography, but is less pronounced. Its gradients are controlled, as would be expected, by the position and level of the points where it intersects the surface of the ground; also, by the rate at which it can percolate through the rock—which depends on the mechanical properties (such as particle-size, grading and cementation) and structure of the rocks. The water appears as springs and seepages, many of which lie on valley-slopes, in river-beds, or close to the seashore. Ultimately, all ground-water not tapped *en route* reaches the sea. If it is held down by clayey beds in maritime areas and is under hydrostatic pressure because of the height of its catchment inland, it may emerge as fountains of fresh-water in the sea, with disastrous effects on fish and other marine organisms. Usually, however, it trickles out on the sea-shore or oozes from cliffs. As the level there is approximately Ordnance datum, the water-table along the sea-shore shows the zero contour.

The surface of the water-table may also show a zero contour in inland areas where basin-like hollows have been produced, because water is being extracted by pumping at a greater rate than it can travel in from the outcrops of the aquifer. Such areas are said to be over-pumped (Figs. 2 and 3). Here, indeed, the hollow may extend below sea-level, with the result that contours can be plotted at 100 feet, 200 feet, 300 feet and so on below O.D. In low-lying areas with internal drainage, like the Dead Sea region, we can also have negative contours, determined by the low level that is provided for the exits of the ground-water.

An early water-contour map of the London area (possibly the first of its kind) was prepared about seventy years ago by J. Lucas, a pioneer in the scientific study of underground water (Fig. 1). He recorded observations of the seasonal fluctuations of water-level and of the depressing effect of pumping on these levels. Thus he showed clearly that, in order to produce accurate contour-maps, we must have regard to the season of the year at which the records were made and if possible take all our readings at nearly the same time; also, that it is the rest-levels (or standing-levels) and not pumping-levels (or working-levels) which should be plotted. These ideal conditions can but rarely be approached; however, when R. E. Middleton, a distinguished engineer, was asked by the Royal Com-

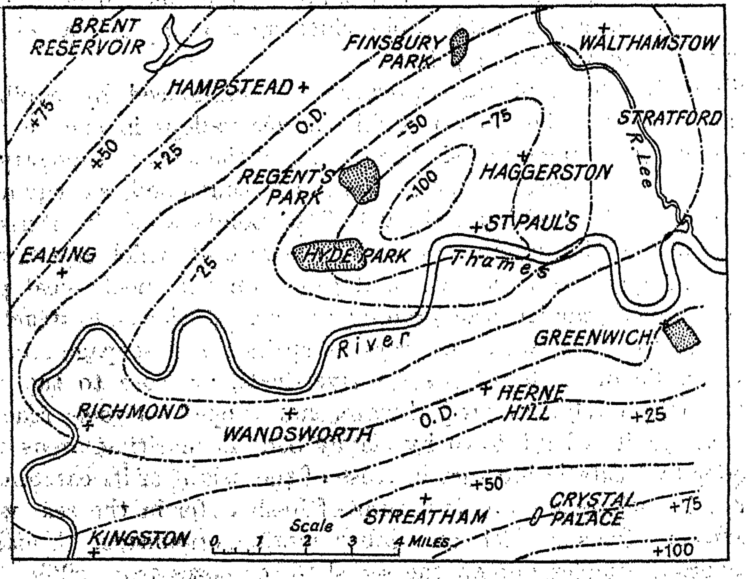


FIG. 1.—Water contours in the chalk of the London area, 1879 (*after* J. LUCAS).
In this and the other figures the contours are referred to O.D. Although the contours show evidence of overpumping in the City area, they still follow broadly the geological structure.

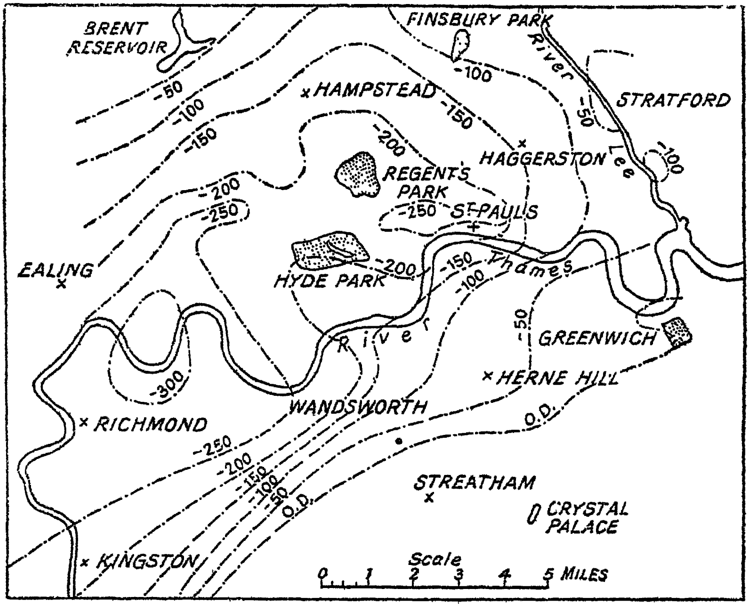


FIG. 2.—Water contours in the chalk of the London area, 1936.

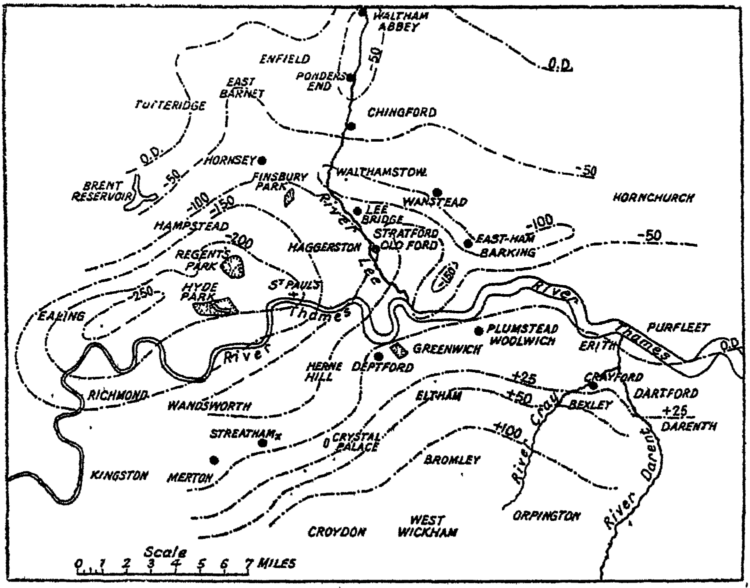


FIG. 3.—Water contours in the chalk of the London area, 1943.

(HETHERINGTON, *Trans. Inst. Water Eng.*, 1944, **49**, 133 and 136.)

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mission on the Water Supplies of the Metropolis, 1893, to investigate water-levels in the northern part of the Greater London area, he succeeded within a few weeks in producing a contoured map of the water surface from observations he was able to make over a wide area. His map has proved of great value and interest.

Subsequently, maps showing the water position in various parts of the country have been produced, but all, so far as the writer is aware, have been compiled from records made at different times (excepting in a few small districts) probably because no official organisation existed for taking synchronous records and no private individual had the time, money or facilities to do so. Thus these maps lack accuracy; nevertheless, when used with knowledge of the probable limits of error, they are of considerable value. And they give us an opportunity for determining broadly the changes, if any, in water-level that have taken place in the course of years (*cf.* Figs. 1, 2 and 3).

Between 1929 and 1936, Mr. R. C. S. Walters prepared maps on a 1-inch scale depicting the water contours of the chief aquifers in England, namely, the Triassic sandstones, the Oolites and the Chalk. Layer-coloured according to height above O.D., his maps were beautiful and illuminating productions, being published by the Institution of Water Engineers and, later, in *The Nation's Water Supply* (London, 1936). Unfortunately, in order to bring them into reasonable compass, they had to be reduced to scales of 4 and 10 miles to 1 inch. From them, however, we can (as Mr. Walters did) draw certain useful deductions, although they cannot serve for detailed work in particular localities, since recourse must then be made to 6-inch or 1-inch maps. It may be noted, however, that in certain areas the reduction in scale probably resulted in some compensation of errors.

We may next consider the possible errors that are likely to arise from use of the existing records. Since the standing-level of the water in the various wells has, with certain exceptions, been noted only occasionally (the dates usually being unspecified), the average levels in any particular wells cannot be determined. In some instances, however, levels in certain groups of wells have been recorded in one and the same season, and in other cases, continuous records of standing-levels in individual wells have been tabulated throughout many years, when the average level can be calculated.

Since the contours are plotted so as to pass through and, proportionately, between a number of controlling points, their positions may be subject to some modification when additional information becomes available. It is noteworthy, however, that on the scale of 1 inch to 1 mile new records are now found in many districts to fit in satisfactorily with the older plotting, thus necessitating little correction. Where modifications of the position of the contours are required, they are usually in the nature of adjustments marking small elevations or depressions of the water-table. Since they do not call for a change in the main run of the contours, we can feel confident that our maps to-day represent the average position of the water-table during the past few years.

Where discrepancies occur, we find that the older records are usually out of accord. It is therefore an advantage to adopt the practice, as Dr. David Williams has done, of indicating on the maps the decade during which each particular observation of water-level was made. Then, especially in areas known to be overpumped, or in danger of becoming overpumped, the earlier records are readily detectable and can be adjusted.

Allowing for the possible margin of error in these contours, we can utilise the maps for predicting, with due caution, the standing-levels at intermediate points ; for helping in the siting of new wells ; and for estimating likely yields and the character of the water. Moreover, we can deduce from them certain interesting conclusions concerning the behaviour of ground-water in the area depicted. Some of these points are referred to in the sequel.

To consider first the problem of predicting the probable standing-level in a newly sited well, which depends on its position with respect to known contours, we may note that limestones such as the Chalk have been regarded as too highly fissured for the water-level to be of more than local value. Now it is true that we have knowledge of closely set, almost adjacent, boreholes or wells in which standing-levels differ by so much as 60 or 80 feet, but in the writer's experience such great differences are exceptional. They can be explained by regarding the lower of the levels as that of water confined in a relatively closed fissure-system (*i.e.* one in which the outlet fissures are very fine and/or greatly ramifying), for the upper of the levels usually accords with the average height of standing-level of the district. Although these abnormally low levels are met with occasionally, it is rare to find a reliable modern record appreciably higher in level than that of the average for the neighbourhood. Therefore, despite the well-known effects of the sinuous fissure-systems of the Chalk, the ground-water apparently fills up the interstices of the formation to concordant levels under atmospheric pressure, and is often assisted in reaching its proper level by the excavation of wells, adits and boreholes. Although a standing-level may take longer to attain in such fissured rocks, it seems to be as reliable as that in sandy and gravelly formations.

The form and spacing of the contours shows that there is a steady downward movement of water under greater or less pressure towards the sea. We are justified in assuming that the direction of most rapid flow is at right-angles to the contours, although we realise that fissure-systems modify the direction locally and may even, in special cases, lead for a space to movement oblique to, or even parallel with, the general run of the contours (as in the Colne-Lee case cited above). Since the height of the water-table is in part controlled inland by its emergence in valleys, the V-ing of contours up major valleys is a characteristic feature of water-contour maps. Further, it is perhaps noteworthy that even where the water-table does not emerge in valleys, but is situated at some distance below them, the same feature is seen (*e.g.* the R. Cray and R. Darent). From this fact we may infer that the valleys mentioned originally owed their position to the ease with which erosion could take place along belts of country which were vulnerable because of open fissuring; and that here the rock was more rapidly disintegrated and the water enabled to penetrate downwards and seawards more easily.

On the other hand, where a permeable formation like the Chalk is concealed by a thick cover of impervious strata such as the Tertiary deposits (as is the case over a large part of the London Basin) and the valleys do not cut through this mantle, the water-contours take no account of the existence of surface depressions such as valleys, but pass directly across them, following the general trend for the district. Examples are seen in the lower part of the Colne valley, the Lee valley area below Hoddesdon, and the Roding.

The gradient of the water-table in the Triassic sandstones of the north Midlands is 23 to 30 feet to the mile, that of the Oolites 30 feet and that of the Lower Greensand very variable, from 5 to 75 feet. That in the Chalk varies in different parts of the country, but over most of the north of the London Basin a figure of 15 to 25 feet to the mile is usual and in the south 40 to 60 feet to the mile. In Essex, however, it is exceptionally low and may not exceed 10 feet to the mile. The effect of overpumping is to increase this gradient to as much as 80 to 110 feet to the mile, but whether or not it is practicable to pump so heavily as to cause this gradient to be exceeded has yet to be demonstrated. In the marginal areas of the London Basin (that is, the actual catchment ground or country near to it) there is a fairly constant gradient.

From the water-contour maps we can, if we wish, prepare an alternative type of map. Instead of the contours, gradient vectors may be inserted showing the directions of water flow (at right-angles to the contours), the length of the arrows being made proportional to the gradient. Underground watersheds are then as obvious as on contour maps and may be readily compared in position with surface watersheds plotted on the map; also, areas of underground catchments may be conveniently shaded and the difference from contoured overground catchments made easily apparent. Indeed, this method of representation is most serviceable when we wish to avoid the confusion that might be caused on maps displaying other sets of contours such as those of the surface or base of the aquifer (Fig. 4), isopachytes (thickness curves), or isometric lines indicating relative hardness or salinity.

In view of the errors to which they are subject and the small variations in height of the water-table resulting from local changes in atmospheric conditions, it is doubtful whether it is serviceable to plot contours at less than 25-foot intervals even in an area where there are numerous wells; or less than 50-foot or 100-foot intervals in districts where records are sparse. Contours have sometimes been plotted at 10-foot intervals—by interpolation. The maps have then a striking appearance, but may convey an impression of greater accuracy than is actually warranted.

In many areas the contours follow generally the strike of the geological strata—which is another way of saying that water tends to travel most readily down the dip of the beds. Locally there may be variations, like promontories or embayments, which are a reflection of local topography or mark unsuspected gentle crumpling in the rocks. Should an area be heavily pumped, however, the maps do not represent standing-levels (since some of the wells may never be rested, or some rested whilst others are at work), but a position of the water-table intermediate between that of the natural surface of saturation and the true depressed table which would result from continuous pumping of all the wells at the same time. The contours may then bear no relation to the dip of the formation or the surface topography, a condition of affairs that is referred to below and illustrated in Fig. 4.

Numerous attempts have been made to estimate the resources of a district by determining the area of permeable rocks exposed at the surface in the overground catchment which may reasonably be expected to supply the underground reservoir. And this method is persisted in, although nobody knows whether or not any of the estimates come near the truth: certainly, in many cases, we know from experience that they have been too low. The method of

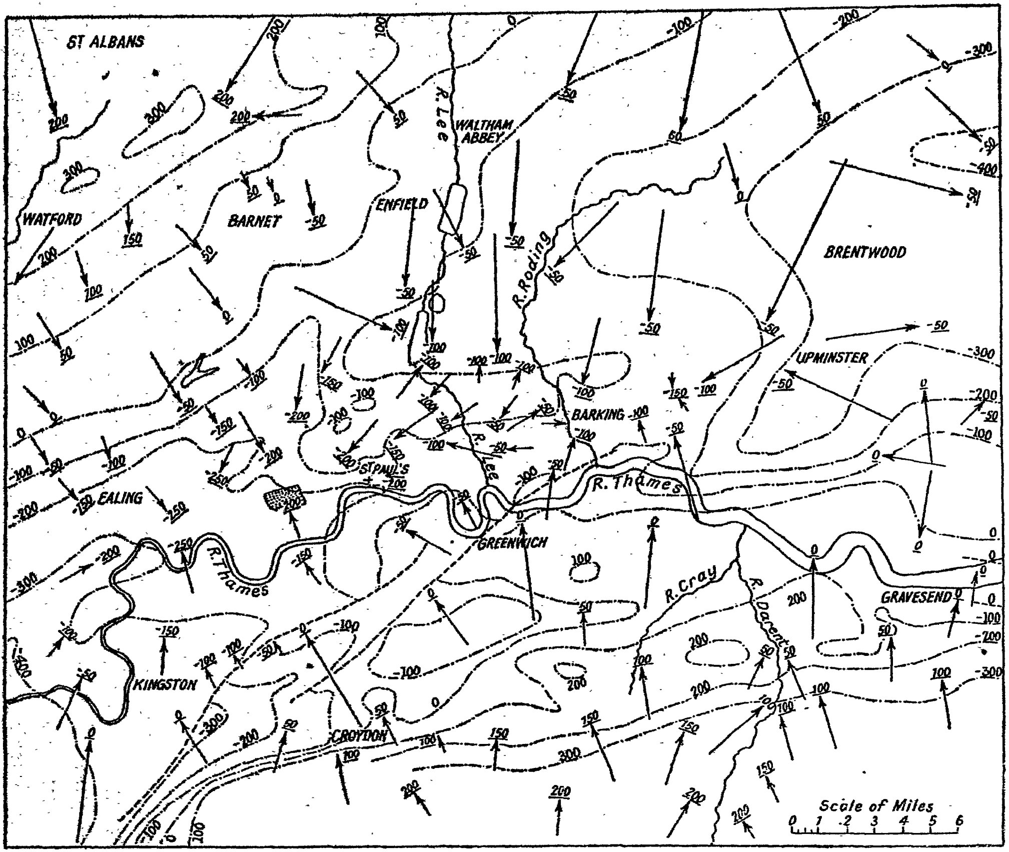

FIG. 4. — Gradients of the water-table and contours on the sub-Tertiary surface of the chalk, which follows approximately the bedding. On the north-west and south-east the natural conditions are relatively undisturbed and the flow of the underground water is still determined by the geological structure. In parts of the area (such as the City and east of Ealing) over-pumping has completely changed the normal rate and direction of flow.

Figures close to points of arrows (and underlined) show height of water-table referred to O.D. Length of arrows proportional to gradient.

arriving at a figure for the total resources has depended on two assumptions, first, that the underground catchment coincides on a map with the overground catchment, and secondly, that a particular figure for percolation, expressed in inches of rainfall per annum, is a fair one. We are here concerned only with the first assumption. The increase in the number of records of standing-level in wells and boreholes has permitted the position of the underground watersheds to be determined more or less accurately and we are now in a position to make much greater use of water-contours than we have hitherto done. They bring out at least one point forcibly, namely, that there is little justification for basing estimates on surface catchments of small streams (for example, those where the area of permeable beds amounts to 60 square miles or less) in as much as there is no secondary underground watershed corresponding with that above ground. In the case of catchments of larger streams or tributaries (where the catchment comprises 100 to 300 or more square miles) the underground and overground catchment areas may differ by so much as 10 or 15 per cent. As still larger districts come into the picture, the give-and-take between the marginal parts of the constituent areas tends to cancel out, so that the underground catchment of a large basin such as that of the River Thames, to take an actual example, may prove to be only 50 or 60 square miles less than the overground area (that is, about 1 per cent., a relatively insignificant amount). Here a belt of country 24 miles in length and 2 to 4 miles in breadth contributing to the Great Ouse drainage accounts for most of the difference.

If we cannot safely use the figures for the surface catchment areas of small streams (as, indeed, we have long realised from the fact that the actual yield from wells is frequently in excess of the postulated theoretical intake) and the water-contours fail to show underground watersheds, we may ask ourselves what use can be made of those lines for estimating the reliable yield of the area of supply of a particular undertaking. Only in very exceptional cases does a statutory area of supply bear any relation to the distribution of geological formations or water-contours. The best method of estimation seems, therefore, to be one based on an assumed delimitation of " zones " defined by boundaries drawn approximately at right-angles to the water-contours, and thus following generally the direction of flow of the ground-water. The boundaries of these zones must necessarily be arbitrary, but they can be fixed for the particular purpose of the calculation to cover the lateral extension of an undertaking's area of supply, if the total resources of that area are in question, or to the sphere of influence of a well or group of wells, if the reliable yield of such is to be determined.

Effectively, the method of dividing the area into a system of zones does not differ materially from the practice of supposing wells to be distributed over that area in such a way that it becomes in due course "saturated." If, by increasing the number of wells, the safe limit is passed, overpumping of the area is inevitable. Obviously, where water-contours form V's up valleys and show that the flow of water is being directed in such a way that the resources from the upland area are relatively depleted while those in the valley area are augmented, the spacing of wells in the upland area must be sparser, while that in the valleys can safely be made closer. Indeed, this is what has happened as a result of "trial and error" in many districts.

This procedure brings us to the thorny problem of the sphere of influence of a well or boring, that is, the distance to which the effect of pumping is felt. Several factors determine this distance —the variations in the lithic character of the geological formations from place to place, the development of fissures in certain prevalent directions, the diameter of the well or borehole and presence of adits, the intensity of the pumping (and therefore the depth of the cone of depression that it causes) and so forth. Here and there, enlightened water authorities have sunk small observation boreholes at varying distances from a pumping-station, when valuable records of the amount of depression of the water-table caused by the pumping have been obtained. At this stage, it should be said that the cone of depression (or cone of exhaustion, as it is sometimes termed) is not a true cone, for the surface is curved and is convex upwards, thus it steepens rapidly near the pumping-site, but flattens off gradually with distance. Were it not for the expense of sinking these observation boreholes and installing automatic recording apparatus, the practice would have been more widely followed. Probably, however, many more of such boreholes will be sunk in the future.

A few examples of the distance to which the effects of pumping have extended may be of interest. In the Triassic sandstones, pumping of the order of 2 to 6 million gallons a day has resulted in an observable depression up to ¾ mile, or in exceptional cases to rather over one mile. (The expression "observable depression" is used because the secular variations in water-level prevent us from relying on records which show a fall of less than 1 or 2 feet.) In Chalk, where fissure-systems are all-important, the extent of the cone varies considerably in different districts. In many areas, the effect of pumping at rates of 2 or 3 million gallons a day has not been felt for more than ½ mile, in others, up to 1 mile, and in a few exceptional districts (as, for example, those of the strongly fissured Chalk of the Lee valley) for as much as 2 or 2¼ miles. In cases where the distance is considerable, the direction of greatest influence is that of the trend of the dominant fissure-system, and the plan of the " cone " becomes, not a circle, but an elongated ellipse.

It will be evident that the cone of depression established by a large pumping-station (of 3 to 8 million gallons a day output) causes a " drawing-in " of the lines of flow of underground water and thus changes the courses of the water-contours. An ideal example can easily be sketched out. If wells, especially large wells, are too closely spaced, the cones of depression interfere with each other and a general lowering of water-level results. Thus the water-contours come to take a new and quasi-permanent pattern, as in overpumped areas.

A comparison of the water-contour maps prepared at intervals over a period of years is most illuminating. Where the demand for water is small, or where the underground supplies of a region have been developed with foresight, no appreciable change is discernible in the position of the contours (apart from small corrections resulting from increased number of data). We have therefore no reason for thinking that during the past half-century there has been any change in underground conditions detrimental to the community, but it must not be forgotten that a gradual reduction in rainfall may take time to show its effects. Where development of an area, especially industrial development, has proceeded intensively, the change in the pattern of the water-contours is strikingly revealed (*cf.* Figs. 1, 2 and 3). If water-contours are checked annually, or every few years, in the future, we are likely to be warned in time of any impending danger of overpumping; and the Minister of Health, under the new 1945 Act, has powers to take action by permitting further development of water-resources in that area only under licence. It will be desirable for his advisers to have before them these contour-maps for reference, because a falling water-level in a particular well, or several wells, is not necessarily evidence that the area is overpumped, as is often assumed, but may only be the result of increased individual activity.

Obviously, this influence of man on the water-table is but transitory so long as the rain continues to fall. No more striking example could be adduced than that of the resilience of the City of London under war-conditions. From Fig. 2 it is evident that the City area had become heavily overpumped before the war, During the war, however, damage to works and evacuation of industry and population materially reduced the quantity of water being pumped from the Chalk, with the natural consequence that the water-level recovered. The condition shown by the water-contours in Fig. 3 was then produced. It will be interesting to see, when planning schemes have become operative and " normal " life is resumed, what the contour-picture looks like in, say, 1950. The Minister of Health can only influence *new* developments ; thus it remains to be seen whether the lesson has been learned or whether the former cut-throat competition in pumping will be resumed. The one and only solution is dispersal of industry, like that envisaged in Sir Patrick Abercrombie's Greater London Plan.

If we are convinced that the use that can be made of these contour-maps warrants the time spent on their preparation, we are led to enquire how they can be rendered more accurate. Obviously, each new effort to obtain water by sinking a well gives precision to the contour-system, provided that the records are carefully kept. But such wells may remain scarce in some districts. An extension of the practice of sinking observation boreholes fitted with automatic recording apparatus is desirable in areas where data are inadequate. The cost of sinking such boreholes would appropriately fall in many districts on the Regional Water Committees.

Alternatively, now that the methods of geophysical prospecting are becoming standardised and cheapened, the depth of the water-table can be ascertained, in favourable circumstances, more speedily and at less expense. The particular method which has come into greatest favour in recent years is that of measurement of the electrical resistivity of rocks likely to be water-bearing. It has been used in America, Russia, the Colonies and to a less extent in this country, and has proved of considerable value, for it has yielded a high proportion of successful results. The claim has been made, indeed, that successes reach as much as 80 to 85 per cent. as shown by subsequent well-sinking, in the case of such aquifers as limestones, sands and gravels and igneous rocks where the water-surface is well defined. The determination of the depth of the water-table by resistivity methods in a formation such as Chalk which is fissured as well as porous is, however, a more difficult proposition, since the pores of the Chalk above the water-table are usually saturated. Resistivity work on fissured rocks must be regarded as in the experimental stage at present, and a suitable subject for research. In saturated formations the resistivity depends on the mineral composition of the water : thus the presence of salt-water, or the limit between that and ground-water, can be inferred.

Although the Water Acts do not require that water shall be analysed and the results communicated to the Geological Survey, the compulsory recording of other information relating to trial borings will inevitably lead to a considerable increase in the data of this kind so returned. From the accumulation of analyses there will result greater detail and accuracy in maps of the type recently prepared, for example, by Dr. A. W. Woodland for the Chalk of East Anglia or by Mr. A. L. Lyon for the Triassic sandstones of the Midlands, which show graphically the variation in the different chemical constituents of the water. These maps, of outstanding interest and economic importance, emphasise the need, not only for greater uniformity and detail in the returns, but also for the estimation of minor constituents hitherto neglected but now known to be necessary for, or detrimental to, health. But that is another story.

Sufficient has doubtless been said to show that in problems of underground water supply it is essential to gain all the knowledge possible about the natural routes and destination of the water. Few of us, in spirit and in truth, would join G. K. Chesterton's Noah in declaring that we " don't care where the water gets if . . ."

OUR FIRST ASTRONOMER ROYAL—THE TERCENTENARY OF JOHN FLAMSTEED

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THIS year we commemorate two of the greatest astronomical observers of all time. The tercentenary of the birth of John Flamsteed falls in August, and in December it will be four hundred years since Tycho Brahe the Dane first saw the light. The progress of practical astronomy during one of the most formative periods in its history could hardly be better illustrated than by a comparison of the scientific work of these two men, linked thus by the accident of chronology. They were both largely concerned with the same problems, but Flamsteed attained to entirely new standards of accuracy in the solution of them by employing the telescope as an instrument of precise astronomy. Again, the use of Huygens' pendulum clock by Flamsteed and his Parisian contemporaries marked a transition from essentially Alexandrian methods of observation to a procedure differing only in detail from that still employed in our observatories. A comparison of Flamsteed with Tycho Brahe would also reflect a new tendency in the organisation of scientific research. The Danish astronomer, like others before him, had established his own observatory, and had gathered round him a school of observers destined to fall apart at his death. But, as the background of the activities of Flamsteed and his successors, we see the modern observatory emerging as a permanent institution, preserving its identity under a series of directors. On this tercentenary occasion it may not be inopportune to recall some of the outstanding events of Flamsteed's career, and to set down some historical notes on the astronomical problems and methods which were his principal concern.

1

Attempts to estimate the character and the attainments of John Flamsteed were long frustrated by difficulties arising from his peculiar relations to those who should have been his best friends. Until the nineteenth century the only authentic account of him appears to have been the article in the *General Dictionary* [1]. This was based upon autobiographical memoranda of Flamsteed's, and upon other material supplied by James Hodgson, the astronomer's nephew by marriage and sometime assistant at Greenwich. However, this account was deficient in many respects, and the editors suppressed any facts likely to reflect unfavourably on Newton or Halley. Hence the article did little to correct the impression, which had been fostered by less-responsible biographers, that Flamsteed was a man of indolent disposition, pursuing his observations at his own pleasure, and unwilling to share the fruits of his labours with others.

A little over a century ago, however, there came to light in London a large collection of letters from Flamsteed to another of his former assistants, Abraham Sharp. They were edited and published by the astronomer Francis Baily, together with other Flamsteed manuscripts which he unearthed at the Royal Observatory [2]. These documents are the principal source of information concerning Flamsteed's life and work, and they presented a very different picture of his many-sided activities, pursued in the face of every kind of difficulty, and of his readiness to communicate his results to other seekers. They also threw a new, if somewhat partial, light upon the relations of Flamsteed to the Royal Society in the matter of the publication of his observations; and this gave rise to some controversy, in which Baily supported Flamsteed's complaint against Newton and Halley, while their conduct was defended by William Whewell [3].

Further letters, to the number of seventy, which Flamsteed wrote between 1673 and 1687 to his friend Richard Towneley (often confused with his uncle, Christopher Towneley) are preserved by the Royal Society. These deal mainly with the technicalities of his work at Derby and at Greenwich; and an account of their contents was given in 1922 by J. L. E. Dreyer [4]. More recently, we have had a brief, documented study of Flamsteed from the pen of Mr. E. F. MacPike [5].

2

John Flamsteed was a north-countryman, born on August 19, 1646, at the village of Denby, between Derby and Sheffield. His parents were of yeoman stock and in comfortable circumstances. What schooling the boy had was on the usual classical lines; but his education was interrupted by the constitutional ill-health which was to pursue him through life. Thrown on to his own intellectual resources, Flamsteed developed a growing interest in mathematics and astronomy. His father taught him arithmetic; and from such books as the *Sphæra* of Sacrobosco (a thirteenth-century manual repeatedly re-edited down to the seventeenth century) and Thomas Streete's *Astronomia Carolina*, he learned, for example, how to calculate the places of the planets, and to predict the times of occurrence of eclipses. Like Tycho Brahe in his day, he was not slow to discover how deficient were the current astronomical tables, and how widely they departed from the results of the observations which he carried out with a rude quadrant of his own construction. At that period there was a considerable demand for almanacs giving particulars of the celestial events of the ensuing year. In 1669 Flamsteed prepared such an ephemeris for the year 1670, and he sent it, by the hand of a friend, to Lord Brouncker, the President of the Royal Society. His communication was acknowledged by the Secretary, Henry Oldenburg, and it was published in the *Philosophical Transactions*. The episode led to an exchange of letters with Oldenburg and with John Collins, and early in 1670 Flamsteed travelled to London to visit these correspondents. Collins took him to the Tower to see Sir Jonas Moore, Surveyor-General of the Ordnance and a Royal Society mathematician, who presented him with "Mr. Towneley's micrometer," and promised to procure for him some lenses with which to make a telescope [6].

This micrometer was an instrument used in conjunction with a telescope, and serving to measure small celestial angles such as those subtended by the diameters of the planets. Such a device seems to have been employed for the first time about 1640 by the English astronomer William Gascoigne. After his death at Marston Moor, several of Gascoigne's micrometers came into the hands of Richard Towneley [7]. Working on hints in Gascoigne's papers, and aided by a local watchmaker, Towneley produced improved models of the instrument; and it seems to have been one of these that Moore now gave to Flamsteed. (A similar device was independently introduced in 1666 at the Paris Observatory.) Flamsteed learned much about the technique of micrometric work while on visits to the Lancashire home of Richard Towneley in 1671 and 1672. While staying there he also saw a "weather-glass" (barometer). He made one for himself, and subsequently gave one to Sir Jonas Moore; he, in turn, presented "weather-glasses" to the King and the Duke of York, together with Flamsteed's rules for foretelling the weather from their behaviour. Moore often talked about the young astronomer to the royal brothers, which "procured me more than ordinary regards from them" [8].

In 1671, Moore having obtained for him the necessary lenses, Flamsteed constructed a telescope and "set up a pole to raise my glasses, at Derby" [9], as was customary in those days. Having no pendulum clocks, "they being not common in the country at that time" [10], he limited himself to observations not requiring an accurate knowledge of time, such as measurements of the angular diameters of the Moon and planets and of the elongations of the satellites from Jupiter. He found that the variations in the Moon's angular diameter were best represented by the lunar theory of Horrocks; and he calculated the tables which were published in the latter's posthumous works [11].

In 1670, Flamsteed had entered himself at Cambridge University. Here he made the acquaintance of Newton; but of any other associations with the University there is little record. He proceeded to the M.A. degree *per literas regias* in 1674, and soon afterwards he took Orders, purposing to devote his life to the service of some country parish.

3

About this time plans were being discussed for founding an observatory under the auspices of the Royal Society to match the one lately established in Paris (1672) as an offshoot of the *Académie des Sciences.* In December 1674, Moore summoned Flamsteed to London with the intention of placing him in charge of the proposed institution. While Flamsteed was hesitating, and alternative sites for the observatory were being considered, an event occurred which (he afterwards declared) "hastened, if it did not occasion, the building of the Observatory" at Greenwich [12].

A Frenchman who called himself Le Sieur de Saint-Pierre, and who was backed by the interest of the Duchess of Portsmouth, put forward a claim to have solved the problem of finding the longitude at sea. As is generally known, an observer's longitude from a prime meridian is determined by comparing, at some given instant, the *local* time with the *standard* time corresponding to that meridian. The difference of the two times measures the longitude of the observer east or west of the prime meridian. The local time is easily ascertained; but methods of arriving at the standard time continued to be faulty until the invention of the chronometer in the eighteenth century solved this most urgent problem. Now the Moon, in its monthly circuit, appears to move fairly rapidly through the sky, and Saint-Pierre (anticipating a method which later became practicable) proposed to obtain the standard time from the predictable positions of the Moon as it travelled like the hand of a clock over the background of the fixed stars. A Royal Commission was set up to consider the feasibility of the plan, and, on the recommendation of Moore, Flamsteed was included among the Commissioners. He convinced his colleagues that the scheme must prove impracticable because of inaccuracies both in the current lunar tables, and in Tycho's star-catalogue; moreover, even on theoretical grounds, the particular procedure proposed was inferior to a method of "lunar distances" previously suggested by the French astronomer Morin. "My letters being shown King Charles," wrote Flamsteed in his account of the episode, "he startled at the assertion of the fixed stars' places being false in the catalogue; said, with some vehemence, 'He must have them anew observed, examined and corrected, for the use of his seamen'; and further (when it was urged to him how necessary it was to have a good stock of observations taken for correcting the motions of the Moon and planets) with the same earnestness 'he must have it done.' And when he was asked 'Who could, or who should, do it?' 'The person (says he) that informs you of them!' Whereupon I was appointed to it," etc. [13]. On March 4, 1675, Charles II issued a warrant appointing Flamsteed "Our Astronomical Observator" at a salary of £100 *per annum.*

Moore had originally proposed to house the new observatory in Chelsea College, which was the property of the Royal Society; later he was inclined to establish it in Hyde Park; but Sir Christopher Wren's choice of a site on Greenwich Hill eventually prevailed. The construction of the Observatory was sanctioned by a warrant dated June 22, 1675. The foundations were laid in August 1675, and the building was completed and Flamsteed installed by the following July.

4

When Flamsteed commenced his labours at Greenwich Observatory, the only instruments he found there were a sextant and two clocks, and these were a gift from Sir Jonas Moore. The sextant was not the nautical instrument that we understand by the term, though it fulfilled the same purpose of measuring celestial angles, and it may serve to typify the contemporary instruments of precise astronomy. It consisted essentially of an iron sector of 60 degrees, 7 feet in radius, and having an arc of brass graduated in angular measure. A moving radius carrying a telescopic sight turned about the centre of the sector, and could be clamped at any desired setting on the graduated arc. The instrument could be fixed with its plane passing through any two points whose angular separation was required, say two stars; the moving radius was sighted upon each in turn, and its setting was read in each position; the difference of the two readings then gave the angle subtended by the two stars at the observer's eye. This type of instrument dated, in principle, from ancient times, though latterly modified and perfected by Tycho Brahe and Hevelius. The use of telescopic in place of open sights had been established by the Parisian observers about 1668 (though here, too, they seem to have been anticipated by William Gascoigne).

As a particular case of this type of instrument, the graduated arc could be permanently fixed in the meridian by attaching it to a mass of masonry so as to form a *mural arc*. By means of such an instrument, employed in conjunction with a pendulum clock, the Parisian astronomers had already begun to deduce the right ascensions and declinations of selected stars from the times and altitudes of their transits across the meridian, as is done to-day with a transit circle. Flamsteed felt the need of such an instrument to provide the absolute measurements which were to form the basis of his star-catalogue; and he determined that the graduated arc should extend over 140 degrees, so as to take in all stars transiting between the south horizon and the Pole in the latitude of London. His first attempt to construct a mural arc was hampered by lack of funds, and the result was disappointing. However, his circumstances improved following his presentation to a Church living and the death of his father, and in 1688 he embarked on the construction of a second mural arc, assisted by his friend Abraham Sharp. The work occupied fourteen months and cost Flamsteed £120. The two clocks at the Royal Observatory were the handiwork of the celebrated horologist Thomas Tompion. They had 13-foot pendulums, and were corrected by taking the altitude of the Sun or of a star with the small quadrant which Flamsteed had brought with him from Derby.

5

Saint-Pierre's scheme for finding the longitude had failed for want of an accurate star-catalogue and lunar table; and it was chiefly to remedy these two deficiencies that Flamsteed laboured for forty-five years at Greenwich Observatory. For his principal tasks were, first, the construction of a star-catalogue destined to supersede all existing ones in scope and precision, and, secondly, the systematic observation of the Sun, Moon and planets;

leading to the correction of the tables that gave their apparent motions.

Catalogues giving the positions of stars upon the conventional celestial sphere have always been of fundamental importance in astronomy. For the stars constituted the system of reference to which the earlier astronomers directly referred the positions of the Sun, Moon and planets. And since the introduction of the pendulum clock, an accurate knowledge of the positions of standard stars is required for the fundamental reckoning of time, which in turn is involved in determining the positions of other celestial objects.

The best star-catalogue in Flamsteed's day was that of Tycho Brahe (published in 1602), which was constructed without the aid of a telescope and which contained only about a thousand star-places. Flamsteed's catalogue, like Tycho's, was based upon certain standard stars, of which he determined the absolute co-ordinates by means of his mural arc and pendulum clock. The places of the secondary stars of his catalogue were then deduced from their "intermutual distances," that is, from their angular separations from one another and from the standard stars, measured by means of the sextant.

Flamsteed's catalogue comprised close on 3,000 stars visible at Greenwich. To these were subsequently added the places of some 300 others of the southern celestial hemisphere, determined by Edmond Halley in the course of his youthful scientific expedition to the island of St. Helena (1676–78). The accuracy of the catalogue naturally varied from star to star; but, broadly speaking, while Tycho's star-places might be several minutes out, Flamsteed reduced the errors to a matter of seconds.

6

Flamsteed was in the habit of observing the places of the Sun, Moon and planets not less assiduously than those of the fixed stars. His results in this department could not be expressed in such a concrete form as the catalogue of stars, but they led to improvements in the tables giving the motions of these bodies. He supplied many of these observations (including upwards of 150 of the Moon) to Newton, who was trying to improve his lunar theory in preparation for a second edition of the *Principia*. The determination of the absolute co-ordinates of celestial bodies required a prior knowledge of the latitude of the observatory, the obliquity of the ecliptic and the position of the vernal equinoctial point. To determine this last quantity, Flamsteed devised a procedure which still bears his name, and which represents one of his most notable contributions to practical astronomy.

In Flamsteed's day, the debate concerning the status of the Earth in the Universe (which had so deeply concerned Tycho Brahe) had virtually ended, at least in Protestant England, with the triumph of the heliocentric theory. Flamsteed showed little concern for such questions, being much more interested in the practical part of astronomy. However, in 1694, his refined observations revealed what he took to be evidence of the annual stellar parallax so long and so eagerly sought by astronomers as proof of the Earth's revolution round the Sun. While comparing the angular distances of the pole-star from the true pole, measured at various times of the year, he noticed that this distance appeared greater " in March, April, July, August and September, than in December. . . . This is the first time, I am apt to think, that any real parallax hath been observed in the fixed stars " [14]. It was reserved for Bradley, in the eighteenth century, to rediscover the phenomenon, and to explain it by reference to the aberration of light—itself a verification of the Copernican theory.

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In his later years, Flamsteed became involved in an unfortunate dispute with Newton and Halley and other members of the Royal Society over the publication of the Greenwich observations. The episode had no lasting effect upon the progress of science, and we shall not enter at length into the complicated history of it. Probably the quarrel took its rise out of the mere mutual incompatibility of the disputants ; but it came to turn upon the accusation that Flamsteed was excessively tardy in publishing the results of his work. And it became hopelessly embittered following the production, without the astronomer's knowledge or consent, of an imperfect and partly spurious edition of his results. Much of the blame for the misunderstanding may fairly be laid upon the adverse and anomalous conditions under which Flamsteed laboured. On his behalf it must be stressed that the Government neither provided him with any astronomical instruments nor, apart from his small salary (sometimes in arrears), with any funds to meet the cost of making and repairing such equipment, or of engaging a computer. He was thus involved in the expenditure of some £2000 of his own fortune, and he was obliged to supplement his income by taking pupils. Under such circumstances, it is not surprising that he claimed the right to judge for himself when his work should have reached a sufficient degree of perfection to justify him in publishing it, as he had always intended to do. On the other hand, it could be argued that Flamsteed was a Government official, and that his observations were State property, to be published expeditiously for the common good. Newton, especially, had an urgent personal motive for pressing the one man in the world who could give him the data he needed for verifying and refining his gravitational theory before his own years should run out. In this connection it was unfortunate that Flamsteed, as we have already remarked, never seems to have shown much interest in the wider problems of cosmology, or much comprehension of what Newton had really achieved in that field.

When, in 1704, plans were at length made for printing the Greenwich observations, Prince George of Denmark, the Queen's Consort, undertook to defray the expenses. Flamsteed was persuaded to hand over to a Committee dominated by Newton an imperfect set of star-places, not for publication, but as a guarantee that the complete catalogue would be forthcoming. There were further delays on both sides, and, towards the end of 1708, Prince George died. The scheme should then have lapsed; but the Committee being anxious to complete the publication, they entrusted to Halley the task of bringing out an edition without waiting for Flamsteed to supply the rest of the copy. This edition appeared in 1712 [15]. It contained the sextant observations, supplied by Flamsteed and already printed, but also the imperfect star-catalogue deposited in 1704, which was completed by data supplied by Halley; the fundamental observations were also given in a curtailed and rearranged form which seriously reduced their scientific value.

However, the political changes resulting from the death of Newton's patron, Halifax, in 1715, afforded Flamsteed the opportunity of securing the unsold copies of the 1712 edition. From these, he separated the pages containing the sextant observations; as for the remainder, "I made a *Sacrifice of them to Heavenly Truth*" [16]. Flamsteed now set about publishing his life-work in his own way and at his own expense. But he did not live to finish the task, for he had not quite completed the second volume when, on December 31, 1719, he died at Greenwich, after a few days' illness. The completion and the publication of the whole work was due to the (unrewarded) labour and self-sacrifice of Flamsteed's friends and sometime assistants Joseph Crosthwait and Abraham Sharp. The book was published in 1725 under the title of *Historia Cœlestis Britannica.* Of its three folio volumes, the first contains Flamsteed's early micrometric measurements (with those of Gascoigne and Crabtree) and his Greenwich observations with the sextant. The second volume contains the main bulk of the Greenwich entries; and the third volume, the star-catalogue, together with an account of Flamsteed's methods and instruments.

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The achievements of John Flamsteed are not to be sought in spectacular discoveries and inventions of immediately obvious significance. He lacked the versatility of genius which enabled many of the leaders of that great age to range over wide fields of scientific speculation and enquiry. Nor did he show that fertility in the design of new types of instruments which characterized his great contemporaries Picard and Roemer. What he could and did do was to establish precise astronomy upon vastly more secure foundations, destined to endure for many years after his death, and then to be superseded only through the labours of an inheritor of his tradition and a successor to his office—James Bradley. Yet perhaps Flamsteed's greatest contribution to the progress of astronomy consisted less in the mere results he obtained than in the exemplary standards of observation which he did so much to establish. His attention to the technical *minutiæ* upon which accuracy depends, his perseverance in continuous and systematic observation, his insistence that only the most refined results obtainable are good enough to publish—these are some of the traits which constituted at once a notable example to his successors, and an expression of the essential spirit of the man himself. That the Royal Observatory ever survived its neglected infancy to enter upon a splendid career of usefulness to all mankind is due in largest measure to the lifelong labour and the enduring inspiration of the great astronomer whose memory we salute to-day.

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THE WALDEN INVERSION

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I. INTRODUCTION

FIFTY years have elapsed since the discovery by Walden of the configurational inversion which bears his name, and an extensive literature bearing on the problem has accumulated. Most of the earlier work on the subject served mainly to establish the phenomenon in a large variety of instances, and it is only in comparatively recent years that a rational explanation has emerged. It is opportune to review the position in the light of recent developments.

First, we shall recall the background to Walden's work and the significance of his discovery. We shall then proceed to discuss the factors which delayed progress in the elucidation of the problem. Finally, we shall consider the advances which have led to a solution of the difficulties and to a fuller understanding of the phenomenon.

At the time of Walden's great contribution to our knowledge of the steric course of reactions, the theory of the tetrahedral carbon atom and of optical isomerism was firmly established. In a substitution reaction of the type CabcX → CabcY there was, however, no evidence regarding the steric course of the change; it was generally assumed that the new substituent would replace the extruded atom or group directly, *i.e.* without a configurational change. One can therefore imagine the interest caused in the chemical circles of that time when Walden reported (*Ber.*, 1895, **28**, 1287, and subsequent papers) that an optically active compound CabcX could occasionally be converted into its enantiomorph by means of simple substitution processes involving the replacement and reinstatement of X, *e.g.* *l*-CabcX → *d*-CabcY → *d*-CabcX.* Clearly, a configurational change had occurred in one of the two substitution processes. The designation "Walden Inversion" was first used to describe such a change by Emil Fischer (*Ber.*, 1906, **39**, 2894).

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* Throughout this paper the symbols *d* and *l* signify sign of rotation.

We shall end this section with a quotation which well describes the unexpected nature of the observation and the importance to be attached to it, and which hints at the difficulty experienced with regard to an explanation of the phenomenon: "Walden's discovery was certainly a puzzle and a surprise, for it did not fit into any pre-existing theory of optical activity, and had not been anticipated as a corollary to van't Hoff's theorem of the asymmetric carbon atom. It is highly probable that it may mark an epoch in our views with regard to the mechanism of the process of substitution in general" (Frankland, *J.C.S.*, 1913, **103**, 713).

II. DETERMINATION OF CONFIGURATION

A difficulty which has greatly retarded progress in the study of the Walden Inversion is that of relating sign of rotation to configuration in reactants and products. Since the sign of rotation of a compound depends on the nature of the four groups attached to the asymmetric carbon atom, the reactant and the product in the substitution reaction $\text{CabcX} \longrightarrow \text{CabcY}$ may have opposite signs of rotation even when no configurational change occurs in the replacement.

For the purpose of demonstrating the occurrence of inversion two methods have been repeatedly employed. The first involves the conversion of an optically active compound into its enantiomorph by means of two or more reactions, *e.g.*

$$l\text{-Chlorosuccinic acid} \xrightarrow{\text{Ag}_2\text{O}} l\text{-Malic acid} \xrightarrow{\text{PCl}_5} d\text{-Chlorosuccinic acid.}$$

(Walden, *Ber.*, 1896, **29**, 133)

The second method depends on the conversion of an optically active compound into enantiomorphous derivatives by the use of different reagents, *e.g.*

$$l\text{-Chlorosuccinic acid}\begin{cases}\xrightarrow{\text{Ag}_2\text{O}} l\text{-Malic acid}\\ \xrightarrow{\text{KOH}} d\text{-Malic acid}\end{cases}$$

(Walden, *Ber.*, 1897, **30**, 3146)

$$\begin{tikzcd}
l\text{-}\alpha\text{-Bromopropionic acid} \arrow[r, "\text{Ag}_2\text{O}"] \arrow[d] & d\text{-Lactic acid} \\
l\text{-}\alpha\text{-Bromopropionyl glycine} \arrow[r, "\text{Ag}_2\text{O}", "\text{and Hydrolysis}"'] & l\text{-Lactic acid}
\end{tikzcd}$$

(Fischer, *Ber.*, 1907, **40**, 489)

In the last example, the formation of the bromopropionyl glycine ester and the hydrolysis of the lactyl glycine ester do not involve the exchange of a bond directly attached to the asymmetric carbon atom and cannot therefore give inversion; it follows that the bromo-acid and its ester behave differently with regard to the stereochemical course of their reactions with silver oxide. Taken collectively, the examples show that the stereochemical result may depend on the substituting agent and the compound substituted.

Although these and many similar results clearly establish the occurrence of inversion, two (or more) reactions are needed to provide the demonstration and, owing to the difficulty of relating rotation to configuration, it is impossible, in the absence of additional data, to say with certainty where the inversion occurred, much less how it occurred. The identification of the configurational change with any one stage in the first method, or with any one reagent in the second, has been the subject of much speculation (Armstrong, *J.C.S.*, 1896, **69**, 1399; Walden, *Ber.*, 1899, **32**, 1833; Fischer, *loc. cit.*; Gadamer, *Chem. Z.*, 1912, **36**, 1327; Biilmann, *Annalen*, 1912, **388**, 330; Bancroft and Davies, *J. physikal Chem.*, 1931, **35**, 1624; and many other authors).

Many methods have been devised to relate rotation to configuration and to identify an inversion with a specific reaction. The first method, as distinct from mere speculation, is due to Frankland (*J.C.S.*, 1913, **103**, 725). This author based his assignments of relative configurations largely on a statistical survey of the recorded optical effects of particular reagents, making a circumspect use of the rule that, when in a majority of examples a particular reagent causes retention or inversion of the sign of rotation, then the action of that reagent is characterised by retention or inversion of configuration as the case may be. The rule depends on the circumstance that, although a change of sign of rotation may accompany a reaction proceeding with retention of configuration, the opposite is more often the case. Frankland's method served as a useful general guide, but we now know that the same reagent may behave in stereochemically different ways towards different compounds, and even towards the same compound under different conditions (*cf.* later sections of this paper).

Another method of determining the relative configurations of optically active compounds was introduced by Clough (*J.C.S.*, 1914, **105**, 49; 1915, **107**, 96, 1059; 1918, **113**, 526; 1926, 1674). It depends on an assumption that compounds of similar configuration undergo analogous changes in rotation under the influence of added salts, or of changes in wave-length, concentration, temperature and solvent. Many erroneous conclusions are known to have resulted from the application of this method (*cf.* Kuhn and Wagner-

Jauregg, *Ber.*, 1928, **61**, 481, 504; Cowdrey, Hughes, Ingold, Masterman and Scott, *J.C.S.*, 1937, 1252) but it appears to have been successful in the hands of Freudenberg and others who have made a thorough investigation of its limitations (*Ber.*, 1923, **56**, 193; 1927, **60**, 2447; 1928, **61**, 1083; 1931, **64**, 708).

Methods of a more mathematical nature have also been developed. The aim of these methods is to relate the optical rotatory power of a compound to the configuration and properties of its constituent atoms or groups. The theory is complex and the rigorously derived formulæ are such that it is almost impossible to apply them with any degree of certainty to existing data, since they involve parameters which cannot in general be related to any readily measurable properties of the molecule (see, especially, Born, *Proc. Roy. Soc.*, A, 1935, **150**, 84; Boys, *ibid.*, 1934, **144**, 655, 675; Lowry, *Nature*, 1935, **136**, 191; Kirkwood, *J. Chem. Physics.*, 1937, **5**, 479). With the aid of certain assumptions and approximations, Boys (*loc. cit.*) deduced an expression for the rotatory power which contains only the refractivities and effective radii of the radicals. Its application to the determination of the absolute configuration of molecules is illustrated by the following rules: "A dextro-compound has the configuration such that, when the largest group is nearest to the hypothetical observer, the other groups in order of diminishing size appear in a clockwise rotation. . . . If one group can be changed while the compound retains the same configuration, the sign of the rotation should change when the effective radius is increased above that of the next largest group." It is inferred that these simple rules are applicable only when the radicals are small and can be regarded as approximately spherical. In the case of larger, more complex radicals, difficulties arise with regard to the definition of " size " and " effective radius." Even with this reservation, it would appear that undue prominence is given to the size of the groups in Boys' treatment. The extensive work of Rule and others indicates the importance of factors such as polarity in determining the sign and magnitude of rotatory power (see especially *Trans. Faraday Soc.*, 1930, **26**, 321, 337). Born (*loc. cit.*) has pointed out that Boys' derivations are seriously at variance with the rigorously established formulæ of the general theory, and that they imply certain relationships which are demonstrably invalid. The application of the simple rules (quoted above) to comparatively complex radicals often leads to conclusions which are not in accordance with the facts (see Hughes, *Trans. Faraday Soc.*, 1938, **34**, 202). At the present stage of its development, it would appear that this method is not particularly useful in its application to the problem under discussion.

An important advance in connection with the problem of relating rotation to configuration has been made by Kenyon and Phillips and their co-workers. The method may be illustrated by a consideration of the following reactions ($R' = p\text{-}CH_3.C_6H_4$):

$$\begin{tikzcd}
 & d\text{-}C_6H_{13}.CHMe.OH \arrow[r, "R'SO{-}Cl", "(1)"'] & d\text{-}C_6H_{13}.CHMe.OSOR' \arrow[d, "O", "(2)"'] & \\
 & & d\text{-}C_6H_{13}.CHMe.OSO_2R' \arrow[dl, "\overline{OAc}"', "(4)"] \arrow[dr, "(5)", "\overline{Cl}"'] & \\
l\text{-}C_6H_{13}.CHMe.OH \arrow[r, "Ac_2O", "(3)"'] & l\text{-}C_6H_{13}.CHMe.OAc & & l\text{-}C_6H_{13}.CHMe.Cl
\end{tikzcd}$$

(Houssa, Kenyon and Phillips, *J.C.S.*, 1929, 1700)

It is argued that of reactions (1), (2), (3) and (4), one (if not three) must involve inversion, and since the first three do not, in all probability, exchange a bond attached to the asymmetric carbon atom, reaction (4) must be accompanied by configurational change. It is now assumed that the reaction of the organic sulphonate with chloride ions in ethyl alcohol would proceed by a similar mechanism, and, like the reaction with acetate ions in the same medium, would be accompanied by an inversion of configuration. It then follows from the observed rotations that *sec.*-octyl alcohol and chloride of like sign of rotation have corresponding configurations.

A number of conversions of this type have been carried out by Kenyon and Phillips and their collaborators, and many configurational relationships have thus been established (*cf.* especially Kenyon and Phillips, *Trans. Faraday Soc.*, 1930, **26**, 451; Kenyon, Phillips and Taylor, *J.C.S.*, 1933, 173; Kenyon, Phillips and Pittman, *J.C.S.*, 1935, 1072). Although the method may not always be completely rigid, it constitutes an important advance in the attack on the problem of the Walden Inversion. The possible weakness in the above example lies mainly in the unproved assumption of an analogy of behaviour between acetate and halide ions. Aided by detailed considerations of mechanism, as disclosed by reaction kinetics, the method could be made the basis of a rigid argument (as will be obvious from the discussion of mechanism contained in a later section). Part of Kenyon's argument receives support from work of an independent kind, which points to a difference of behaviour in the reactions of organic sulphonates and carboxylates, bond fission usually taking place as follows: $R|{-}O{-}SO_2R'$ and $R{-}O{-}|COR'$ (carboxylic esterification is generally similar, *e.g.* R—O—|H + X—|COR′ → R—O—COR′ + HX ; X = Hal., OH, O.COR., etc.) (*cf.* Lapworth, *J.C.S.*, 1912, **101**, 273 ; Ingold and Ingold, *ibid.*, 1932, 756 ; Polanyi and Szabo, *Trans. Faraday Soc.*, 1934, **30**, 508). Owing to the difference of behaviour described, alkyl sulphonates are usually alkylating agents while carboxylates are acylating agents. It should be emphasised that we refer to the most common behaviour ; exceptions may occur in special structures (see, especially, Kenyon, Partridge and Phillips, *J.C.S.*, 1936, 85 ; 1937, 216 ; Cowdrey, Hughes, Ingold, Masterman and Scott, *ibid.*, 1937, 1264 ; Hughes, Ingold and Masterman, *ibid.*, 1939, 840 ; Day and Ingold, *Trans. Faraday Soc.*, 1941, **37**, 686 ; Balfe and Kenyon, *ibid.*, p. 721).

A recent method of overcoming the difficulty of relating the sign of rotation of the reactants and the products of a replacement reaction to their configuration has been based on a study of the kinetics and mechanism of substitution. A discussion of this topic is contained in the following pages.

III. REACTION MECHANISM AND THE WALDEN INVERSION

In the last section we discussed one of the factors which retarded progress in the study and understanding of the Walden Inversion, namely, the difficulty of relating sign of rotation to configuration in the reactants and the products of a substitution reaction involving the replacement of an atom or group directly attached to the asymmetric centre. A second difficulty was a lack of sufficient knowledge concerning the mechanism of substitution. It is obvious that the conditions which control the steric orientation of substitution must be intimately dependent on the reaction mechanism and it is only within recent years that this has been fruitfully studied in aliphatic compounds. We cannot deal effectively with developments along these lines without first having recourse to a brief description of the advancement of knowledge concerning the basic subject of the mechanism of substitution at a saturated carbon atom, which we now give in outline below.

A. *The Mechanism of Substitution at a Saturated Carbon Atom*

In the early days of the development of chemical theories, the main question concerning the mechanism of substitution processes was put in the following form : " Is substitution always preceded by addition ? " In a reaction at a saturated carbon atom, of the form Y + R—X → Y—R + X, where one bond is formed and one bond is broken, it is obvious that the process could be initiated either by the attack of the reagent (Y) on the molecule R—X, a process tending to form the new bond $Y{-}R$ first and leading to the expulsion of X, or, alternatively, by the preliminary fission of the $R{-}X$ bond, followed by the union of Y with R:

First Hypothesis: $Y \cdots R{-}X \rightarrow Y{-}R + X$
Second Hypothesis: $R{-}X \rightarrow R + X$; $Y + R \rightarrow Y{-}R$.

Thus there emerged two types of theories of substitution reactions. One postulated addition as an essential condition for the expulsion of the group replaced; the other assumed that the primary dissociation of bonds was involved. In the early versions of the first type of theory, which may be called "the addition-complex theory," it was not clear how the addition took place; it was customary to speak rather vaguely of a union dependent on the "residual affinity" of the carbon atom, and so on, and it was obvious that there were difficulties regarding the valency of this carbon atom, which was already saturated before the attachment of the entering atom or group. These difficulties were dispelled by the idea that addition is not previous to but synchronises with dissociation. This was first stated in an acceptable form by G. N. Lewis (*Valence and the Structure of Atoms and Molecules*, 1923, p. 113). The physical basis of the synchronous mechanism has been discussed by Polanyi and by Olson (see later references (below) to the work of these authors). In the second type of theory also, development has taken place in the direction of greater precision in its formulation; the theory has evolved, from a stage at which little attempt was made to explain how the replaced group was expelled, to its position at the present time, when we distinguish between various types of fission (*cf.* Hughes, *Trans. Faraday Soc.*, 1941, **37**, 604). The impression that the two types of theories were incompatible (in the sense that no degree of application could be given to both) has also undergone revision. The concept of the duality of mechanism in substitution reactions at a saturated carbon atom has in fact become the main feature of recent developments, which we shall now describe.

The substitution reactions with which we shall be mainly concerned are of the "Nucleophilic" type (labelled S_N for convenience in reference; Ingold, *Chem. Reviews*, 1934, **15**, 225; Hughes and Ingold, *J.C.S.*, 1935, 244), *e.g.*

$$Y\!: + R\,\vdots{-}X \rightarrow Y{-}R + :X \quad (S_N)$$

As indicated by the position of the dotted line in the representation, this form of substitution is characterised by the condition that there is a transfer of electrons (represented by dots in the formulation) from Y to R and from R to X. The reagent may therefore be either negative or neutral (provided it has unshared electrons) and the replaceable group may be either neutral or positive. Four cases thus arise, which may, for the most part, be illustrated by examples of familiar reactions in organic chemistry:

(1) Y negative and X neutral, *e.g.* $\overset{-}{\mathrm{HO}} + \mathrm{R—Br} \longrightarrow \mathrm{HO—R} + \overset{-}{\mathrm{Br}}$

(2) Y negative and X positive, *e.g.* $\overset{-}{\mathrm{Br}} + \mathrm{R—}\overset{+}{\mathrm{N}}\mathrm{R_3} \longrightarrow \mathrm{Br—R} + \mathrm{NR_3}$

(3) Y neutral and X neutral, *e.g.* $\mathrm{R_3N} + \mathrm{R—Br} \longrightarrow \mathrm{R_3}\overset{+}{\mathrm{N}}\mathrm{—R} + \overset{-}{\mathrm{Br}}$

(4) Y neutral and X positive, *e.g.* $\mathrm{R_3N} + \mathrm{R—}\overset{+}{\mathrm{S}}\mathrm{R_2} \longrightarrow \mathrm{R_3}\overset{+}{\mathrm{N}}\mathrm{—R} + \mathrm{SR_2}$

The examples refer to (1) the hydrolysis of alkyl halides, (2) the decomposition of alkylammonium salts, (3) the formation of alkylammonium salts from tertiary amines and alkyl halides, (4) the formation of alkylammonium cations from tertiary amines and alkylsulphonium cations.

It has been shown that there are two distinct mechanisms of this form of substitution (*cf.* Hughes, Ingold and co-workers in papers published since 1933). The first is a one-stage mechanism, involving a synchronous electron transfer from Y to R and from R to X, *e.g.*

$$\mathrm{Y:} + \mathrm{R—X} \longrightarrow \mathrm{Y—R} + \mathrm{:X}\ (\mathrm{S_N2})$$

$$(\textit{Example}:\ \overset{-}{\mathrm{HO}} + \underset{\substack{\text{Initial}\\ \text{State}}}{\mathrm{R—Cl}} \longrightarrow \overset{\sim\frac{1}{2}-}{\mathrm{HO}}\cdots\cdots\underset{\substack{\text{Transition}\\ \text{State}}}{\mathrm{R}}\cdots\cdots\overset{\sim\frac{1}{2}-}{\mathrm{Cl}} \longrightarrow \underset{\substack{\text{Final}\\ \text{State}}}{\mathrm{HO—R} + \overset{-}{\mathrm{Cl}}})$$

Since both reactants are simultaneously involved, this mechanism is termed " bimolecular " ; the numerical part of the designation ($\mathrm{S_N2}$) is intended to symbolise this character. As an example, we may take the bimolecular alkaline hydrolysis of an alkyl halide, in which instance the anionic charge, initially on the entering group and finally on the expelled group, becomes distributed in the transition state, as indicated in the schematic representation above.

The second mechanism is a two-stage process, wherein a rate-determining ionisation is succeeded by a reaction between the carbon cation formed and the substituting agent, *e.g.*

$$\left.\begin{aligned} \mathrm{R—X} &\xrightarrow[\text{(slow)}]{} \overset{+}{\mathrm{R}} + \mathrm{:X} \\ \mathrm{Y:} + \overset{+}{\mathrm{R}} &\xrightarrow[\text{(rapid)}]{} \mathrm{Y—R} \end{aligned}\right\}\mathrm{S_N1}$$

$$(\textit{Example}:\ \mathrm{R—Cl} \longrightarrow \overset{\delta+}{\mathrm{R}}\cdots\cdots\overset{\delta-}{\mathrm{Cl}} \longrightarrow \overset{+}{\mathrm{R}} + \overset{-}{\mathrm{Cl}}\ \text{(slow), followed by}$$
$$\overset{+}{\mathrm{R}} + \overset{-}{\mathrm{OH}} \longrightarrow \mathrm{R—OH}\ \text{(rapid)}).$$

In the unimolecular hydrolysis of an alkyl halide, for example, there is a partial separation of charges in the transition state of the rate-determining ionic dissociation, which is followed by a rapid reaction between the carbon cation and a hydroxide ion. Although the importance of solvation in ionic dissociation is recognised, this mechanism is termed " unimolecular " because only one reactant, R—X, is undergoing co-valency change in the rate-determining stage. The numerical part of the designation S_N1 again indicates the molecularity of this mechanism.

Clearly, the two mechanisms may be characterised by their distinctive kinetic requirements and this, together with other significant properties, is extremely useful for purposes of diagnosis. In illustration of the kinetic evidence, we may mention one set of observations, which refer to the hydrolysis of alkyl halides with alkali in aqueous alcohol. For certain halides (*e.g.* the methyl halides) the reaction is of the second order, the rate being dependent on the concentration of the alkyl halide and on the concentration of hydroxide ions. The hydrolysis of certain other halides (*e.g.* the *tert.*-butyl halides) is, on the other hand, found to be of the first order, the rate being independent of the concentration of hydroxide ions and the same in acid, neutral and alkaline solution. Evidently, there is a difference of mechanism between the two types of cases and the kinetic evidence supports the view that, while some substitution reactions take place by a bimolecular mechanism (S_N2), others follow a unimolecular course (S_N1). Numerous other observations confirm this conclusion. We cannot at this time give a full discussion of the evidence, for the available space will now be required for a description of the application of the mechanistic principles to the special problem under review. Suffice it to say that the two mechanisms are securely founded on the firm basis of experimental studies and that the question as to which of the two processes will be operative in a given case has been shown to depend in an intelligible manner on the structure of the reactants and the conditions of reaction (*cf.* Hughes, *Trans. Faraday Soc.*, 1941, **37**, 603, and references therein cited). Occasionally, a given reaction may follow both mechanisms, the predominance of one or the other depending on the precise experimental conditions employed.

Before concluding this section on the basic mechanisms of nucleophilic substitution reactions, it will be convenient for the purpose of later discussion to consider briefly the special case of the reactions of alkyl halides with hydroxylic solvents in the presence of silver salts. Detailed kinetic investigations have shown that silver ions here facilitate the removal of the halogen from a carbon-halogen bond weakened by adsorption on the polar surface of an insoluble silver salt, *e.g.*,

$$\text{R—Cl} + \overset{+}{\text{Ag}} \xrightarrow[\text{slow}]{} \overset{+}{\text{R}} + \text{Ag—Cl}\,;\quad \overset{+}{\text{R}} + \text{H}_2\text{O} \xrightarrow[\text{rapid}]{} \text{R—OH} + \overset{+}{\text{H}}.$$

The reaction, which thus depends on an assisted heterogeneous ionisation, is therefore analogous to the homogeneous unimolecular mechanism (S_N1), and similarities of behaviour between the two types have been observed (see, especially, Hughes, Ingold and Masterman, *J.C.S.*, 1937, 1236). For convenience in reference, this process will henceforth be termed "the silver-ion mechanism."

B. *The Steric Orientation of Substitution and its Dependence on Reaction Mechanism*

(i) *Historical Development.*—In attempting to explain the occurrence or non-occurrence of the Walden Inversion, most of the earlier theories postulated the prior formation of an addition complex in which the entering group attached itself to the asymmetric carbon atom either on the side of the group to be replaced or on the opposite side (Werner, *Ber.*, 1911, **44**, 873 and *Ann.*, 1912, **386**, 70; Pfeiffer, *Ann.*, 1911, **383**, 123; and others). The synchronous addition and dissociation mechanism which dispelled certain difficulties inherent in the older theories (*cf.* p. 522) was used by G. N. Lewis to explain how an inversion may be effected. "Let us consider a carbon atom attached to the four radicals, R_1, R_2, R_3, and R_4, and let us assume that a fifth group, R_5, becomes temporarily attached to the carbon atom near to the face of the tetrahedron which is opposite to R_1. A slight shift of the kernel might make it now the centre of a new tetrahedron with corners at R_2, R_3, R_4, and R_5, while R_1 would become detached from the molecule. Then if the radical R_5 in the new molecule were to be replaced by R_1, the resulting molecule would be the mirror image of the one with which we started. In this explanation it is not necessary to assume that the five radicals are attached to the carbon for any appreciable period of time, indeed it might be assumed that the R_1 leaves at the same instant that the R_5 becomes attached to the carbon atom" (Lewis, *loc. cit.*).

The alternative theory of prior dissociation in substitution processes has also received consideration in connection with the problem of the Walden Inversion. It was suggested by Lowry (*Conseil de Chimie Solvay*, 1925, 130) that substitution through an intermediate carbon cation might proceed with predominating retention of configuration, while Kenyon and Phillips (*Trans.*

Faraday Soc., 1930, **26**, 451; *J.C.S.*, 1930, 415) supposed on the contrary that it would tend to produce inversion. Kenyon and Phillips assumed that the formation of an intermediate carbon anion would lead to eventual substitution with retention of configuration, and they stressed the possibility of retained configuration resulting from an even number of inversion processes. It was implied in these theories that the ionic interaction would occur before the intermediate carbon ion had time to assume its stable planar configuration.

More recent and precise hypotheses concerning the laws which govern the steric orientation of substitution have been advanced by Polanyi and by Olson, mainly on the basis of the theory of synchronous addition and dissociation (Meer and Polanyi, *Z. physikal. Chem.*, B, 1932, **19**, 164; Bergmann, Polanyi and Szabo, *ibid.*, 1933, **20**, 161; Ogg and Polanyi, *Trans. Faraday Soc.*, 1935, **31**, 604; Olson, *J. Chem. Phys.*, 1933, **1**, 418; Olson and Long, *J. Amer. Chem. Soc.*, 1934, **56**, 1294). Polanyi suggested that substitutions effected by anionic reagents ("negative mechanism") involve inversion, while those in which the attack is by a cation ("positive mechanism") result in retention of configuration. Olson on the other hand assumed that every substitution in which only one bond is exchanged involves inversion. Polanyi and Olson have applied their theories mainly to the reactions between halide ions and simple alkyl halides or halogenoacids. Holmberg (*J. prakt. Chem.*, 1931, (II), **88**, 553; *Arkiv. Kem. Min. Geol.*, 1917, **6**, No. 23, 33) had previously suggested that the racemisation of optically active halides by halide ions was generally due to a halogen exchange reaction leading to inversion, but he did not specify that each and every act of substitution had this stereochemical effect. Polanyi pointed out that the dipole field in a link such as $\overset{\delta+}{\mathrm{C}}\text{—}\overset{\delta-}{\mathrm{Cl}}$ would preferentially direct an anionic reagent such as $\overset{-}{\mathrm{I}}$ to the position leading to inversion. Olson employed substantially the same idea, pointing out also that, since the carbon "Eigenfunction" involved in the carbon-halogen bond extends beyond the carbon atom on the side away from the halogen, there will be a tendency for bond formation at the face of the tetrahedron opposite the halogen. "A particle with unbonded electron pairs such as a chloride or a hydroxide ion will require a smaller energy of activation to react with $R_1R_2R_3C.Cl$ if it approaches the carbon face directly opposite the chlorine . . . the group which is to be displaced determines a unique path for the entering group, such that the system requires less energy than it would for any other path. This, then, leads to complete inversion of configuration for a reaction which takes place in one step" (Olson, *loc. cit.*).

(ii) *Application of the Concept of the Duality of Mechanism.*—In developing the application of considerations of mechanism to the problem of the occurrence or non-occurrence of the Walden Inversion, Hughes and Ingold and their co-workers adopted as the basis for investigation the concept of the duality of mechanism in substitution reactions. Recognising that the steric course of substitution must be intimately dependent on reaction mechanism, and having traced through the medium of kinetic data an intelligible relationship between mechanism on the one hand and the structure of the reactants and the conditions of reaction on the other, it was an obvious sequel to apply the kinetic method to the study of the Walden Inversion. By studying the steric course of substitution under conditions conducive to the operation of definite mechanisms (as disclosed by reaction kinetics) it has been possible to relate steric orientation to mechanism and its determining factors, structure and experimental conditions, in a more rational manner than was before possible.

As already indicated in a preceding section, Hughes and Ingold employ the theory of synchronous addition and dissociation in conjunction with that of prior dissociation, and they do not make the net charges of the interacting species a primary basis for the classification of substitutions. Treating nucleophilic substitutions as either bimolecular (S_N2) or unimolecular (S_N1), it has been shown that while the bimolecular process invariably leads to inversion of configuration, the unimolecular mechanism may give rise, depending mainly on structural influences, either to retention of configuration or to racemisation with predominating inversion.

The work of previous investigators, particularly that of Polanyi and Olson, had gone far in establishing that inversion is the rule for bimolecular reactions involving the substitution by a negative ion of a halogen atom in an alkyl halide molecule. A direct demonstration of the correctness of this view was given with the aid of the radioactive indicator method (Hughes, Topley and co-workers, *J.C.S.*, 1935, 1525; 1936, 1173; 1938, 209). By means of a comparison of the rates of substitution in the presence of the corresponding halide ions (containing a proportion of a radioactive isotope) with the rates of racemisation of the optically active halides under comparable conditions, the substitution of iodine by iodine in *sec.*-octyl iodide and bromine by bromine in α-phenylethyl bromide and in α-bromopropionic acid was shown essentially to involve inversion.

Although recent theories sometimes differ in their detailed application to certain special cases of bimolecular substitution reactions (see below), it is now generally agreed that reactions of the above type invariably lead to inversion of configuration and the result may be regarded as so definite that it can be used as a means of relating sign of rotation to configuration in the reactants and the products. The conclusions thus reached have been shown to be consistent with the assignments of relative configurations which can be deduced from the sum total of evidence relating to the application of the most reliable of the methods discussed in a previous section (p. 517). The stereochemical consequences of single reactions and, especially, of particular reaction mechanisms can then be deduced, as exemplified below. In bimolecular hydrolysis with aqueous potassium hydroxide, *d*-α-bromopropionic acid gives *d*-lactic acid; retention of sign in this example may therefore be regarded as indicative of inversion of configuration. Now in the reaction with moist silver oxide, *d*-α-bromopropionic acid gives *l*-lactic acid; it may therefore be concluded that this conversion is accompanied by retention of configuration. Thus, with the aid of further kinetic work on the reactions with silver salts and so on, it becomes possible to discover the stereochemical consequences of mechanisms other than the bimolecular mechanism, particularly the unimolecular process and the analogous " silver-ion " reaction. A clearer conception of the whole phenomenon then emerges as will be illustrated below for the replacement of .Hal. by .OR (.OH, .OEt, etc.).

An extended investigation (*J.C.S.*, 1937, 1196–1271; *Trans. Faraday Soc.*, 1938, **34**, 202; *J.C.S.*, 1940, 1010) along the lines considered above has shown that the qualitative stereochemical result of substitution by any of the mechanisms herein discussed is independent of the halogen which is replaced (chlorine or bromine, in the examples studied) and of the group which replaces it (hydroxyl, methoxyl or ethoxyl). Furthermore, the qualitative data for the heterogeneous reactions with silver salts are independent of whether the active surface is silver chloride or bromide, or one of these together with silver oxide or carbonate. It is then possible to express the main results of the investigation in the condensed form adopted in the Table below, which summarises the empirical knowledge available concerning the relationship between the steric course of the replacement of .Hal. by .OR on the one hand, and the structure of the reactants and the mechanism of the reactions on the other.

The substantially complete inversion which accompanies substitution by the bimolecular reactions, the marked differences

STERIC COURSE OF THE HOMOGENEOUS AND HETEROGENEOUS REPLACEMENTS OF .Hal. BY .OR.

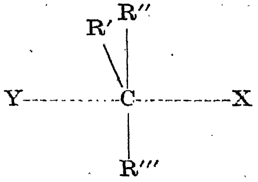
(I = Inversion ; Ra = Racemisation ; Re = Retention)

R in R.CHMe.Hal.	Effect on Configuration.		
	Mechanism S_N2	Mechanism S_N1	Silver-ion Mechanism.
C_6H_{13}	I	I + Ra	I + Ra
C_6H_5	I	I + Ra	I + Ra
CO_2R'*	I	—	I + Ra
$\overset{-}{CO_2}$	I	Re	Re + Ra

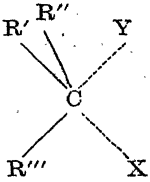
* For mechanisms S_N2 and S_N1, R′ in the examples investigated = H or Alkyl ; for the silver-ion mechanism, R′ = Alkyl.

between the results for the two homogeneous mechanisms (S_N1 and S_N2), and the similarities exhibited between mechanism S_N1 and the silver-ion mechanism are striking features of the Table. It remains for us to discuss the various results in more detail.

Inversion of configuration is presumed to be the rule for bimolecular substitution (S_N2) primarily because the transition state which leads to inversion (I) will have a smaller energy than that which corresponds to retention of stereochemical form (II) :



(I)

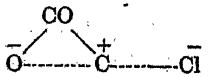


(II)

The relative energies of the transition states are assessed by application of the exclusion principle. Transition states such as (II) will be less stable than the " linear " state (I) because of the large positive exchange integrals between the electrons of the nonlinearly split bond and those of the full bonds. The electrostatic factor introduced by a dipole field in the breaking bond, as in Polanyi's theory, will also make a definite contribution to the difference between the theoretical rates of the alternative processes, but the effect arising from the exclusion principle is considered to be more important, and it may take control when the two factors oppose each other, as in the bimolecular reaction of an anion with an 'onium cation. In the attack of $\overset{-}{Y}$ on $C—\overset{+}{N}R_3$ the electrostatic forces direct the anion to the position leading to a retained configuration, while the exclusion principle still favours substitution with an inverted configuration. Considerable interest is therefore attached to the experimental determination of the steric course of a reaction of this kind. There are indications that inversion occurs in the one (rather complex) example of such a reaction for which results were already available (see *J.C.S.*, 1937, 1257); and inversion has recently been observed for a simple substitution process of this type (unpublished experiments by Dr. S. H. Harvey).

The stereochemical consequences of the unimolecular reactions are not so clear-cut, the result depending on the nature of the groups attached to the asymmetric carbon atom, but again the experimental observations can be satisfactorily interpreted on the basis of the adopted mechanism (S_N1). When only neutral groups are attached to the reaction centre, the dominant and characteristic stereochemical result of unimolecular substitution is extensive racemisation, and this is in harmony with independently derived evidence that, given sufficient time, the carbon cation will tend to assume a planar configuration. Complete racemisation should result only if the ion is long-lived, either by reason of an intrinsically small reactivity in the ion itself, or on account of the low reactivity and/or concentration of the reagent. In general, however, this condition is not expected to obtain, especially in the case of those reactions ("solvolytic" reactions) in which the solvent is also a potential reagent for the substitution process. The experimental observations are that partial inversion usually accompanies the loss of optical activity and this may be interpreted as an effect of the partial shielding of the cation by the receding anion, which may not be many molecular diameters away when the attack by the enveloping solvation shell or by an anionic reagent completes the substitution process. The evidence (which is as yet incomplete) relating to the effects of changes in structure and in experimental conditions on the degree of completeness of racemisation in substitutions of this kind is consistent with the explanation given.

The most interesting stereochemical outcome of substitution by a unimolecular reaction occurs in the presence of a group, such as an α-carboxylate-ion group, which can bind a cationic centre, holding it with a bond of mainly electrostatic character pending solvent attack. During the process of ionisation the charged substituent will be oriented away from the eliminated ion (*cf.* inset),



and it will tend to hold the pyramidal configuration thus produced until a new group enters the position from which the old one left, thus leading to retention of configuration. It may be assumed that, contributing to the normal state of the "ion" (which is now a betaine) there is a canonical structure in which the two charged atoms are given septets, which maintain a tetrahedral character in the orbitals.

The complete contrast between the results for the two homogeneous mechanisms (S_N1 and S_N2) when a carboxylate-ion group is directly attached to the asymmetric carbon atom illustrates most forcibly the importance of studying the steric effect of substitution under kinetically controlled conditions. The hydrolysis of the α-bromopropionate ion, for example, is found to occur in general by both the bimolecular and the unimolecular mechanisms, but, by suitably choosing the concentrations of the reactants, the separate mechanisms, S_N2 and S_N1, can be isolated (the bimolecular reaction in concentrated alkaline solution and the unimolecular reaction in dilute alkaline solution), and, by studying the optical effects of substitution at these two extremes of concentration separately, it is found that while the bimolecular mechanism leads wholly to inversion, the unimolecular mechanism gives almost quantitative retention of configuration, as already explained. One can imagine the complicated and misleading results which might emerge from an investigation carried out without such a control of mechanism.

In the early work on the Walden Inversion it was often found, as already mentioned, that different stereochemical results could be obtained by the use of different reagents, such as, for example, silver oxide and potassium hydroxide in the replacement of halogen by hydroxyl. In our example we have obtained two opposite stereochemical results by the use of *one* reagent, namely potassium hydroxide, at different concentrations, these concentrations being such that one mechanism predominates at one extreme of concentrations and another mechanism at the other extreme. There could be no better illustration of the soundness of the belief that the key to the apparent mysteries of the Walden Inversion is to be found through a study of reaction mechanism.

Concerning the similarity of behaviour between the unimolecular and the silver-ion mechanisms, no detailed explanation is necessary. The conditions in the homogeneous reaction being different from those obtaining in the heterogeneous process, we could make no quantitative predictions in regard, for instance, to the comparative degree of racemisation to be anticipated, but, from the nature of the two mechanisms, a qualitative similarity should hold, and this is found to be the case.

By applying the orientation rules, deduced as outlined above, to previous investigations of the optical effects accompanying the hydrolysis and alcoholysis of optically active halides, it has been possible, in spite of the general lack of precise information concerning the conditions under which the determinations were made, to formulate tentative assignments in regard to the relationships between the signs of rotation and the configurations of the reactants and the products involved in many replacements of .Hal. by .OR (*J.C.S.*, 1937, 1252). With this preliminary knowledge, it is then possible to obtain some insight into the stereochemical behaviour of those reagents (phosphorus halides, thionyl chloride, hydrogen halides, etc.) which, acting on the hydroxy-compounds, restore a halogen atom, and some progress has been made in the reinvestigation of these reactions (*cf.* Hughes, Ingold and Whitfield, *Nature*, 1941, **147**, 206.).

Some further work remains to be done on the mechanism and steric orientation of the reciprocal replacements of .Hal. by .OR. Other reactions are being investigated in a similar way.* It may perhaps be claimed that a good start has been made towards a fuller understanding of the fascinating problem under review and its broader implications regarding the stereochemical course of reactions generally. Commenting on the method used in the recent work, Hammett has expressed the following view: "It demonstrates the possibility of determining relative configurations of optically active compounds by reaction kinetic measurements, and breaks a path through the jungle of confusion which has always surrounded the Walden Inversion" (*Trans. Faraday Soc.*, 1938, **34**, 221). That is roughly how the position stands at the present time; the rapid progress which seemed possible in 1938 has been inevitably retarded by the events of the intervening years. Other parts of the "jungle" still require intensive cultivation but the "path" which has been shaped, and the knowledge and experience of a successful technique now available, will be of great assistance in completing the work.

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* Considerable progress has been made, particularly on the reciprocal replacements of .Hal. by $.NH_2$.

THE NEW OPTICAL GLASSES

By H. W. LEE, B.A., F.Inst.P.

Pullin Optical Co.

HISTORICALLY the production of optical glass has followed stimulus from the instrument-maker. Newton's experiments seemed to show that dispersion was proportional to refraction (in modern terms that the Abbe V number was constant for all glasses) so that achromatism was impossible. This misconception must have arisen from the paucity of glasses available. Later a different type of glass was introduced by the English glassmakers, who found that the addition of lead salts produced a heavier glass from which table-ware of increased lustre could be made. The amateur Chester Moor-Hall and the optician Dolland independently found that an achromatic combination could be made with a dispersive lens of this glass and a more powerful collective lens of the old "crown" glass.

Glass at that time was not made specifically for optical instruments; only by selection was it possible to find suitable pieces, and these not exceeding 3 inches in diameter, for telescope objectives. The first problem for the glassmaker was to improve the manufacture of existing glass, the two types of which fulfilled the requirements of the day. Henri Guinand, a Swiss watchmaker, made the crucial invention, that of stirring the glass in the molten state, in 1784. Fraunhofer had also realised the necessity for improved glass for optical purposes and had made some experiments. He and his partners induced Guinand to impart his secret to them, and the manufacture of optical glass was started in Bavaria in 1805. The partnership was dissolved in 1813, and Guinand resumed work on his own account. On his death in 1824 his son associated himself with the French optician Lerebours and the glassmaker Bontemps and founded a glass-works at Choisy-le-Roi, from which the present firm of Parra-Mantois is descended. The English firm of Chance Brothers, which had established a great reputation for lighthouse lenses and plate glass, found itself at a disadvantage in making telescopes because of the limited size of the discs of suitable quality it could produce, while discs three times as large were being produced in France and Germany. They had recourse to the expedient of enticing Bontemps to work for them and bring his skill and secret methods, and even his assistants, to England.

Faraday came into contact with glassmaking through his appointment by the Government as assessor of the relative merits of English and French lighthouse lenses—the form of which was due to Fresnel. He made numerous laboratory experiments, using platinum crucibles, and is responsible for the introduction of boron into glass, but, probably from his lack of experience, his work did not result in successful production. The manufacture of glass seems to be one of those crafts which require for success a background of traditional skill and knowledge ; it has in fact been called an art rather than a science by a manufacturer and a scientist writing in collaboration. Not only must the materials be uniform in quality and combined in the correct proportions, but the operation of mixing the batch, raising the temperature, stirring, cooling and annealing must be carried out with skill and precision. The composition and manufacture of the melting-pots is also important. Glass is considered to be a mixture, a "supercooled solution," and if the correct process is not carried out in all its detail the ingredients may crystallise out—the process of "devitrification." Some substances always devitrify and are thus useless for glassmaking ; others require the presence of complementary substances to prevent devitrification. With these and other difficulties to be encountered glassmaking is rarely successful as a laboratory process. Another amateur attempt at extending the glass range was made by Vernon Harcourt, who systematically investigated the use of, among others, boric, phosphoric and titanic acids. (These were afterwards successfully employed, the first two by Schott ; the last appears in the latest glasses.) He tried to stir his glass by rotating the pot. It is probably because of his failure to achieve reasonably homogeneous pieces of glass that his results were never fully published. In 1862 he was associated with Stokes and it is on record that they produced an objective made with two borate flint glasses and a crown glass which had a reduced secondary spectrum. Reduction of the secondary spectrum had then become the optician's aim.

When, through the improvement in the manufacture of glass, it was possible to make large astronomical refracting telescopes the achromatism was found to be very imperfect. Achromatism, that is, the bringing rays from two parts of the spectrum to a common focus, is possible because dense flint glass has greater dispersion than crown glass, but the increase is not uniform throughout the spectrum—the dispersion of flint glass is relatively greater at the blue end than at the red end of the spectrum, so that the dispersion of the crown is not exactly neutralised throughout the spectrum. The residual colour is known as the secondary spectrum. The problem was to find glasses with greatly different dispersions in which the ratio was uniform throughout the spectrum. It may here be stated that while some improvement has since been effected the problem is not yet completely solved, and the manufacture of large refractors has been abandoned in favour of reflectors.

While these attempts to extend the range of glasses were being made photography had been developing in importance and the need had arisen for photographic lenses which would yield a sharp image of an extended field on a flat photographic plate. The theoretical conditions for this had long ago been worked out by Airy and by Petzval with the disconcerting discovery that with the glasses in existence this was inconsistent with achromatism. Actually this was an error[1] comparable with Newton's, and Piazzi-Smyth, the Astronomer Royal, had shown that a flat field correction was possible with the existing glasses. (Incidentally he invented miniature photography. Both his inventions were premature and still-born.) Apart from the Piazzi-Smyth construction the flat field lens seemed more likely to be realised if glasses were available having high refraction and low dispersion which could be combined with glasses of low refraction and high dispersion.

Again it was the combination of an optician (Abbé), a glass-maker (Schott) and an instrument-maker (Zeiss) that led to success after experiments lasting from 1878 to 1884. The first flat field photographic lenses were termed anastigmats because they had a flat field without astigmatism, whereas earlier lenses possessed a quasi-flattened field by the introduction of overcorrected astigmatism. These anastigmats, using the new glasses, began to appear about 1890 in German designs, while H. D. Taylor followed with his much more original Cooke lens in 1893.

Chance Brothers started to make the new glasses in 1895, but it was not till well on in the new century that they were able to issue a short list of optical glasses which included Dense Barium Crowns and Light Flints. At the beginning of the war in 1914 the overwhelmingly greater part of the glass used in optical instruments was made in Germany, with smaller amounts in France. However, the nucleus existed from which an organisation was built up able to supply England's need in the war and since. An immense amount of work also was done by Dr. Peddle, in systematically investigating the effects of different constituents, for the Derby Crown Glass Co. This company afterwards became the Parsons Glass Co. and was finally amalgamated with Chance to produce the Chance-Parsons optical glass.

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[1] Both achromatism and field flatness can be attained by separation of the collective from the dispersive lens. Piazzi-Smyth placed a dispersive lens right away from the camera lens and close to the focal plane.

It will have been noticed that hitherto every fresh advance in glassmaking had been stimulated by some demand from the optical instrument-maker, and the question arises, what is the present demand of the optician, and is it fully met by the glassmaker? It would be as well, before attempting to answer these questions, to examine the optical situation.

In photographic optics the trend has been for lenses of larger aperture; cinematography and miniature photography have emphasised this trend because only in lenses of small focal lengths, suitable for miniature images, is there sufficient depth of field at large apertures. Undoubtedly rapid lenses and extremely sensitive emulsion give the photographer to-day powers of photographing under adverse lighting conditions undreamed of a few years back. Now extremely rapid films tend to become coarse-grained and lose resolving power, while as the size of the format decreases the need is for greater resolving power. The resolving power of the photographic lens, on the other hand, increases as the focal length decreases, but depth of field (the ability to render sharp images of objects at different distances from the lens) decreases with increasing aperture (and speed). It seems probable that in future it will be possible to produce very rapid photographic emulsions with increased resolving power; lenses must increase in resolving power *pari passu*. Another direction in which lenses can be improved is by giving a more uniform illumination over the area covered. At present it is usual to tolerate a diminution of exposure from the centre to the edge of the picture. To rectify this by increasing the diameter of the lenses generally leads to a deterioration in the quality of the image. No doubt research will be directed to improving lenses in this respect. It is unlikely that any great increase in present apertures will be needed except in special circumstances, *e.g.* the photography of fluorescent X-ray skiagrams.

In lenses used for reproduction of pictures by surface printing, *i.e.* "process" lenses, the increasing use of colour work will stimulate research in apochromatic correction. In microscope objectives the need is for extremely high resolution and apochromatism to effect this. Hitherto the mineral fluorite has been employed to make apochromatic objectives and any further improvement will probably be sought for in the use of natural or synthetic crystalline material (the size is so small that practical difficulties, which usually occur in the production of large crystals, do not exist, and expense is almost no object). But with increasing use of photography in association with the microscope the demand is likely to arise for flat field objectives of large aperture. Such lenses are difficult to design and the construction is necessarily complicated and costly, so that progress has not been great and has hitherto been confined to one firm. Nevertheless, the demand will increase and will have to be met by the production of a whole series of flat field objectives.

In telescopes advance is needed in two directions—objectives are wanted of larger aperture and field for hand-held instruments (binoculars) and of flatter field in general, especially where graticules are used. Graticules require the flat field eyepiece, a design that has hitherto been shirked through fear that the construction will be too complicated and the increased number of glass-air surfaces lead to too much loss of light and contrast.

All these possible developments suggest increasing complication in structure, and it is natural that the optician should look to see if the glassmaker can help him either to simplify his construction or to improve the quality of the image, by providing glasses having an extended range of optical properties.

Just before the war it was evident that progress was being made in Germany in the production of glasses of unusually high refractive index with relatively low dispersion, as such glasses began to appear in patent specifications of German origin. The advance was not spectacular but showed the direction in which things were moving. We now know through the publication of a specification by the Custodian of Alien Property in the U.S.A. that these new glasses contained lanthanum oxide. This is found suitable for increasing the refractive index but produces a tendency to crystallisation, especially in the presence of silicon oxide, the usual flux. To overcome this tendency boric acid is substituted, but this introduces a fresh trouble—the glass has low resistivity to acid: substitution of barium oxide for some of the boric overcomes this defect. Part of the barium may be replaced by other earth alkalis as well as zinc and lead to produce a range of satisfactory glasses in which n_D is greater than 1·86 − 0·004 V. The latest information is that thorium oxide also enters into the composition.

The next advance to be recorded was made by Kodak, as described in British Patent Specifications 462,304 of September 3, 1935, and 534,680 of October 7, 1939, in which glasses are claimed having refractive indices in excess of 1·75, with V less than 50 but greater than $38 - 38\sqrt{n_D - 1{\cdot}75}$, that is, n_D greater than $2{\cdot}75 - 0{\cdot}0526\ V - 0{\cdot}00069\ V^2$. In the later specification the glasses have larger V and smaller n_D. These striking results (the refractive indices go up to 2·0) were obtained by the incorporation of oxides of one or more of the following metals: tungsten and metals having an atomic weight greater than 47 and lying in the even series of Groups III, IV and V of the periodic series, with silica content not greater than 10 per cent. The metals specified are titanium (tried a century earlier by Harcourt), zirconium, lanthanum, tantalum, thorium, tungsten, yttrium, colombium and hafnium. Some of these are too rare to have much practical importance. A notable breakaway from the traditional materials of glassmaking is thus recorded. Hitherto silica had been an essential ingredient, but in these glasses its content is limited on account of the tendency to devitrification with the special oxides. This tendency can be checked by addition of sodium hydroxide, but Kodak prefer not to use silica at all or limit it to 1 per cent. or less. The process of manufacture is to melt the weighed, screened and mixed materials at a temperature of 2000° to 2500° Fahrenheit. The melted batch is then poured into a container at a temperature of about 1000° and placed in an annealing oven at a temperature of about 1500° and slowly cooled.

During the war the Chance glasses appeared; these are neither so numerous nor cover so wide a range as the Kodak glasses, but mark a distinct advance in optical properties over the pre-war glasses. It is not possible to disclose at present the constituents of these glasses beyond the general statement that appreciable quantities of silica and rare metallic oxides enter into their composition. One of the difficulties encountered with glasses of extreme type is that in the melting process they attack the pots. The newest glasses are therefore melted in platinum vessels (as were Faraday's experimental melts). Electrically heated furnaces are specially designed to give the exact heat control required. The whole contents of the pot are poured into one or relatively few slabs so that although the weight of each melt is comparatively small quite large lenses can be produced. The experimental batches were no greater than 5 pounds, but this has been progressively increased to 10, 20 and 50 pounds, and 100-pound melts are contemplated. The costly materials and the method of manufacture make these glasses necessarily more expensive than ordinary glasses. One important advantage of melting in platinum pots is increased freedom from colour, seed (small glass bubbles), etc., so that the newest glasses are almost as " white " as borosilicate crown, hitherto regarded as the standard in these respects, and therefore usually chosen for prisms. Exact temperature and other controls are necessary to ensure that uniformity between one batch and another which is a desideratum if the optician is not to be continually recomputing his systems. Constant watch has to be kept on the tendency to devitrification, and Chance's have worked out a method of exploring this tendency. These glasses, despite their extreme properties, seem to be at least as resistant to weathering as normal flint glasses. They are, moreover, harder than flints of the same refractive index, which is an important advantage to the glass polisher.

It may be of advantage to explain here the nomenclature of optical glass. "Crown" glass was originally made by spinning out a sheet from a lump of molten glass gathered on an iron rod, to a diameter of 3 feet 6 inches to 4 feet. In the days when excise was collected on glass and every operation of its manufacture supervised by resident excise men, stringent regulations were in force as to the raw materials that could be used. Rather better materials were allowed for "crown" than for bottle glass, hence the name. "Flint" glass was so named from the source of one of its ingredients. When the number of glasses was increased for the benefit of the instrument-maker the two names were retained for glasses having small and large dispersions respectively, but the main classes were subdivided. Thus there were Hard, Soft and Zinc Crowns (H.C., S.C. and Z.C.). The addition of boron gave us Borosilicate Crown (B.S.C.) and of fluorite Fluor Crown (F.C.). The Schott era added barium, and there were introduced Light, Medium and Dense Barium Crowns (L.B.C., M.B.C. and D.B.C.) and Barium and Dense Barium Flints (B.F. and D.B.F.). The ordinary flints were divided into Extra Light, Light, Dense, Extra Dense and Double Extra Dense Flints (E.L.F., L.F., D.F., E.D.F., D.E.D.F.). The dividing-line between crown and flint is somewhat vague and in fact they now overlap. The newest glasses have been named by Chance Special Barium Crown and Flint (S.B.C. and S.B.F.).

The figures used in assessing the optical properties of glass are, firstly the absolute refraction, specified by the refractive index for the mean sodium D lines or helium d; secondly the Abbé ν or V, the quotient of the refractive index minus one by the standard dispersion for the hydrogen C and F lines; and thirdly the relative partial dispersions, that is, the ratios of the dispersion between various lines of the spectrum to the standard dispersion. The partials are needed only in estimating the secondary spectrum.

If n_D is plotted against V for all glasses listed in the different makers' catalogues the "old" (*i.e.* pre-Schott) glasses are seen to lie close to a well-defined curve. The "new" glasses (barium crowns) lie away from the curve in the region of increasing n, and their separation from the old curve is an indication of their optical value. Glasses which lie still further away from the curve should be of still greater value, and this is just what the "newest" glasses do. In Fig. 1 the pre-war glasses are taken from the Chance-Parsons list, but for simplification some are omitted. The newest glasses as listed by the various makers are also shown, S.B.C. and S.B.F. are Chance glasses, P.M. those of Parra-Mantois of France, and S those of Schott. The remainder are Kodak's glasses. A circle indicates that the properties are not given with

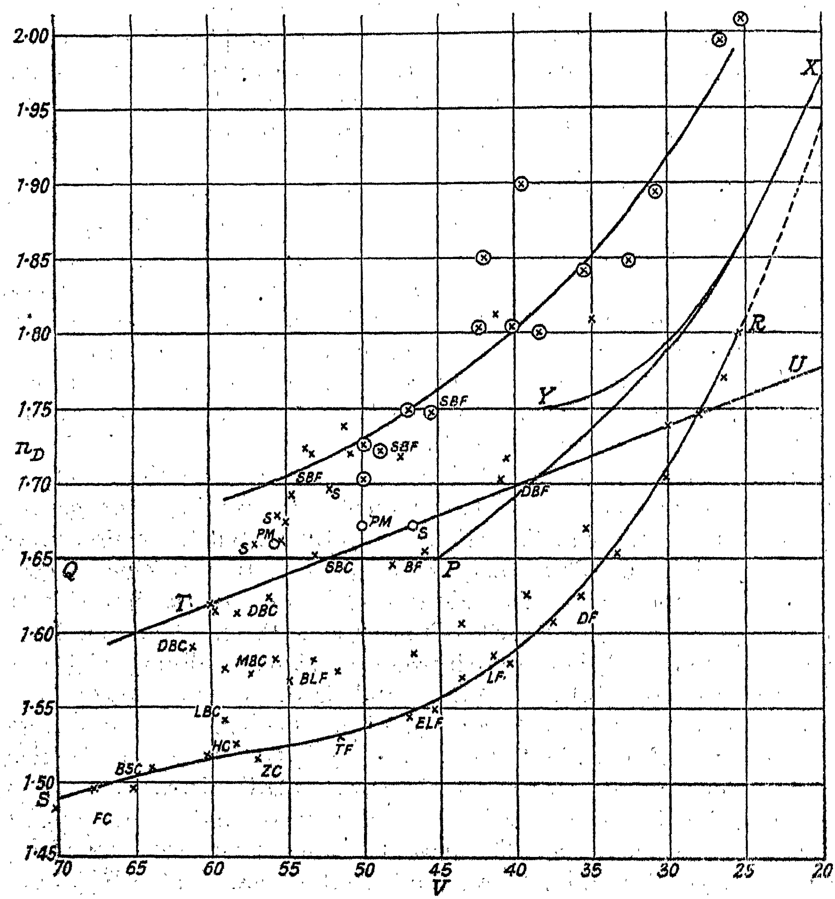


FIG. 1.

full accuracy. XY is the limiting curve of Kodak's specification, *viz.* $V = 38 - 38\sqrt{n - 1{\cdot}75}$. QPX marks the region above which the glasses in Kodak's later specification lie. The straight line $n = 1{\cdot}86 - 0{\cdot}004\,V$ of the German specification is also shown (TU). The " newest " glasses are seen to lie on a higher curve than the " new " glasses, and mark a distinct step forward comparable with that of Schott in the 'eighties. It is rather remarkable that the glasses should lie on such well-defined bands in view of the number and variety of new metals that have now been used.

To exhibit any possible improvement in achromatism indicated by the new glasses, the ratios of the partial dispersions for the region from the helium red line *b* to the hydrogen C to that for hydrogen F to mercury *g* are plotted as abscissa and ordinate respectively (Fig. 2). It is at once seen that all the glasses lie on a narrow straight band, implying that for all glasses the relation

$$k(n_b - n_C) + l(n_g - n_F) + (n_F - n_C) = 0$$

is approximately true. If two lenses of curvatures C_1 and C_2 are achromatised for C and F, then

$$C_1(n_F - n_C)_1 + C_2(n_F - n_C)_2 = 0$$

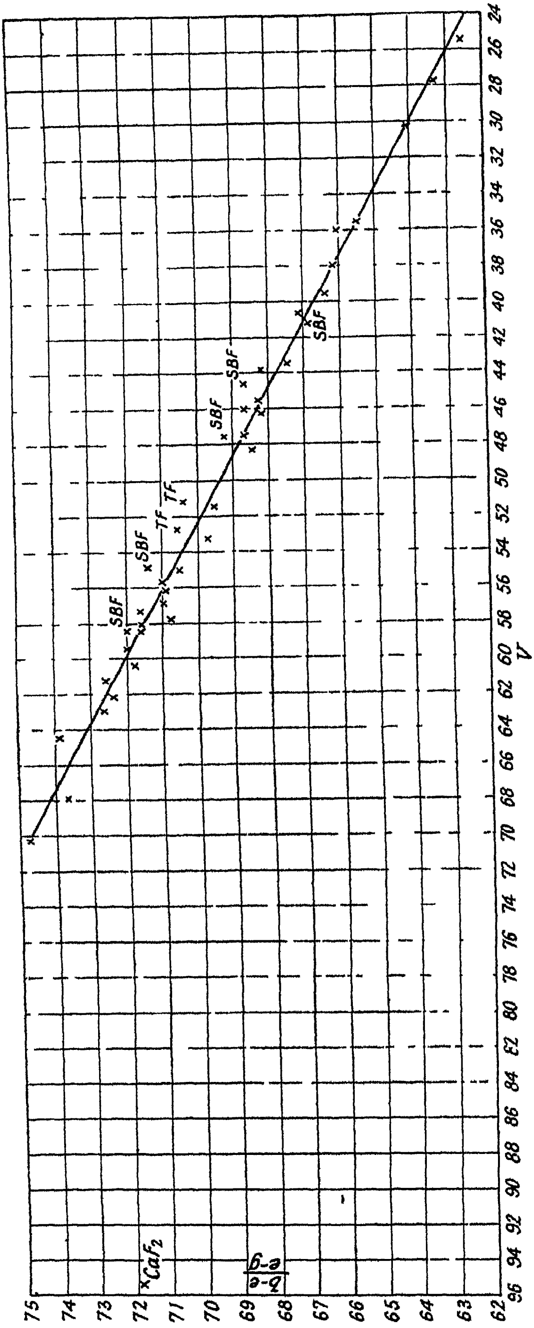
The partial differences in power are

$$\delta F_{g-F} = C_1(n_g - n_F)_1 + C_2(n_g - n_F)_2$$

and $$\delta F_{b-C} = C_1(n_b - n_C)_1 + C_2(n_b - n_C)_2$$

whence $k\delta F_{b-C} + l\delta F_{g-F} = 0$, so δF_{g-F} will only be zero if δF_{b-C} is, and this requires that the partial dispersions should be identical for the two glasses. No glass pairs having a substantial difference in V fulfil this condition, though some approach it. Flint glasses of which the partial dispersions approach those of crown glasses are called Telescope Flints (T.F.). The position of fluorite is plotted for comparison to show how much more favourable this is for producing apochromatism than two glasses are. Some of the new S.B.F.'s are seen to be nearly as useful as the T.F.'s, which have been particularly introduced for reducing the secondary spectrum.

The question now arises, what advantages can the optician expect to derive from the new glasses ? In telescope objectives, no great improvement in apochromatism being likely, the possibilities of achromatic combinations having other advantages may be explored. For each crown glass each flint glass within a certain range will form with it two different achromats free from spherical aberration, and among these possible combinations some have special advantages. A very few are almost free from coma and give an extended field ; these are termed aplanats. The new glasses extend the list slightly. There are some pairs which have the first surface flat. These are useful in two ways. Their shape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SBF
SBF
SBF
SBF
SBF
TF
TF
CaF₂
β-a
b-e

FIG. 2.

renders them suitable for cementing to flat-sided cells through which it may be desired to pass a parallel beam of light, *e.g.* in the liquid cell used in television where the collimated beam is diffracted by a modulated supersonic train of waves. If two such objectives are placed with their flat faces in contact the two central components having a common flat face can be replaced by a single lens; the triplet so formed is aplanatic when used to produce a real inverted image full size. These triplets have considerable practical importance, *e.g.* as erectors in telescopes. Hitherto there has been only one really useful combination of glasses giving the desired correction and this has required rather deep curves and so has a limited aperture. With the new glasses there are several more possible combinations with flatter curves, which will give better definition at a larger aperture.

The telescope objective is a simple system which is quickly investigated, but the photographic objective, the flat field eyepiece and microscope objective are more complex and the effects of the new glasses have not yet been fully investigated. Generally speaking, the additions to the glass list will facilitate design and lead to flatter curves. Those glasses which depart most widely from the curve RS of the " old " glasses have hitherto proved most useful in flat field systems, so the " newest " glasses should prove still more favourable. Probably they will be used in conjunction with flints higher up the curve and take advantage of the large dispersion of such flints. The higher dispersion needs lower powers for achromatism, while the higher refractive indices will result in shallower curves with equal satisfaction of the Petzval condition for field flatness. But it must be observed that the two curves of the " old " and the " newest " glasses tend to approach one another as the refractive index gets higher and V lower, so that the advantage begins to be lost if this advance is pushed too far. It is not easy to formulate the optical requirements therefore, and no new optical writings have suggested on theoretical grounds what new glass types are needed. There was, however, a general feeling before the war that the design of photographic lenses had reached almost a stalemate—that it was unlikely that new designs of great originality or outstanding excellence would be forthcoming; that the ground had been fully explored and that the appearance of new materials or new surfaces would be necessary before much further improvement could be expected. As new material, synthetic or natural crystals and plastics were being explored, while aspherical surfaces and methods of producing them were also being investigated. Some success has been attained in the manufacture of large crystals and some of these will be useful, especially in the spectroscope, because of increased transmission in special regions, or because with their help achromatism over a large region of the spectrum will be possible. The plastics have hitherto proved less useful as they do not attain to the standard of glass in stability, uniformity, transparency or durability. They have succeeded chiefly in showing what a wonderful substance optical glass is! Aspherical surfaces present difficulties both to the computer and the manufacturer, which cannot yet be said to have been overcome. They cannot be made with the accuracy of spherical surfaces, so that at present their use is confined to condensers and eyepieces and the hand-figured surfaces used in astronomical systems.

Opticians have been too busy with urgent war needs to investigate yet the full possibilities of the new material placed at their disposal. This is not to say that no patents have yet incorporated them. It was mentioned that before the war the Germans had specified some of these glasses, although the extreme ones had not then been produced, and it is known that some of these designs have been used in serial camera lenses. Since the introduction of the Kodak and Chance glasses English and American opticians have had over fifty patents incorporating them published in Great Britain alone. These nearly all apply to standard designs, however. Designers have apparently striven to peg out each his claim for the use of these glasses in the old designs, and it looks as if some improvement in existing designs is possible. As an example, B.P. 564,306 relates to a lens of the Goerz Dagor type made from Kodak glasses. As compared with the original this should give equally good results over a field 4° or 5° greater. Although this type of lens is obsolescent in England, in the U.S.A. it is used in air photography, where an extended field is of great importance as increased "coverage" of the lens reduces mapping costs. B.P.'s 523,062 and 532,950 claim a Cooke triplet made from the new glasses and having an aperture up to f/2·2. With the old glasses it was possible to design a triplet having an aperture of f/3 or even f/2·8 covering a field of about 35°. To increase the aperture still further it was necessary to incorporate more elements or use a different construction also employing more elements. The new lens thus shows a saving in manufacturing costs to offset the increased price of the glass. In another design (patent applied for) the new Chance glasses have been employed in a lens of the well-known Tessar type, with the result that the maximum departure from field flatness has been reduced to one-third the value in the original Tessar. B.P. 523,061 employs the new glasses in a well-known six-lens type having an aperture of f/2, with the result that zonal spherical aberration is very considerably reduced and the sharpness of the image improved. These, then, are some of the improvements that have been noted, although the writer cannot claim to have investigated the whole fifty patents. It must be repeated that the real possibilities of the latest glasses have not yet been exploited and there may emerge designs of an entirely new type. This is especially possible in flat field eyepieces and microscope objectives where the ground has been less well covered than in the case of the photographic lens.

The latest glasses have some disadvantages besides higher cost. They are heavier, and on account of their higher refractive index reflect more light. The amount of light reflected from a polished surface at normal incidence is $\{(n - 1)/(n + 1)\}^2$. This fraction amounts to 0·04 for $n = 1{\cdot}5$, *i.e.* about 4 per cent. of the light is reflected at each surface. The light reflected at the first surface of a lens is lost to the image, that reflected at the second surface returns to the first surface and some of it is again reflected back and falls as unwanted light on the image plane. With a complicated system there is interreflection at all the free surfaces so that a considerable fraction of the light is lost and another fraction falls as scattered light on the focal plane. In a photographic lens (for example) having six glass-air surfaces it has been calculated that as much as one-third of the incident light is lost and that 6 per cent. of the light reaching the photographic plate is stray light. With $n = 1{\cdot}6$ the proportion of light reflected at each surface is 0·055 and for $n = 1{\cdot}7$ it is nearly 0·07, and in the six glass-air lens the scattered light on the focal plane is 12 per cent. of the imaging light. This scattered light reduces the contrast of the image and is more serious than the loss of light which merely compels an increase of exposure. With instruments such as telescopes having as many as twelve surfaces the scattered light in the image plane may amount to as much as that in the image itself. This serious state of affairs has been alleviated by the recently introduced process of covering the surfaces with a non-reflecting coating. This acts by interference between the light reflected from the coating and that reflected from the lens surface, and under the correct conditions reflected light disappears entirely. These conditions are that the refractive index of the coating substance should be the square root of the refractive index of the glass it covers, and that the thickness of the coating should be an odd number (preferably one) of half-wavelengths of the light in the coating substance. Now the square root of 1·51, the refractive index of Boro Silicate

Crown, is 1·23 and there is no suitable substance having this low index. The square root of 1·7, the refractive index of a Special Barian Crown, is 1·30 which is close to 1·36, the refractive index of the usual coating materials magnesium and aluminium fluorides. Hence non-reflecting or "bloomed" surfaces as they are called are more efficient on glasses of higher than on those of lower refractive index. In practice, it is possible to reduce the surface reflection to the order of 1 per cent. by blooming; a bloomed lens of the new glasses therefore gives greater contrast than an unbloomed lens of the older glasses. The blooming process, of course, adds to the cost and indicates that the new glasses will not appear in the cheaper lenses and instruments.

Blooming is thus seen to be essential if the fullest use is to be made of the new glass, and it removes the restriction that the optician has always worked under to reduce the number of his glass-air surfaces to a minimum. With this limitation removed, the optician is freer to work out his designs, and with the new wealth of glass put at his disposal should be able to evolve better optical systems in the near future.

MIMICRY, PROTECTIVE RESEMBLANCE AND ANIMAL BEHAVIOUR

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IN discussions of protective resemblance and mimicry in animals—a recent example is the review of the genetics of mimicry by Dr. R. Goldschmidt [1]—the assumption is almost always made that a resemblance will be of value to the animal if it shows characters that would deceive a human observer. At the same time it is admitted that the value of these resemblances in the natural life of the animal lies in any power they possess of rendering ineffective the attacks of predatory animals. For insects these are very generally birds; less frequently and in some habitats, monkeys and other animals. The assumption thus implies that animals, especially birds, will be deceived by the same characters as those that would deceive us. The purpose of this note is to point out that, so far as birds are concerned, this is not so, for recent work on bird behaviour shows that in many situations birds recognise objects in a way very different from ours, and are often deceived where we should not be. If this is true, some revision of our theories of the evolution of these resemblances would seem to be needed.

Even before this recent work, it was by no means clear that a bird searching for an object, such as a caterpillar on a tree, will behave as we do. One cannot believe that the bird, like us, will form a conceptual image of the object of which it is in search, combining many characters of the object in a generalised conception of the whole, and will keep this image in mind as it examines the tree. It will not be continually *considering* whether each object it sees is caterpillar or bark. It is more likely that the bird will pass its gaze over the bark until some object stands out sufficiently from the background to attract its attention, and has a resemblance to a caterpillar good enough to stimulate the reaction of pecking. So, too, in chasing flying butterflies a bird will avoid a mimic if the visual stimulus it provides is sufficiently like that of the distasteful species it mimics. The bird will not *consider* whether any particular butterfly is or is not a mimic.

However, arguments such as these tell us nothing of the characters by which birds recognise objects as being of particular types. The conclusion of recent studies of bird behaviour is that very generally, at any rate in those situations—with which we are concerned in discussing mimicry and protective resemblance—in which recognition of the object leads to some course of action, a bird recognises an object not by its general appearance as a whole, as we do, but by some simple and usually single feature possessed by the type of object to which it belongs. This feature may have very little to do with the general appearance of the object.

It is true that not all bird behaviour is of this type. In some situations, especially in recognition of individuals of the same species [2] and in the artificial situations provided by experiments on problem-solving [3], but perhaps also in some other situations, birds seem to be capable of behaviour more like ours. At least, the simple behaviour here described is general enough to be taken as a very important, and probably the most important, part of the background for our views of protective resemblance and mimicry.

Lorenz [4] speaks of the features used in recognition as "releasers," by which he means that they serve as signals to initiate or release the action with which they are associated. He gives an example from the behaviour of the jackdaw (p. 247). "For a long time I have been familiar with the fact that my tame but free-living jackdaws would furiously attack me if I gripped one of them in my hand, but I was very much astonished when I inadvertently elicited exactly the same response by carrying a wet, black bathing-suit in my hand. Subsequent experiments showed that anything glistening, black and dangling, carried by any living creature, would release the very same reaction in the jackdaws. Even jackdaws themselves were subject to attack from their fellows when they happened to carry nesting material possessing the characteristics just mentioned." The social defence reaction was called out by this simple signal; if that were present, the reaction resulted whatever the other characters of the situation.

Again, Lack [5], in his study of the robin, found that the bird's aggressive behaviour against another bird in its territory, though somewhat complex, is built up of similar reactions. It consists of at least three reactions, each stimulated by a different releaser. The reactions are (1) flight in pursuit of the intruder, if it is flying; (2) posture against it in threat display; and (3) active attack, mainly by striking it with the beak. For the first the stimulus is the sight of a small bird flying in the territory; for the second that the object should have red feathers similar to those of a robin's breast; and for the third that it should have the shape of a robin. The observations that most clearly showed that the behaviour was of the kind analysed by Lorenz was that the birds would posture against a patch of breast feathers taken from a stuffed robin, when this patch of feathers was set up alone. They would both posture against and attack a whole stuffed robin in spite of there being no response, and, once, the attack was so violent that the head of the stuffed bird was knocked off. The only object that possessed these characters and was not attacked was the bird's mate. Otherwise, the reactions were general and stereotyped, with only a few exceptions.

Many other examples of this type of behaviour in birds are given by Lorenz [2] and may be found elsewhere in the literature.

Russell [6] has recently discussed these aspects of animal behaviour. In his phraseology characters that are competent to initiate action—Lorenz's releasers—are called valent for the action initiated. Other features of these valent characters are that they may be valent only in certain surroundings, as a nestling in but not out of the nest, and that the same character may be valent in different circumstances for different types of action. He shows in addition that animals of other groups besides birds may recognise objects in this way. He concludes (1940–41, p. 267): "We must carefully guard against the fallacy of assuming that animals perceive objects in the detailed and complex way that we do, as continuing 'things,' recognisable by a large number of associated characteristics; the evidence tends to show, on the contrary, that animals may attend to and perceive only one or few characters of objects. That is why their perception sometimes leads them astray."

This last sentence from Russell brings us back to the problems of mimicry. These views of bird behaviour imply that, so far as the behaviour is of this type, the bird will confuse the mimic and its model if they both possess the same valent characters, and not necessarily if they are similar in general form and colour. In butterflies and other insects we have no knowledge of the nature of the characters that are valent for birds. They may be features of the flight, of the form or coloration of the body, of its smell, or of any other character of its biology, but it is their presence or absence that will determine the selective value of the resemblance. And this will be true of protective resemblance as well as of mimicry;

for a bird will also recognise its prey by valent characters. Here, however, there is the antecedent requirement that a motionless prey must stand out sufficiently from the background to be recognised as a discrete object. Much of protective resemblance is clearly adapted to prevent this [7].

If these conclusions are accepted, the question arises, why the very perfect resemblances, as they seem to us, that are so often found in both protective resemblance and mimicry, should have been evolved. Though one or at most a few valent characters are used by a bird at any one time for recognition, it is unlikely that the same characters will be used by all birds. Thus, a resemblance will be more effective the more perfect it is, for a good resemblance will contain the valent characters for many species of bird. Nevertheless, the slight protection given by the possession of characters valent for only one species of bird will have selective value [8]. It is also probable that the valent characters used by a bird may alter in time; that if one valent character is mimicked the bird may learn some other character and this may become valent. This second character may be then mimicked and by a repetition of this process a general resemblance may be built up.

These considerations do not provide any answer to many of the problems of mimetic and other resemblances, but they do make the theory of their evolution easier in some ways. They finally remove the difficulty that has so often been felt in understanding the initiation of these resemblances. Since a simple and very partial resemblance may deceive a bird, the argument that a bird's eye is too good to be deceived by any but a more or less perfect resemblance is no longer valid. The jackdaw's eye is undoubtedly good enough to see the difference between a bathing-dress and another jackdaw, but the difference was not *perceived*. However good the eye may be, only the resemblance will be perceived if it is in the valent characters.

These views may also help to explain the genetic origin of the resemblances. Simple resemblances as are here postulated might easily arise as genetic variations, and a later general resemblance could be built up of many such variations, whether they are due to mutations, modifying genes or position effects. On the other hand, a good general resemblance due to a single genetic factor, as seems to occur in the species of *Papilio* [9], will also be effective if it includes the valent characters.

The purpose of this note, however, is not to provide a theory of mimicry and protective resemblance, but to suggest that these phenomena should be discussed in the light of the facts of animal behaviour as we now know them, and not on the assumption that predatory animals behave as we do.

I have to thank Dr. W. H. Thorpe for help and criticism in writing this article.

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NOTE

The Editors regret that, owing to lack of space, Mr. F. I. G. Rawlins' article entitled "A 'Directive' Philosophy of Paintings" has had to be held over until the October Number.

RECENT ADVANCES IN SCIENCE

ASTRONOMY. By A. Hunter, Ph.D., F.R.A.S., Royal Observatory, Greenwich.

The astronomer is perforce an internationalist, if only because not all of the sky is visible from a given point on the Earth's surface ; and in a world war, astronomy is one of the first casualties of organised scientific research when international programmes of observation are cut short. Even the less ambitious research is at a discount when national service of one kind or another takes alike the observer from his telescope and the mathematical astrophysicist from his desk. But the recent world conflict has interrupted astronomical work even more than these factors would indicate, for the fear of indiscriminate bombardment from the air—a fear only too well justified in the event—early led to the dismounting of the bigger instruments in many European observatories. Later, Russia alone suffered the destruction of no fewer than nine observatories, some in ground fighting, others apparently from sheer malice. When still later the U.S.A. were drawn into the struggle, though no dismantling proved necessary, the energies of the younger men in the big American observatories were diverted, as in Great Britain, to the study of ballistics, to operational research, to navigational instruction and to the multitude of tasks in the physical laboratory which have made this war as truly the physicists' war as the previous one was the chemists'. Only a skeleton staff, consisting of the older astronomers, was left to carry forward greatly reduced programmes.

But the mills of total warfare, though they grind exceeding small, grind slow, and since these notes last appeared many a programme planned years before the war has been carried to a successful conclusion. We cannot here do more than scan in hasty review some of the more interesting results of the past seven years. If in doing so we find that solar physics seem to occupy a disproportionate amount of our time, there are very good reasons for this circumstance. First, solar work is done with small instruments which it has not been necessary to dismount for safety ; and moreover, it is done in friendly daylight, a factor not likely to be underestimated by those who have endeavoured, from beneath steel helmets, to get time observations between zenith passages of hostile aircraft. Second, neutral countries such as Switzerland and Sweden have long been in the forefront in solar research, and have used their good fortune to consolidate their position. Third and perhaps, alas! most important, solar research has itself recently become a matter of interest to belligerent countries. Both the U.S.S.R. and, as it now appears, Germany, set up special organisations in an attempt to correlate, for prediction purposes, solar phenomena with those ionospheric factors which control telecommunication; and if this country's reaction was less apparent, that is not to say that co-operation was lacking between the service departments and the solar observers.

Let us start, then, with the Sun. On the observational side, coronagraphs have been in use throughout the war by Waldmeier in Switzerland and by Lyot in the Pyrenees. The Swiss results point to a connection between coronal activity (mainly abnormally high intensity of the green emission line in the coronal spectrum) and those recurrent geomagnetic storms which have hitherto defied association with any visible solar disturbance. If this connection is confirmed by further study, preferably over a complete solar cycle, warning of this type of magnetic storm may become possible six or seven days ahead, an advance which the telecommunications engineer will welcome. Lyot's recent work has been concerned with the application to coronal study of an ingenious optical filter with which he has obtained the first monochromatic pictures of the corona in the light of its own emission lines. The filter, of which the principle has been used independently in Sweden and also in the U.S.A. (under the magnificent name "interference polarising monochromator") employs six quartz plates and seven polarisers transmitting, between the red and the violet, 13 bands two or three angstroms wide arranged to pass the principal chromospheric and coronal lines. Simultaneous cinematograph pictures in the red and green coronal lines fail to confirm the motions reported in the corona by previous observers. Even when the film is shown speeded up 2400 times, the corona remains stationary: arches and jets appear and fade along invisible trajectories, thus modifying the shape and aspect without perceptible motions.

An entirely new instrument called the coronaviser is being developed in America and may come to rival the optical coronagraph in time. It employs a television scanning system in which the light intensity around the Sun is scanned in a spiral path.

The uniform glare of diffused sunlight is then transformed by standard electronic methods into a direct current, while variations in intensity due to prominences and coronal features give rise in the scanning process to small alternating currents which can be amplified. In the final picture on the screen of a cathode-ray tube the unwanted glare has thus been filtered out, leaving only the chromospheric and coronal detail.

Another war-time development in observational work on the Sun has been the striking confirmation of connections, hitherto little more than suspected, between solar flares and certain geophysical disturbances. This work, contributed to by many physicists, astronomers and geophysicists, has been systematised by H. W. Newton of Greenwich in a series of papers which have won him the 1946 Jackson-Gwilt Medal of the Royal Astronomical Society. A solar flare, until recently called a chromospheric eruption in spite of the fact that it generally shows no outflow of matter, is manifested by short-lived activity in a limited area of the chromosphere, usually near a spot. The activity is shown by abnormal emission in the spectrum lines of hydrogen, helium and certain metals, and its progress is ordinarily followed by observations with the spectrohelioscope. Over two dozen spectrohelioscopes were in use before the war over the whole world, and given good weather, the Sun was under observation by at least one instrument at a time throughout the twenty-four hours. The emission usually lasts for less than an hour and is evidently accompanied by other radiation whose exact nature is not yet known. Some of this travels in all directions at the speed of light, and penetrates the Earth's atmosphere as far as the D region of the ionosphere (height 70–100 km.), where it causes increased ionisation over all the sunlit hemisphere ; some is emitted from the flare roughly radially in the form of particles travelling at about 1600 km. per sec. Among the consequences of the increased D-layer ionisation are abnormal attenuation, frequently reaching the stage of complete fade-out, in short-wave radio communication on the daylight side of the Earth, and disturbances in the geomagnetic elements in the same sense as the ordinary daily swing of the magnetic needles. Both effects appear and disappear with the visible flare. The first is a matter of some importance to the radio engineer, who is faced on these occasions with so abrupt a cessation of signals that mechanical breakdown is sometimes suspected. The corpuscular bombardment may cause widespread magnetic storms and auroræ when the particles reach the Earth the day after the visible flare ; but this normally happens only if the generating flare occurs within about 45 degrees of the centre of the Sun's disk. Accompanying these magnetic storms are ionospheric storms still more troublesome to radio communication than flares, not because the interruption to signals is so complete (it is not, usually) but because the disturbance may last for days, especially on channels utilising the polar regions of the ionosphere. These effects are distinct from those noted above as apparently connected with coronal activity: the storms rarely recur as the Sun rotates, and they are so closely connected with flares that, given favourable geometrical conditions and a flare of sufficient intensity, the astronomer may venture on a twenty-four-hour prediction with a fair chance that the Sun will not let him down.

The nature of the electromagnetic radiation causing the D-region ionisation is as yet unknown. The difficulty is that radiation of wave-length longer than about 1000 A. cannot ionise the nitrogen and oxygen molecules and the oxygen atoms which are believed to be the main constituents of the atmosphere in the D region; and likely-looking shorter wave-lengths are so short that they ought to be absorbed before they penetrate to the D region. Fundamentally the trouble lies in the imperfections in our knowledge of the photochemistry and discharge chemistry of the upper atmosphere—a deficiency which is being repaired so rapidly that the next few years may well see the whole matter cleared up. The nature of the corpuscular radiation is not quite so obscure, for both theory and observation give us hints. As long ago as 1926, Milne pointed out that a solar atom which once begins to move upwards in the Sun's atmosphere will be subjected to more intense radiation than a stationary atom, since the Doppler effect of its outward velocity causes it to absorb on the violet wing of its absorption line. An initial balance between radiation pressure and gravity is thus upset in favour of the former, and the atom will be accelerated upwards, " climbing out " of the absorption line until it is exposed to the full continuum. A limiting velocity of about 1600 km. per sec. is reached when the inverse-square weakening of the radiation prevents further acceleration. This hypothesis of vertical instability of rising atoms thus gives theoretical grounds for expecting the ejection, from local disturbed areas of the Sun's disk, of particles taking about a day to reach the Earth. In 1929 Chapman drew attention to the possibility of detecting these particles directly by comparing solar spectrograms taken at times of magnetic quiet with those secured in periods of magnetic disturbance. During the latter periods the Earth is probably enveloped in a stream of solar particles for hours or even days, and the whole of the Sun's disk must be viewed through a great length of the stream. Now it is the basis of Milne's hypothesis that the atoms of the stream are of the same kind and in the same condition of excitation as those producing the Fraunhofer lines. After leaving the Sun, then, they will continue to absorb, but in light of wave-length modified by the Doppler displacement corresponding to their velocity of approach towards the Earth. Thus, just before the onset of a magnetic storm and during its development, the strong absorption lines in the Sun's spectrum should be bordered on their violet sides by absorption features whose mean displacement measures the mean velocity of the particles, whose width measures the velocity dispersion, and whose depth measures the number of absorbing particles in the line of sight. It is now reported that spectrograms taken at Mt. Wilson near the H and K lines during two violent magnetic storms, when compared with those taken during a magnetic calm, give some indication of two very shallow absorption bands beginning near the centres of the lines and extending about 12 A. towards shorter wave-lengths. The maximum depth is about 1 per cent. of the continuum, and the displacements indicate maximum velocities of about 1000 km. per sec., and mean velocities of the order of 600 km. per sec. The cautious wording of the announcement suggests that the results are preliminary, but confirmation may be obtained during the coming solar maximum. This is the first direct evidence for the existence of corpuscular radiation from the Sun in interplanetary space.

Theoretical solar work during the war is dominated by a major discovery which has won Professor B. Edlén of Lund University the 1945 Gold Medal of the Royal Astronomical Society. This is none other than the identification of the origin of the emission lines exhibited by the inner solar corona. These lines have been a mystery that has baffled astronomers for the seventy-five years or so that have elapsed since they were first observed. Edlén's explanation, in removing the mystery, substitutes for it a revolutionary conception of the physics of the outer solar envelope. He attributes the lines to the ions of iron, nickel, calcium and argon in very highly ionised states corresponding to excitation temperatures of millions of degrees. The electronic jumps involved are those known as forbidden transitions, *i.e.* inward transitions from metastable states in which the atoms concerned cannot rid themselves of their energy except by radiating wave-lengths which are ordinarily barred to them. In the laboratory, or even in ordinary stellar atmospheres, an atom or ion in such a state is promptly removed from it by collision with another particle or by absorption of radiation; in the corona the pressure is apparently so low, and the radiation necessary is of such short wave-length, that metastability persists long enough for forbidden transitions to occur.

Edlén's identifications are made either from term values known from the extreme ultraviolet spectra of such atoms as calcium 11 or 12 times ionised, or iron with 9 or 10 outer electrons removed, or else from term values for other ions deduced by extrapolation. In neither case have the lines actually been observed in the laboratory —as yet our technique is limited to producing these ions momentarily in high-voltage vacuum sparks : we have no means of keeping them isolated till they radiate their forbidden lines. In the former case, however, the numerical coincidences are completely convincing, and in the latter the circumstantial evidence is so strong that few astrophysicists will dispute Edlén's claim. To the objection that the theory is revolutionary he can retort with justice that nothing but a revolution could explain the coronal lines, for all the more conventional lines of approach have been tried and tried again in the past three-quarters of a century. But his work, brilliant though it is, has only pushed the puzzle back a stage. We are now faced with the problem of how excitations of hundreds of electron volts can exist in the outer atmosphere of a cool star like the Sun, when at its surface the excitation rarely rises above two or three electron volts. It is a measure of the stimulus provided by Edlén's work that several explanations have already been advanced. Perhaps the one to which least exception can be taken is that due to Saha, who, it will be remembered, was faced with and solved a qualitatively similar discrepancy in excitation twenty-five years ago, when he produced the ionisation theory which gave low pressure its proper place in atomic excitation. Saha suggests that nuclear fission occurring in heavy atoms near the solar surface can give rise to high-velocity nuclear fragments, some of which, starting life as iron atoms lacking 15 electrons, pick up electrons as they shoot through the chromosphere, and emit the coronal lines (most of which are due to iron) farther out. The essence of this theory (and indeed, one may conjecture, of any other which can hope to explain the origin of the coronal stripped ions) is the rejection of the idea of a general excitation of the whole coronal region by any thermal or photoelectric process and the substitution of a purely local excitation. It is as if a Martian observer, familiar with the Earth's meteorology, were faced with the problem of explaining a peculiar spectrogram of the terrestrial atmosphere secured on Guy Fawkes' night. He would be on the right track only when he abandoned any idea that the observed excitation was characteristic of the whole atmosphere and substituted a theory based on a strictly localised luminosity.

Another major development in solar theory concerns the source of the energy which the Sun, in common with other stars, pours so lavishly into space. Astronomers have long since rejected chemical combination, gravitational contraction and "natural" radioactivity as possible sources: the first is ridiculously inadequate from any point of view, the second is inadequate through geological time, and the third, though not inadequate, is contradicted by currently accepted ideas on the constitution of solar matter. The only likely source is the transformation of mass m into energy E according to the relation

$$E = mc^2 \quad (c = \text{velocity of light})$$

proposed by Einstein nearly half a century ago. But until the beginning of the war no detailed mechanism had been put forward, though it seemed likely that many nuclear reactions would be exothermic to the required degree. The position was that astrophysicists had developed the theory of stellar structure as far as was possible without knowing in some detail what the mechanism of energy generation was. Eddington's work indicated for the central temperature of the Sun about 20 million degrees, a temperature at which atomic nuclei can approach one another sufficiently close, despite mutual repulsion, for collisions to result in transmutations. If those modern alchemists, the nuclear physicists, could suggest a thermonuclear reaction occurring in these conditions and violating the conservation of mass sufficiently to account for the observed energy output, the problem was solved. A detailed examination by Bethe of the nuclear processes likely to be important inside the Sun led him to suggest the following cyclic process, in which the helium nucleus is built up from four protons, with the catalytic aid of carbon and with the release of mass-energy in the form of γ-radiation:

$$\begin{aligned}
{}_6C^{12} + {}_1H^1 &\rightarrow {}_7N^{13} + \gamma \\
{}_7N^{13} &\rightarrow {}_6C^{13} + {}_1e^0 \\
{}_6C^{13} + {}_1H^1 &\rightarrow {}_7N^{14} + \gamma \\
{}_7N^{14} + {}_1H^1 &\rightarrow {}_8O^{15} + \gamma \\
{}_8O^{15} &\rightarrow {}_7N^{15} + {}_1e^0 \\
{}_7N^{15} + {}_1H^1 &\rightarrow {}_6C^{12} + {}_2He^4
\end{aligned}$$

All these reactions were observed in the laboratory in the early part of the war, but a preliminary solar model in which the energy was assumed to be produced by the above process in a convective core containing 11 per cent. of the total matter at a temperature of 26 million degrees and a density of 110 gm. per c.c. failed to reproduce the solar luminosity by a factor of nearly 150. Other conceivable processes failed by a much bigger factor, however, and at the expense of a central density (45 gm. per c.c.) much lower than had previously been assumed, more recent calculations using improved values of the opacity of solar matter have removed this discrepancy and reproduced the observed energy outflow almost exactly. Whether such a model is the only possible one and even whether it is itself entirely consistent with other astrophysical data is a matter for the future to decide.

Turning now from the Sun to the solar system, we must mention first of all the completion by the Astronomer Royal of a monumental piece of work made possible only by international collaboration of the closest kind. In 1931 the minor planet Eros made an exceptionally close approach to the Earth: 16·2 million miles as against a possible 13·8 million miles. Such an event made feasible a redetermination of the solar parallax, or what is equivalent, the distance from the Earth to the Sun, the fundamental unit of astronomical distance. For this purpose, it will be remembered, the distance of a nearby member of the solar system is measured, and the rest follows by the application of Kepler's laws. The extent of the observational material will be realised when it is mentioned that the measurement of the photographic plates and the reduction and discussion of the measures took ten years. The final value of the parallax is 8·790″ ± 0·001″, which corresponds to a mean distance of 93,005,000 ± 11,000 miles from the Earth to the Sun.

The 82-inch reflector at the new McDonald Observatory in Texas has recently been used for a survey of the atmospheres of planetary satellites. The most interesting result has been the detection, from red and infra-red spectrograms, of methane and possibly ammonia on Titan, the principal satellite of Saturn, sixth in order of distance from the planet. This is the first certain evidence of an atmosphere on any planetary satellite, and it raises some interesting questions. For instance, the diameter and mass of Titan are of the same order as those of the Jovian satellites Callisto and Ganymede; of Triton, the satellite of Neptune; and indeed of our own Moon. The question arises: Why are there no atmospheres on these bodies, which have velocities of escape comparable with that of Titan? In the case of the Moon, the molecular root-mean-square velocity at the prevailing temperature is too high for an atmosphere to be retained. But at about 90° K., the probable temperature of Titan, only hydrogen and helium will escape. There is thus no difficulty in accounting for an atmosphere of the composition found, if it has been picked up at a late evolutionary stage, *i.e.* since the satellite has been as cold as this. Jupiter's major satellites are only some 30 degrees higher in temperature, however, and it is difficult to see why they should not have detectable atmospheres. It seems likely that the atmosphere of Titan is a relatively recent acquisition, and that Jupiter's satellites have not encountered the conditions necessary to acquire similar atmospheres, though they could retain them if they did. It is perhaps not irrelevant to note that the discovery makes Saturn unique in possessing a satellite with an atmosphere; it is already, of course, unique in possessing a ring system.

Another important spectroscopic advance is the identification of the famous "4050 group" of bands in cometary spectra. The identification was first made from the structure, which conforms best with a triatomic molecule XH_2, where $X = C$, N or O. The band was then actually produced by a discharge through rapidly streaming methane in conditions which make it almost certain that the emitter is CH_2.

An exhaustive investigation of some 1250 meteor observations by Porter would appear to have laid the ghost of hyperbolic meteor orbits once and for all. Meteor velocities from two to three times the Earth's orbital velocity were reported some years ago, and these, of course, signify the approach of these bodies from outside the solar system. Porter's work is based on a rigid statistical analysis of observational material of known accuracy conducted without the assumption made in the earlier work that the directions of approach are uniformly distributed in space. It shows no evidence of hyperbolic orbits; indeed, the speeds observed are only about two-thirds the parabolic values, a circumstance which is attributed to atmospheric retardation before the meteors become luminous enough to be seen.

Extending our view now beyond the solar system, perhaps the most spectacular discovery of the war years is that of non-solar planets. The motions of two nearby stars (61 Cygni and 70 Ophiuchi), both of them, as it happens, binaries, show peculiarities which can only be explained by assuming the presence of planetary companions. Photographs of 61 Cygni, hitherto regarded as a system of only two components revolving round their common centre of gravity in 720 years, show small periodic deviations from Keplerian motion suggesting the presence of a companion of only one-sixtieth the mass of the Sun, or sixteen times that of Jupiter. This is about one-tenth that of the lightest known star, so the companion is probably better classified as a planet than as a star. Every five years it describes round one of the main stars of the system an orbit about the size of an asteroid orbit in our own solar system. Similar periodic discrepancies in the behaviour predicted for the components of 70 Ophiuchi suggest that here the mass of the companion is even smaller. A planet of about one-hundredth the mass of either of the main components circles around one of them (it is uncertain which) every seventeen years in an orbit about the size of Jupiter's. If planetary systems are as frequent as this—at least 3 out of 40 stars known to be within 17 light-years—our theories of the origin of our own solar system may have to be revised.

Interstellar space has been forced to yield up another of its secrets by the identification of the sharp " detached " line at 4300 A. as due to the molecule CH. That the identification is correct is proved by the classical test of all hypotheses : the successful prediction of other effects. After the identification, a number of other and fainter lines due to CH were predicted by McKellar of the Dominion Astrophysical Observatory, Victoria, and found at Mt. Wilson in the spectrum of ζ Ophiuchi, a distant star showing strong interstellar lines. It is likely, too, that interstellar space contains CN and NaH molecules. The theoretical astrophysicist is now set the problem of explaining the fact of molecular association in the physical conditions of interstellar space ; whilst the observer sees with satisfaction the 4300 A. line, as well as the coronal lines and the 4050 A. group in comets, crossed off the list of unidentified celestial spectra.

Instrumental developments during the war call for little mention. The mounting of the 200-inch telescope awaits its mirror on Mt. Palomar. Though the grinding is finished, the figuring, always a tedious business because of the lengthy delays necessary for cooling between polishing and testing, has been held up by the pressure of essential war work. In another direction the war has forced astronomers' hands. Demand for a time service capable of giving twenty-four-hour intervals to the 0·001 sec. accuracy required for checking precision radio frequency standards has led to the abandonment of mechanical clocks as the basis of the Greenwich time service. Quartz crystal clocks have now assumed the duties hitherto performed by Shortt free pendulums except so far as short-period extrapolation is concerned. Quartz clocks consist essentially of phonic motors controlled *via* frequency dividers by quartz crystals maintained in vibration at their natural period of oscillation (some 100,000 cycles per sec.) by electronic means. When their teething troubles—notably a tendency to stop at intervals of a few days or weeks—have been overcome, it is likely that the national time service will depend wholly upon them. Time signals giving twenty-four-hour intervals to an accuracy of a tenth of a millisecond are already the target, albeit a distant one.

Lest the opening paragraphs of this review should have conveyed too gloomy a picture of the effect of the war on astronomy, let us conclude by enumerating a few of the possibilities which war-time research has opened up. The "blooming" of optical surfaces to reduce waste reflections will allow optical computers of the future to use many more components in their designs than hitherto, and will reduce long exposures at the telescope by many a weary hour. The scientific use of rockets of the V2 type will, it is hoped, enable us at last to explore the solar spectrum below the limit set by ozone absorption at 2900 A.—a development which the meteorologist will welcome as much as the astronomer. Radio receivers working in the radar range are already being used to obtain reflections from the Moon and perhaps other bodies of the solar system, and to detect emission from the Sun and the Milky Way. And finally, astronomers look towards the development of electronic devices in the confident hope that they may do in many branches of astronomy what they have already done in microscopy.

PHYSICS. By F. A. Vick, O.B.E., Ph.D., F.Inst.P., The University, Manchester.

The Luminescence of Solids.—In this article we shall not be concerned with chemiluminescence or triboluminescence, but with the emission of visible or near-visible radiation due to the absorption of energy from incident light, X-rays, gamma rays, electrons, alpha particles, etc. Luminescence, in which radiation is emitted only during the absorption of energy or within a microsecond afterwards, is called "fluorescence," and that occurring during "afterglow," lasting from microseconds to days after the absorption ceases, is known as "phosphorescence." Until the decade before the war, the preparation of luminescent materials was almost a culinary art, and most experiments did not seem susceptible to theoretical interpretation, but during that decade much careful experimental work and parallel progress in the general theory of electron energy states in solids were beginning to revolutionise the subject ("General Discussion in Luminescence," *Trans. Far. Soc.*, **35**, 1939; J. T. Randall and M. H. F. Wilkins, *Phys. Soc. Rep. Prog.*, **6**, 174, 1939; N. F. Mott and R. W. Gurney: *Electronic Processes in Ionic Crystals*, 1940). Stimulated by applications to cathode ray tubes and gas discharge tube lighting, progress has continued during the war, and some of the results are now appearing in various publications. It therefore appears appropriate to summarise recent work and the present views on the theory of fluorescence and phosphorescence, to serve as a background to papers reporting further progress.

Two methods of measuring luminescence due to excitation by U.V. have become fairly standard (*e.g.* J. T. Randall and M. H. F. Wilkins, *Proc. Roy. Soc.*, **184**, 347, 1945). In a modern form of the phosphoroscope, a disc is rotated about a horizontal axis at a known speed, and the specimen is mounted in an annular region near the circumference. U.V. from a quartz mercury vapour lamp is selected by a quartz monochromator and focussed on a small patch of the annulus. A vacuum photocell, connected to a suitable amplifier, is mounted in a shielded box on an arm carried on an axis in line with the axis of the disc. A divided circle allows the position of the photocell slit to be measured to within about 0·1 degree with reference to the exciting patch. The speed of the disc is adjusted so that the luminescence decays fully within one revolution. The photocell current at different angular distances from the exciting patch is a measure of the luminescent afterglow intensity at corresponding times.

In the second method, the U.V. radiation falling on a specimen from a constant source is regularly interrupted by a rotating sector. The luminescent light from the specimen is focussed on to the cathode of an electron-multiplier photocell, and the amplified output fed to a cathode ray oscillograph. The advantage of this method is that both rise and decay curves are seen on the screen and can be photographed, and the rise curve can sometimes give more information than the decay. For studying luminescence due to electron bombardment, the U.V. beam and rotating sector are replaced by an interrupted electron beam which strikes the specimen, as in a cathode ray tube, but a complication which may enter is the charging up of the screen with consequent alteration in the effective energy of the incident beam, and this must be measured or eliminated. (E. R. Piore and G. A. Morton, *J. Appl. Phys.*, **11**, 153, 1940.)

The interpretation of the experimental results depends on the theory of electronic energy states in non-metals, which is based largely on the work of A. H. Wilson (1931) and extended by Slater, Mott, Seitz, and others. The familiar energy levels of isolated atoms and molecules are broadened into bands in solids. In an insulator these bands are normally either completely full or are empty, and a full band is separated from an empty one by a forbidden zone (see Fig. 1). While electrons may move within the full band, it follows from the Pauli exclusion principle that such movement must be accompanied by equal electron movement in the opposite direction, and there is no net current. For the solid to conduct there must be sufficient absorption of energy to raise electrons from the full band A to the next lowest empty band C. An electron in the conduction band is separated from its parent atom, and is in this sense free, so the process is analogous to ionisation. The absence of a negative charge in the parent atom is sometimes called a " positive hole," which can be filled by migration of another electron in band A. This is equivalent to the motion of the positive hole in an opposite direction, contributing to the conduction. Unless there is a permanent photochemical change, an electron in the conduction band must eventually return to the band A, though not necessarily directly or to its parent atom. It was shown by Peierls in 1932 and since confirmed that there may exist associated with individual atoms or ions (luminescent centres) isolated levels (excitation levels, E) in the forbidden zone B. Electrons raised to these levels are still bound to the parent atom, and the solid is not made conducting. On returning to band A, the energy is given up as radiation or as heat. Electrons in the conduction band may fall to levels E and thence to band A. They may also fall to " electron traps," T, further levels from which they cannot escape until raised to the conduction band when supplied with the necessary quanta from the thermal energy of the solid. These traps are associated with impurities in the solid or irregularities in the lattice. In the case of the alkali-halides, they are the F-centres studied by Pohl, and have been ascribed by Mott and Gurney (*loc. cit.*) to points in the lattice where a negative ion is missing.

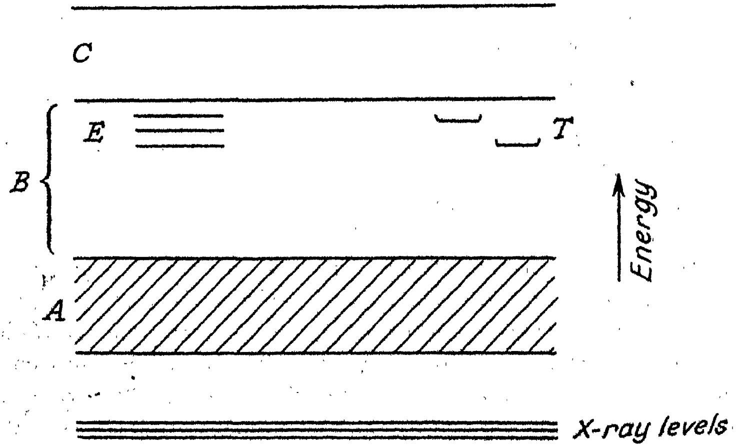

FIG. 1.

We can now consider the possible ways by which the absorption of energy can lead to luminescence. (*a*) The raising of an electron from band A to one of the excitation levels E in the same atom, and its subsequent return to the ground state. (*b*) The movement of an electron to the conduction band C and return through the levels E of the same or a different atom of the solid, perhaps after spending appreciable time in the conduction band. While the electron is in the conduction band, the positive hole left may also move through the phosphor. (*c*) The capture of an electron in the conduction band by an electron trap T. The electron can then return only after acquiring sufficient thermal energy to raise it to the conduction band again. In all these cases, the electron returns to the ground state through the excitation levels E, and gives up energy corresponding to the difference in level between E and the ground state. This energy may be given up by the emission of radiation or as heat. The energy of the emitted quantum cannot, of course, be greater than the absorbed quantum, and in general it is less (Stokes' Law). A diagram used by Seitz (*Trans. Far. Soc.*, **35**, 74, 1939) is helpful here (Fig. 2). The normal atom will have its equilibrium position at the state of lowest energy, represented by the point A. The upper curve in each diagram represents the potential energy—distance curve for the excited atom, with its minimum B at a different relative distance. According to the Franck-Condon principle, atomic nuclei remain at rest during any optical transition, so the absorbing quantum $h\nu_1$ is represented by AA′. The energy of the excited atom then falls to the equilibrium state B by the loss of energy A′B as heat. The re-emitted quantum of radiation is represented by BC $= h\nu_2$, and finally there is a loss

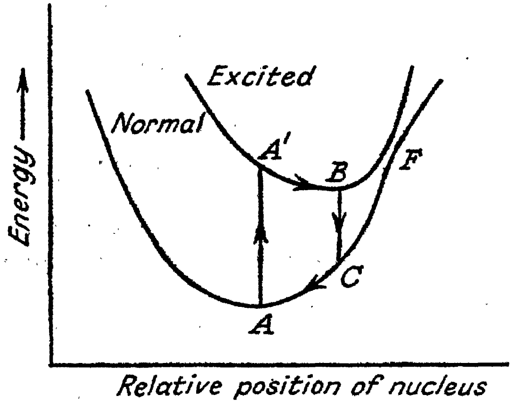

FIG. 2.

of energy as heat CA as the atom returns to its normal energy state. It is evident that $h\nu_2$ is less than $h\nu_1$. According to Peierls, it is possible for the transition BC to occur without radiation by setting neighbouring atoms in vibration. This means the absorption of a large number of vibrational quanta at once, and the probability will not be high, though increasing at higher temperatures. N. F. Mott (*Proc. Roy. Soc.*, **A. 167,** 384, 1938) has considered another method whereby excited phosphors may dissipate their energies as heat, and that is a transition from one state to another at the point F (Fig. 2), where the two energy curves are close together (they cannot cross). The probability for this transition is $A^{-W/kT}$, where W is the energy BF, the probability again increasing at high temperatures. We thus see that there may be a drop in the intensity of luminescence with rise in temperature, and this has been found with some substances. These considerations apply to all mechanisms (*a*), (*b*) and (*c*), and we must now examine the differences between them.

(*a*) *Electrons in Excited States Only.*—Since the electrons need not enter band C, the solids will not be photoconducting (except perhaps at high temperature). The intensity of luminescence will be proportional to the rate at which electrons return to the ground states. If n = the number of electrons per unit volume in a level E,

$$I = -\frac{dn}{dt} = pn, \text{ and } I = I_0e^{-pt} \quad . \quad . \quad (1)$$

where p is the probability of an electron returning to the ground state. The dimensions of p are sec.$^{-1}$. The decay curve is thus exponential, of form determined by p, but independent of intensity and duration of the exciting radiation. For normal optical transitions, p will be of the order of 10^8 sec.$^{-1}$, and for "forbidden" transitions, about 10^4 sec.$^{-1}$ (Mott and Gurney, *loc cit.*, p. 204). The form of the decay curve is characteristic of the atom concerned, and may be largely independent of the material of the solid as a whole.

(*b*) *Electrons in Conduction Bands.*—If there are no free electrons or empty luminiscent centres (positive holes) in the unexcited solid, the number n of electrons in the conduction band will equal the number of empty centres, and the probability of an electron from the conduction band combining with a luminescent centre will be An^2, where A is a constant.

$$I = -\frac{dn}{dt} = An^2$$

$$\frac{1}{n} = At + \text{const.} \qquad n = \frac{n_0}{1 + n_0At} \quad (n = n_0 \text{ at } t = 0)$$

$$I = An^2 = \frac{An_0^2}{(1 + n_0At)^2} \quad . \quad . \quad . \quad (2)$$

Equation (2) is characteristic of a bi-molecular reaction. When $t = 0$, $I_0 = An_0^2$

$$\frac{1}{\sqrt{I}} = \frac{1}{\sqrt{I_0}}(1 + An_0t)$$

so that $1/\sqrt{I}$ is a linear function of the time (hyperbolic decay law). Phosphors for which this mechanism holds will be photoconducting in an applied field. The rate of decay depends on the initial intensity (*i.e.* on n_0) so that high intensity luminescence of this form has a rapid initial decay.

If there are initially m vacant luminescent centres (or positive holes), we have instead

$$I = -\frac{dn}{dt} = An(n + m)$$

the solution of which is

$$n = \frac{m}{Be^{mAt} - 1}, \quad B = 1 + \frac{m}{n_0}$$

When t is large, the decay is exponential, since

$$n = \frac{m}{B}e^{-mAt}$$

If initially $n_0 \gg m$, the decay is hyperbolic.

(*c*) *Electron Traps.*—If the energy level of a trap is V electron volts below the conduction band, V is the trap depth, and the probability of an electron with thermal energy kT escaping from the trap is $s.e^{-V/kT}$ where s is a constant of the order 10^8 sec.$^{-1}$. If there is no "retrapping" the rate of escaping is

$$\frac{dn}{dt} = -nse^{-V/kT}$$

If the time spent in the conduction band is negligible, this will be proportional to the intensity of luminescence. On integrating

$$I_t = I_0 \exp. [-ste^{-V/kT}]$$

for single trap depths V. This is, of course, highly temperature dependent. Trap depths may, however, cover a wide range of energies, and the form of the decay curve and its temperature dependence will depend on the distribution of traps.

Experimental Results.—It is a remarkable fact that very few pure solids are luminescent. J. T. Randall has suggested (*Trans. Far. Soc.*, **35**, 2, 1929) that pure solids will fluoresce only if unit cells of the lattice contain ions or co-ordination groups in which there are incomplete shells of electrons fairly well screened from neighbours. In such cases the incomplete shells act almost as if they were free atoms, and emission spectra will be fairly sharp (depending on the sharpness of the levels E in Fig. 1). If the fluorescent solid is soluble, the solution also will be fluorescent. The mechanism of fluorescence is that of class (*a*) considered above. Uranyl salts, platinocyanides and some rare earth salts belong to this class. The great majority of phosphors depend for their behaviour on the presence of quite small amounts of impurities. So-called pure substances which have to be activated by heat treatment really belong to this type, since the activation leads to stoichiometric excess of one of the constituents—*e.g.* excess of Zn in zinc sulphide (F. Seitz, *J. Chem. Phys.*, **6**, 454, 1938).

J. T. Randall and M. H. F. Wilkins (*Proc. Roy. Soc.*, **A. 184**, 347, 1945) have measured by the methods described above the luminescence of a range of solids containing manganese impurity, and have also measured the photoconductivity of the solids. They found that the decay curves were exponential, or almost exponential, of form independent of the intensity of the exciting U.V. This indicates that the luminescence is of our class (*a*), the slight changes from a pure exponential decay being explained by different values of the decay constant *p* for manganous ions in different parts of the crystal. Photoconductivity was, however, observed in most specimens, even in the pure state. Randall and Wilkins suggest that this photoconductivity is not directly associated with the luminescence. The phosphorescence of zinc beryllium silicates and zinc orthosilicates is at first exponential and then slows down to a long tail, which is ascribed to electrons trapped in the neighbourhood of the manganous ion, and subsequently slowly released by thermal vibrations.

In two further papers (*Proc. Roy. Soc.*, **A. 184**, 306 and 390, 1945) Randall and Wilkins go on to the study of electron traps and their effects. They first explore the distribution of electron trap depths by activating with U.V. a phosphor held at liquid air temperature, and then allowing the solid to warm up at a uniform rate. As the thermal energies reach the values at which the electrons can be released from traps of the corresponding depths, the solid emits radiation by the mechanism of class (*c*) above. The "glow intensity" is plotted against temperature, and gives a measure of how the trap depths are distributed in energy. The manganese activated materials having phosphorescent tails to their decay curves were found to exhibit pronounced peaks in their glow curves, indicating the existence of electron traps. Hermann and Hofstadter (*Phys. Rev.*, **57**, 936, 1940) have shown that manganese-activated zinc silicate exposed at liquid air temperature to U.V. shows a sudden increase in conductivity at 213° K. This temperature corresponds to that of a peak in the glow curve found by Randall and Wilkins, and indicates that in passing from traps to excitation levels the electrons do go through the conduction bands. The energy distribution of traps in impurity phosphors is complex, and for details the original paper must be consulted.

For phosphorescence with very long decay times, the mechanism of class (*c*) above applies. Measurement of the glow curves gives some indication of the distribution of energy of the electron traps. By extending the theory given above, Randall and Wilkins predicted that for a uniform distribution of traps the luminescent intensity varies as 1/t, and for an exponential distribution the phosphorescence falls off according to an inverse power of the time. These predictions were verified by experiment, alkaline earth phosphors giving the 1/t curve, and zinc sulphide phosphors the inverse power law.

For short-period phosphorescent decays, either class (*b*) or (*c*) mechanisms could, in principle, apply. Many investigators have explained their results in terms of the bi-molecular theory, class (*b*). G. F. J. Garlick and M. H. F. Wilkins (*Proc. Roy. Soc.*, **A. 184,** 408, 1945) have examined this question and have come to the conclusion that hyperbolic and similar forms of decay curves are due to the time spent by electrons in traps, class (*c*), and not to the time in moving through the conduction band. This mechanism may also be combined with time spent in excitation levels E (class (*a*) with " forbidden " optical transitions), giving an initial exponential fall, and a phosphorescent tail, as for some of the manganese activated phosphors mentioned above.

This important paper, and those associated with it, emphasise the value of the conception of electron traps in luminescent solids, as in the study of photochemical change, photoconductivity, etc. It is evident that the general picture is becoming much clearer and that there is a fruitful field for further experimental work linked with theory.

METEOROLOGY. By P. A. Sheppard, B.Sc., F.Inst.P., Imperial College of Science and Technology, London.

Variations in the Field of Atmospheric Pressure.—This subject may fairly be regarded as the most fundamental in dynamical meteorology, and it is for that reason, rather than for any striking progress which has been achieved in it (though progress there has been), that I have chosen it to open the post-war series of articles on progress in meteorology. Let us note here also that in addition to the intrinsic interest of this branch of the subject, progress in the science and art of weather forecasting is quite certainly dependent upon our gaining greater insight into the physical processes in the atmosphere which lead to or make up the phenomenon of development in the field of atmospheric pressure.

The by now well-known work of the Norwegian school of meteorology had up to shortly before the war provided us with a systemised body of knowledge on certain aspects of the inception, growth and decay of extra-tropical depressions formed at and travelling along the polar front. Something regarding the conditions necessary for the development of an unstable frontal wave had been deduced by V. Bjerknes, Sölberg and others, but hardly in relation to the field of pressure about which only a rank empiricism prevailed. This was a thoroughly unsatisfactory state of affairs since, apart from the dynamical significance of pressure at a point, as the weight of the superincumbent atmosphere, pressure is a meteorological variable which can be measured with great and sufficient accuracy and which is, at the earth's surface at least, a much more representative quantity than other variables, such as wind, temperature and humidity. If then we can gain a clearer insight into the mechanism of pressure change we shall be in the position to understand the dynamics and thermodynamics of the atmosphere in a much more profitable sense, in all the meaning of that epithet.

Before dealing with the recent investigations on the genesis of pressure inequalities, a word about tools. Synoptic meteorologists have until quite recently been forced to analyse—as far as they were able—the behaviour of a three-dimensional atmosphere from observations taken at the lower boundary, reinforced by relatively few vertical soundings made rather sporadically from a quite inadequate network of stations. The advent of the radio-sonde and the much increased use of aircraft, particularly over the oceans, have now materially changed the picture so that, in certain quite large areas at least, a reasonably complete set of observations is available. But that is not all. The full value of the additional sounding facilities was only to be obtained by having these soundings made as nearly as possible simultaneously over as large an area as possible and with the vertical soundings made practically coincident in time with the observations made over the network of surface reporting stations of the same area. Only thereby was it possible to build up an adequate three-dimensional picture of the structure of the atmosphere at a given time and to follow the changes with time in that structure.

It is the writer's opinion that this integrated treatment of upper air observations and their marriage with surface observations represents potentially the major advance in meteorology during the last few years. I say " potentially " because the physical processes of the atmosphere are so complex that it would be folly to expect the acutest analysts to extract the full significance of the new charts which may now be prepared at 6- or 12-hour intervals after only a short experience of their use. The new charts referred to are representations of the pressure temperature and humidity fields at a number of horizontal levels, from sea-level up to about 15 km., or, more fundamental, the representation of the heights (contours) of selected pressures from 1000 mb. down to 150 mb. together with the fields of temperature and humidity in the selected isobaric surfaces. Additional charts may be drawn of the topography of the tropopause, and vertical cross-section diagrams in selected directions showing the dry-bulb and wet-bulb temperatures in the vertical together with the frontal topography and structure. Finally, in the matter of tools, reference should be made to the systematic inclusion of observations of wind in vertical soundings. Here Great Britain may fairly claim to have been one of the leaders, having now for several years obtained regular observations of wind, independent of weather, by radio methods whose accuracy has steadily increased. Such observations are a vital part of the information on which analysis in the chosen levels is based, and as deeper insight into weather processes is gained these observations are likely to prove of increased usefulness.

The variation of pressure in time at any point in the atmosphere is readily derived, in terms of the fields of density and of the motion, from the concept of pressure as the weight of the superincumbent atmosphere and from the equation of continuity. Thus, with p = pressure, g = gravity, ρ = density, all at height z, and t = time

$$p = \int_z^\infty \rho g \, dz \quad . \quad . \quad . \quad . \quad (1)$$

and

$$\partial p/\partial t = \int_z^\infty g \frac{\partial \rho}{\partial t} \, dz.$$

The equation of continuity is

$$\partial \rho/\partial t + \nabla . (\rho \mathbf{V}) + \frac{\partial}{\partial z}(\rho w) = 0$$

where $\nabla.$ is the horizontal divergence, $\mathbf{V}$ the horizontal wind velocity vector and w the vertical component of wind velocity. Substituting for $\partial\rho/\partial t$ into the previous equation,

$$\partial p/\partial t = -\int_z^\infty g\nabla.(\rho\mathbf{V})dz + g\rho w \quad . \quad . \quad (2)$$

since $g\rho w$ is zero at the upper limit of the integral (outer limit of atmosphere). This is the fundamental "pressure tendency" equation, expressing the tendency as the sum of the integrated mass-convergence above the reference level and the flow of weight upwards per unit area through the reference level.

James (*Quar. Jour. Roy. Met. Soc.*, **71**, 74, 1945) and later Petterssen (*ibid.*, **71**, 56, 1945) have given an interesting new form of the tendency equation. The first term on the right of (2) may be written $-\nabla.\int_z^\infty g\rho\mathbf{V}\,dz$ which, substituting $-dp$ for $g\rho\,dz$ from (1), transforms to $\nabla.\int_p^0 \mathbf{V}\,dp$ or $-\nabla.(p\overline{\mathbf{V}})$ where $\overline{\mathbf{V}}$ is the isobaric mean wind velocity (mean with respect to pressure) from the reference level to the upper limit of the atmosphere ($p = 0$). So (2) may be written

$$\partial p/\partial t = -\nabla.(p\overline{\mathbf{V}}) + g\rho w \quad . \quad . \quad . \quad (3)$$

At the earth's level surface ($z = 0$) where $p = p_0$, and $w = 0$, (3) becomes

$$\partial p_0/\partial t = -\nabla.(p_0\overline{\mathbf{V}})$$

or

$$\partial p_0/\partial t = -p_0\nabla.\overline{\mathbf{V}} - \overline{\mathbf{V}}.\nabla p_0 \quad . \quad . \quad . \quad (4)$$

Thus the pressure tendency at the surface is compounded of (*a*) the surface pressure times the convergence of the isobaric mean wind of the whole atmosphere above and (*b*) the component of the isobaric mean wind normal to the surface isobar times the gradient of surface pressure.

Take a point in the level $z = 0$ at which the second of these terms is zero, *e.g.* at a centre of pressure ($\nabla p_0 = 0$) or where the isobaric mean wind velocity is along the surface isobars. If, there, $p_0 = 1000$ mb. and $\partial p_0/\partial t = 1$ mb./3 hr., equation (4) gives $\nabla.\overline{\mathbf{V}} = 1$ cm./sec. 100 km. only! Such accuracy in the measurement of the horizontal gradient of velocity is quite unattainable so that we cannot hope to derive the tendency arising from divergence from a direct measurement of the motion. This result is of long standing, having first been demonstrated by Margules at the beginning of the century. It is reintroduced here for another purpose, namely as a warning against making non-uniform approximations to the properties of a field of flow when investigating the changes in pressure associated with it.

H. Jeffreys (*Phil. Mag.*, **37**, 1, 1919) in a famous theorem inferred that no changes in surface pressure were possible in a field of geostrophic motion—unaccelerated horizontal great-circle motion at right-angles to the pressure gradient. R. C. Sutcliffe, in two papers (*Quar. Jour. Roy. Met. Soc.*, **64**, 495, 1938, and **65**, 518, 1939), has made considerable play upon this theorem, inferring that the horizontal mass divergence and the consequent changes of surface pressure arise solely from the field of acceleration, or from the field of departure of the wind from the geostrophic ("geostrophic departure") to which the acceleration may be readily related.

But in deriving his theorem Jeffreys neglected the variation of the Coriolis acceleration in the horizontal, and such neglect requires justification which has not been forthcoming. Indeed, there is little doubt that geostrophic motion directed along the meridian is associated with mass divergence of appreciable amount, appreciable, that is, in its effect on the pressure tendency. This conclusion needs qualification in one important respect; a rectangular co-ordinate system, to which motion over the earth is normally referred and which Jeffreys and others have used in discussing the field of divergence, may lead to false results as soon as one moves away from the origin of co-ordinates unless one is very careful to bear in mind the significance of the terms in the equations away from the origin. Moreover, quantities which may be readily defined and measured at the origin are not so readily interpreted and measured at a distance. It seems desirable therefore to re-examine the motion of the air in terms of spherical polar co-ordinates in which the component velocities and accelerations have a clear significance at any point in the co-ordinate space, direct measurement of many of the quantities being also possible under these conditions.

In spite of the approximation involved in Sutcliffe's basic assumption his papers referred to above are valuable in indicating the likely nature of the motion of the air which will result in the development of pressure inequalities. In his first paper he considers, term by term, the components into which the horizontal acceleration of a particle of air may be resolved, and deduces the corresponding components of the geostrophic departures, namely (i) the isallobaric [1] wind, $(-1/l^2\rho)\nabla(\partial p/\partial t)$ (where l is the Coriolis parameter $2\omega \sin \phi$, with ω the angular velocity of rotation of the earth and ϕ the latitude), blowing down the isallobaric gradient to the point of maximum negative pressure tendency, and first demonstrated by Brunt and Douglas in 1929, (ii) a wind of speed $\frac{1}{2l}\frac{\partial \mathbf{V}^2}{\partial s}$ at right-angles to, and to the left of the streamline s for $\mathbf{V}$ increasing along s, and to the right for $\mathbf{V}$ decreasing along s, *i.e.* in quasi-geostrophic flow, motion down the pressure gradient with converging isobars and up the gradient with diverging isobars, (iii) a cyclostrophic component of wind $\mathbf{V}^2/R_s$, where R_s is the horizontal radius of curvature of the streamline, in the direction of the motion for anticyclonic curvature and opposed to the motion for cyclonic curvature, and (iv) a wind $(w/l)\ \partial\mathbf{V}/\partial z$, arising from the vertical component of motion, and given approximately by $-\frac{gw}{l\mathrm{T}}\nabla \mathrm{T}$, where T is the temperature, *i.e.* a wind blowing from regions of high temperature to low temperature with ascending motion, and conversely.

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[1] An isallobar is a line in a horizontal plane along which the pressure-tendency is constant.

That the isallobaric wind may be of appreciable magnitude is now well established; the movement of fronts under certain conditions can only be explained by invoking its aid. Sutcliffe suggests that the other components may be of equal importance as may, indeed, be readily demonstrated by appeal to particular situations. The difficulty, however, with Sutcliffe's treatment is to relate the rather mathematical nature of the components to physically distinct processes. But Sutcliffe does claim physical reality for the effect of the vertical derivative ((iv) above), and, moreover, he links the geostrophic departure arising from vertical motion in a rather happy but very qualitative way with the occlusion of warm air in a deepening extra-tropical depression.

In Sutcliffe's second paper referred to above, associated fields of a-geostrophic convergence in the lower atmosphere and of over-compensating divergence in the upper troposphere are taken as necessary for the production of low pressure at the surface. Development is then related in a qualitative way with the vector change of surface wind in the direction of the thermal wind above (more exactly in the direction of the vertical shear vector) and a number of typical situations giving rise to development are considered. The reversal of the thermal gradient in the stratosphere compared with that in the troposphere and the corresponding reversal of the thermal wind in the stratosphere suggests that divergence in the upper troposphere is associated with convergence in the stratosphere, accounting perhaps for the relatively high ozone concentrations observed above depressions. Corresponding conclusions are arrived at in regard to anticyclonic development for which it is taken as a necessary condition that a-geostrophic divergence in the lower atmosphere must be associated with over-compensating convergence in the upper troposphere.

Philipps (*Met. Zeit.*, **56**, 460–83, 1939) has considered the constitution of the geostrophic departure in considerable detail, and has obtained more or less useful forms according to the form of the pressure field and the nature of the temporal changes in the field. But he also is unable to associate the geostrophic departure with any clear physical process: while the methods adopted in deriving a useful form for the geostrophic departure are far more complex than Sutcliffe's yet lead to an identical result. Philipps, however, goes on to investigate changes in the pressure field arising from a-geostrophic divergence. Now in such an investigation all that one should rightly assume is a given initial field of pressure and of temperature, corresponding say to mean conditions over a particular part of the globe, and hence determine the changes in space and time resulting from a given likely initial disturbance. Philipps, however, makes much wider assumptions so that it is difficult to say whether he has done much more than show the necessary association between surface and upper-air pressure fields for a given surface pressure field and given distribution of temperature. The association which he does establish is certainly in line with observation. He also deduces the spatial distribution of vertical velocity during the period of growth of associated depressions and anticyclones at the surface and again his results appear very reasonable.

Reverting to equation (4) Petterssen (*loc. cit.*) has shown how the contribution to the pressure tendency arising from the component of mean flow across the surface isobar (the second term on the right of (4)) may be readily evaluated from routine upper-air data. This contribution to the pressure tendency he calls the transport term and while it is usually important he adduces evidence that the contribution of the first term on the right of (4)—the divergence term—is usually of equal importance. Indeed, at the centre of a system of closed isobars the divergence term contributes the whole of the tendency, for at that point ∇p_0 is zero. Probably the chief significance of equation (4) is that by means of it and of routine upper-air data, it will be possible to deduce the distribution of integrated divergence throughout the area of analysis. When such computations have been made for representative sets of weather situations our understanding of the motions associated with the growth of pressure inequalities should be much enhanced, and this appears to the writer to be one of the most promising features of the current study of the genesis of pressure inequalities.

James' and Petterssen's studies have had another useful result; they have, for the first time, given an adequate formal background to the rather loosely used term "thermal steering," by which is understood the observed tendency for pressure patterns at the surface to move in the direction of and at a speed determined by the thermal wind aloft. The basis of this idea is manifest in equation (4), mainly in the transport term, for the isobaric mean wind is largely dependent on the thermal field, particularly, as Petterssen has emphasised, on the thermal field in the lower troposphere. The maximum contribution of the transport term to the pressure tendency at the surface is mainly where there is a maximum cross-isobaric flow of the isobaric mean wind, numerical variations in pressure gradient being of minor account. An effect of the thermal field on the divergence term is also bound to be present though how it will be distributed is not clear without further investigation.

The last contribution to the subject of pressure change with which we shall deal in this article is that of J. Bjerknes and J. Holmboe (*Jour. of Met.*, **1**, 1, 1944), with which the new journal of the American Meteorological Society was auspiciously launched. The theory given in this paper, an elaboration of earlier work by Bjerknes published in *Met. Zeit.*, **54**, 462, 1937, and *Quar. Jour. Roy. Met. Soc.*, **66**, (Supplement), 57, 1940, is an attempt to derive in qualitative form the changes in pressure resulting from an incipient wave produced by the intrusion of a tongue of warm air in a broad westerly current in which the temperature in isobaric surfaces in the troposphere decreases towards the pole. The authors assume that the intrusion of the warm tongue is associated with vertical motion, upward on the east side of the tongue, downward on the west. These initial conditions result in or are associated with the formation of an upper wedge of pressure in the region of rising air and an upper trough in the region of descending air. Further, the wave-shaped isobaric pattern which is formed aloft is shown to distort the initially straight isotherms running west–east into a wave-shaped form out of phase with the pressure wave, the crest of the pressure wedge being east of the crest of the thermal wedge. At any pressure level $\partial p/\partial z \propto 1/T$ and so the consequence of this phase-shift is that the wedge and trough lines are displaced westwards with increasing height, a feature fundamental to further development.

But before considering the development which is likely to result, it is necessary to determine the pattern of mass divergence and convergence associated with a wave-shaped isobaric pattern. The authors investigate this pattern in semi-quantitative fashion. They show that if the motion is quasi-gradient divergence will occur from trough to wedge and convergence from wedge to trough, following the wind, provided that the speed of the wind relative to the pressure pattern exceeds a critical magnitude determined by the wave-length, amplitude and mean latitude of the isobaric wave, to each of which the critical value is roughly proportional. Now in the basic flow pattern considered the wind increases with height up to the tropopause and thereafter decreases with the reversal of the thermal gradient in the stratosphere. There will therefore be a class of wave disturbances for which the relative wind exceeds the critical value over a major part of the troposphere though there may be a sub-critical relative velocity through a layer of the lower atmosphere and again in the stratosphere; indeed, these latter regions must exist unless the wind near the surface "blows through" the surface pressure pattern with considerable speed. In the layers of sub-critical relative speed the above-mentioned distribution of mass divergence and convergence is reversed, levels of non-divergence appearing at the boundaries of these layers with the intermediate layer.

Reverting now to the westward-sloping wedge and trough lines which we found to result from the initial disturbance, it is readily seen that, above the level of non-diverging flow, there is divergence above and to the east of the trough line at any level and convergence above and to the east of the wedge line at the same level. (We may neglect the effect of the stratosphere in the first approximation since relatively little mass is involved in those levels.) Hence, if the lower level of non-divergence is sufficiently low, that is, if the wave-length and other parameters of the initial disturbance are jointly less than the critical, the disturbance will intensify and move eastward. Such instability is obviously increased by a rapid increase of wind with height. This is the most valuable and convincing part of the Bjerknes-Holmbœ theory. It is valid not merely for a broad westerly current with the temperature gradient directed towards north—the conditions under which extra-tropical depression often form and grow—but for any flow with low temperature to the left (right in the southern hemisphere) of the direction of flow, and the authors suggest as a consequence the conditions suitable for the development of tropical storms.

The theory is not, however, able to account for the development of a closed isobaric pattern at the surface. This later stage in the development of a pressure disturbance, in which a wave-shaped isobaric pattern lies above a closed pattern on the surface, with, for example, an upper air trough to the west of a centre of low pressure, is taken from observation. The authors show that in the levels of closed isobars convergence is the most likely condition on the eastern side of a cyclone and divergence on the western side so that the existence of closed isobars tends to act as a brake on the intensification and eastward motion produced by a system of upper trough and wedge with axes inclined to the west. As the closed isobaric pattern extends upwards, that is, as a cyclone from observation becomes old and deep, the integrated divergence (positive or negative) through the layer of closed isobars may be sufficient to overweigh the divergence (negative or positive) in the upper levels of open isobars and the system may become retrogressive.

The authors' treatment is non-frontal except for the initial disturbance, but the modifications imposed by fronts is not expected to upset the major patterns disclosed by the present work. This simplification apart, it is clear that the treatment forms only the first draft of a complete theory of the development and later decay of pressure systems. The inter-relations between the growth of a pressure pattern, the modification of the thermal field and the speed and direction of motion of the pressure pattern are so complex that only a broad qualitative treatment of the earliest stages of the process is so far possible. The production of a system of closed isobars and the corresponding eviction or accumulation of air represented by that pattern remain to be explained, even qualitatively. Further, vertical motion, which is likely to be of fundamental importance at all stages of the process, is only directly invoked for its inception. But the theory is nevertheless considered to be of outstanding interest and importance and is likely to be fruitful for further advance, particularly as the physical processes involved are not hidden beneath a mass of equations.

To summarise the researches reviewed in this article, we note first James' and Petterssen's happy modification of the pressure-tendency equation with the introduction of the valuable concept of mass flow represented by the isobaric mean wind velocity, and an explanation of the concept of thermal-steering. The use of their equation, in conjunction with the observable quantities contained in it, to determine the otherwise unobservable divergence of the mean wind should ultimately add much to our knowledge of the features of the flow responsible for pressure variation. Sutcliffe's stress on the a-geostrophic components of flow and their relation to the shear of the upper wind across the lower in producing fields of divergence and convergence has probably been salutary, even though it is somewhat approximate and has not yet led to any clearly defined line of development. Bjerknes' and Holmboe's important contribution to the problem is in showing how an initial distribution of vertical motion in a given field of flow and of temperature leads to the development of a pressure wave aloft which, for appropriate wave-length or other parameter values, is unstable, extending its influence to the surface, intensifying and moving eastward. In these researches the approach has been wholly dynamical and one wonders whether complete success is attainable on these lines alone. A thermodynamical approach particularly in regard to heat realised by condensation may perhaps need to be interwoven with the dynamical before we gain a much-improved insight into the causes of the net transfer of mass from one part of the atmosphere to another which is what atmospheric pressure variations signify.

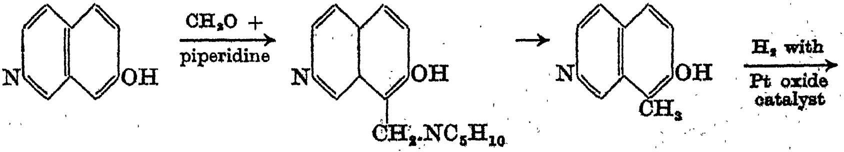
GENERAL AND ORGANIC CHEMISTRY.

By O. L. Brady, D.Sc., F.R.I.C., University College, London.

Synthesis of Quinine.—Ninety years ago a youth working in A. W. Hofmann's laboratory at the Royal College of Chemistry, London, attempted to synthesise quinine by oxidising allyltoluidine in accordance with the equation.

$$2C_{10}H_{13}N + 3O = C_{20}H_{24}N_2O_2 + H_2O.$$

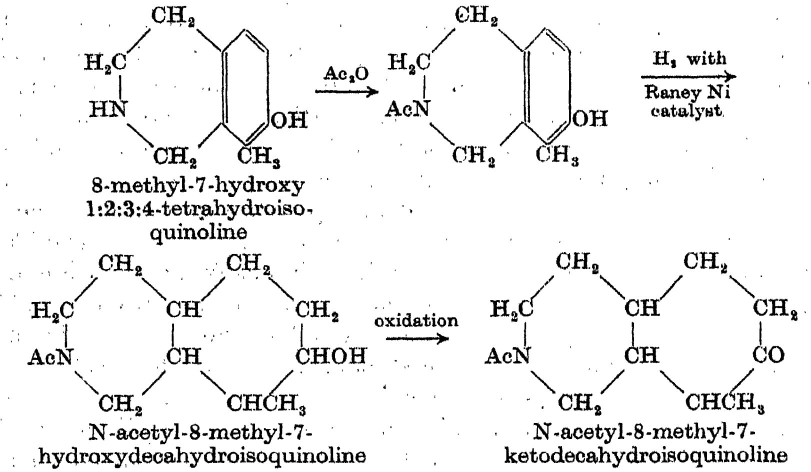
At that time the structural formulæ of few organic compounds were known and, although this first attempt at the synthesis of quinine was inevitably a failure, it led its enthusiastic protagonist, W. H. Perkin, later Sir William Perkin, to the discovery of mauve, the first aniline dye, and to the foundation of the aniline dye industry at Greenford, Middlesex. Another fifty years passed before the structure of quinine was established, mainly by the work of Königs, Rabe and their collaborators but, despite the commercial importance of this alkaloid in connection with the treatment of malaria, its complete synthesis has only recently been achieved by Woodward and Doering (*J. Amer. Chem. Soc.*, 1942, **64**, 2588) by the following series of reactions:



7-hydroxy-isoquinoline

8-piperidinomethyl-7-hydroxyisoquinoline

8-methyl-7-hydroxyisoquindine

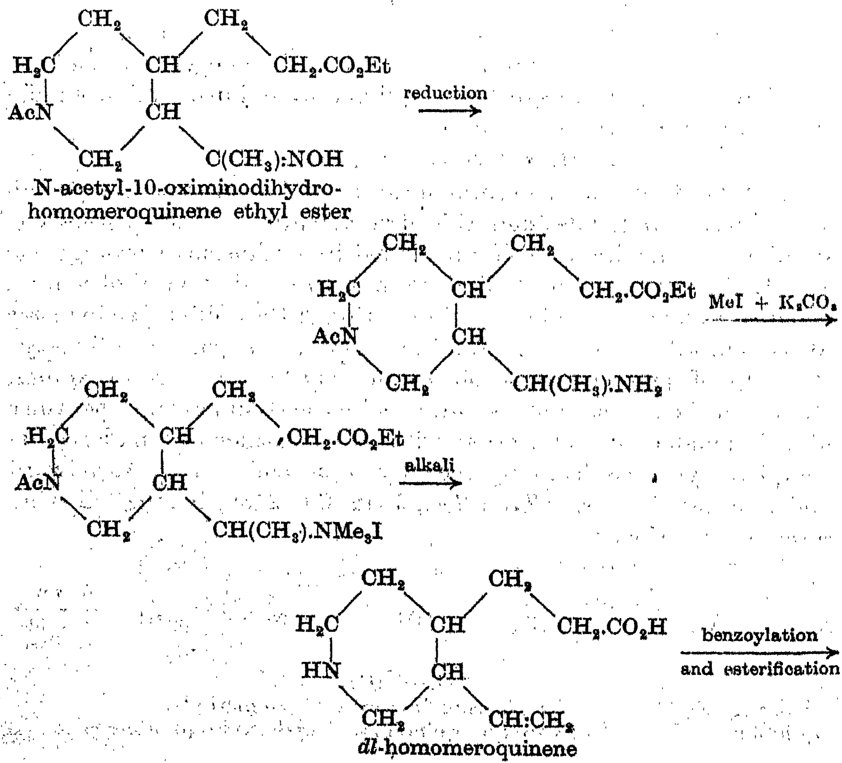


8-methyl-7-hydroxy 1:2:3:4-tetrahydroisoquinoline

N-acetyl-8-methyl-7-hydroxydecahydroisoquinoline

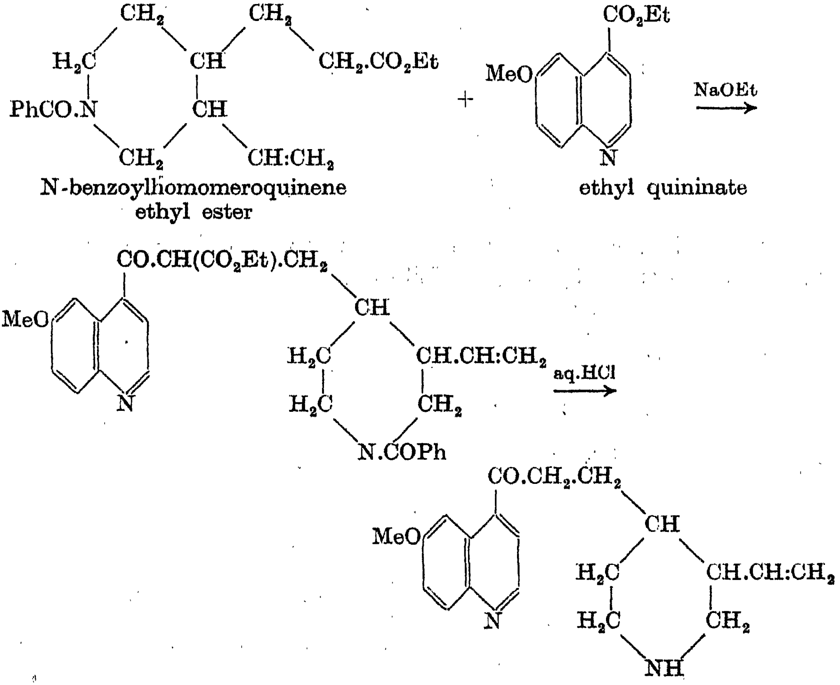
N-acetyl-8-methyl-7-ketodecahydroisoquinoline

The cis form of this ketone was isolated in the form of its monohydrate from the mixture of two stereoisomerides obtained corresponding to cis and trans fusion of the rings, and on treatment with ethyl nitrite and sodium ethoxide gave



N-acetyl-10-oximinodihydrohomomeroquinene ethyl ester

dl-homomeroquinene

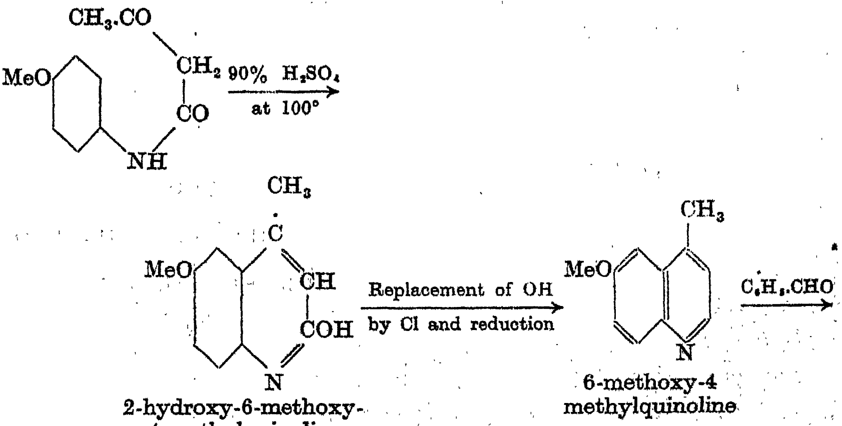

N-benzoylhomomeroquinene ethyl ester

ethyl quininate

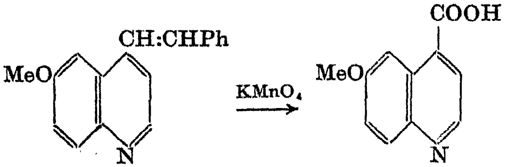
dl-quinotoxine or quinicine

The *dl*-quinotoxine was resolved through its salt with dibenzoyl-*d*-tartaric acid.

Quininic acid can be synthesised from *p*-methoxyaniline by condensing with ethylacetoacetate to give


2-hydroxy-6-methoxy-4-methylquinoline

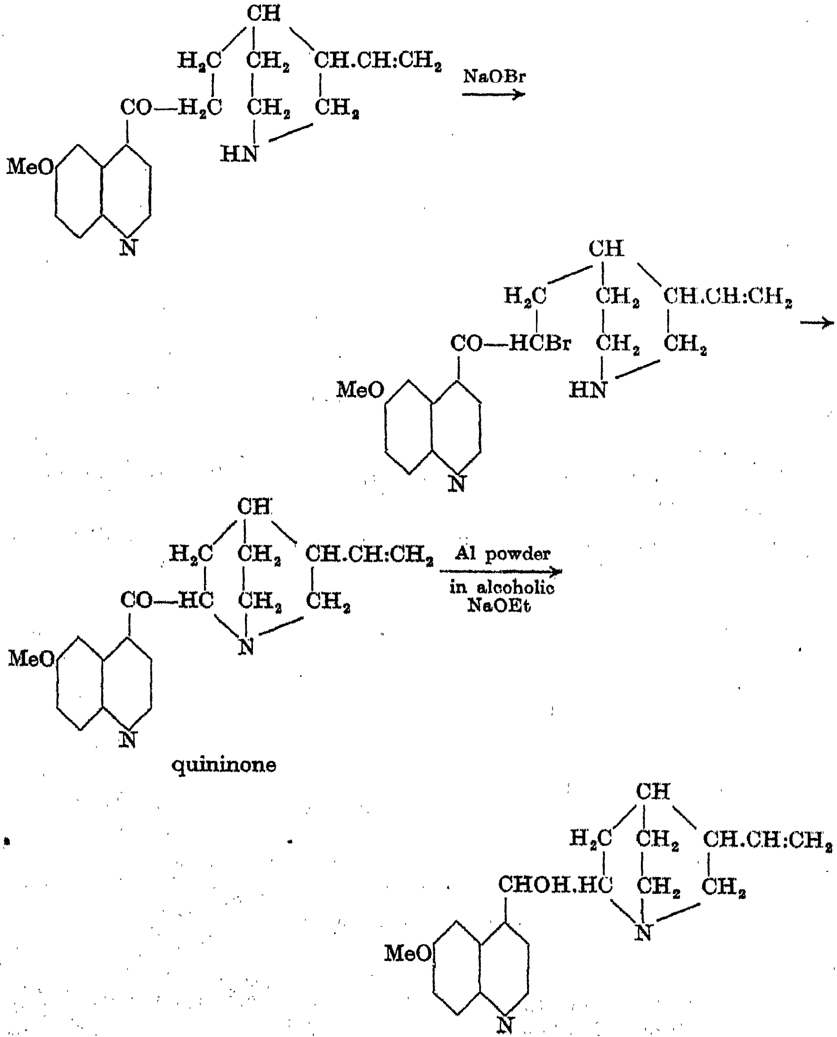
6-methoxy-4 methylquinoline



6-methoxyl-4-styryl-quinoline

quininic acid

Quinotoxine or quinicine has been converted to quinine by Rabe (*Ber.*, 1918, **51**, 466) as follows:



quininone

Quinine

The stages subsequent to the synthesis of *dl*-homomero-quinene were based on the work of Rabe, who had previously synthesised dihydroquinine, a compound in which the vinyl group in quinine is replaced by the ethyl group (*Ber.*, 1918, **51**, 1360; 1919, **52**, 1842). The present workers have at last overcome the final difficulty in introducing the vinyl group.

THE MECHANISM OF SUBSTITUTION AT A SATURATED CARBON ATOM.—There still remains much to be discovered about the simplest organic reactions, even the substitution which is represented to the beginner in the form $CH_3I + KOH = CH_3OH + KI$. Reactions of this type have been classified and discussed by Hughes (*Trans. Faraday Soc.*, 1941, **37**, 603) and may be generalised in the form $Y + R{-}X \rightarrow Y{-}R + X$, the example above being more correctly written

$$\overset{\ominus}{\mathrm{O}}\mathrm{H} + \mathrm{CH_3I} \rightarrow \mathrm{CH_3OH} + \overset{\ominus}{\mathrm{I}}$$

The first consideration in the study of the mechanism of the reaction is the nature of the bond fission of R—X, it may be homolytic, where one of the pair of electrons constituting the bond goes with each fragment $\mathrm{R}{}^{\times}_{\times}\mathrm{X} \rightarrow \mathrm{R}{\times} + {\times}\mathrm{X}$, or heterolytic, where the pair of electrons remains associated with one of the fragments $\mathrm{R}{}^{\times}_{\times}\mathrm{X} \rightarrow \mathrm{R} + {}^{\times}_{\times}\mathrm{X}$. Simple substitution involving homolytic fission is usual for gas phase reactions and involves either an atomic substituting agent or a molecular one containing an odd electron such as nitrogen dioxide.

$$\mathrm{Y}{\times} + \mathrm{R}^{\times}|{}_{\times}\mathrm{X} \rightarrow \mathrm{Y}{}^{\times}_{\times}\mathrm{R} + {\times}\mathrm{X},\ \textit{e.g.}\ \mathrm{H} + \mathrm{H{-}H} \rightarrow \mathrm{H{-}H} + \mathrm{H}$$

In the past, substitution by this type of reaction has not often been met with but is now assuming great technical importance as the vapour phase nitration or halogenation of paraffins proceeds by this mechanism.

The usual mechanism in solution is that involving heterolytic fission and attack by ionic or ionogenic substituting agents, and here there are two types of substitution, nucleophilic

$$\mathrm{Y}{}^{\times}_{\times} + \mathrm{R}|{}^{\times}_{\times}\mathrm{X} \rightarrow \mathrm{Y}{}^{\times}_{\times}\mathrm{R} + {}^{\times}_{\times}\mathrm{X},\ \textit{e.g.}\ \overset{\ominus}{\mathrm{I}} + \mathrm{R{-}Cl} \rightarrow \mathrm{IR} + \overset{\ominus}{\mathrm{Cl}}$$

or electrophilic

$$\mathrm{Y} + \mathrm{R}{}^{\times}_{\times}|\mathrm{X} \rightarrow \mathrm{Y}{}^{\times}_{\times}\mathrm{R} + \mathrm{X},\ \textit{e.g.}\ \overset{\oplus}{\mathrm{D}} + \mathrm{R{-}H} \rightarrow \mathrm{DR} + \overset{\oplus}{\mathrm{H}}$$

of which the nucleophilic are the most common.

Nucleophilic substitutions are further subject to variation according as Y initially is neutral or negative and X neutral or positive.

(*a*) Y negative X neutral $\overset{\ominus}{HO} + RCl \rightarrow HOR + \overset{\ominus}{Cl}$

(*b*) Y neutral X neutral $R_3N + RCl \rightarrow R_3\overset{\oplus}{N}R + \overset{\ominus}{Cl}$

$H_3N + RCl \rightarrow H_3\overset{\oplus}{N}R + \overset{\ominus}{Cl}$

$\hookrightarrow H_2NR + \overset{\oplus}{H}$

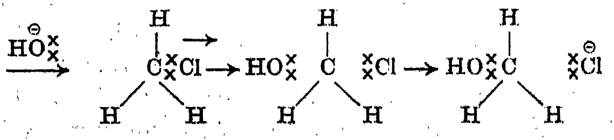
$H_2O + RCl \rightarrow H_2\overset{\oplus}{O}R + \overset{\ominus}{Cl}$

$\hookrightarrow HOR + \overset{\oplus}{H}$

(*c*) Y negative X positive $\overset{\ominus}{HO} + R—\overset{\oplus}{N}R_3 \rightarrow HOR + NR_3$

(*d*) Y neutral X positive $R_3N + R\overset{\oplus}{S}R_2 \rightarrow R_3\overset{\oplus}{N}R + SR_2$

One mechanism which has been applied to these heterolytic substitutions in solution is a synchronous bond-forming and bond-breaking reaction; for example, as the chlorine moves away from the carbon with its pair of electrons so the substituting hydroxyl ion moves in, and only when the carbon has completely lost interest in the pair of electrons of the departing chlorine does it acquire a full share of the pair of electrons of the entering hydroxyl group.

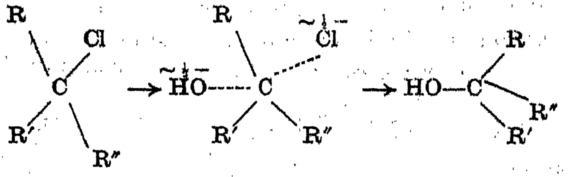


The middle transitional state is represented

$$\overset{\sim\frac{1}{2}-}{HO} \ldots R \ldots \overset{\sim\frac{1}{2}-}{Cl}$$

This is termed the " bimolecular " nucleophilic substitution reaction and designated S_N2.

Such a representation of substitution carries with it the implication that the entering substituent approaches the molecule from the opposite side to the outgoing one and provides an appropriate picture of the mechanism of the Walden Inversion.



The experimental study of the kinetics of nucleophilic substitutions has shown, however, that the S_N2 mechanism is not of itself a sufficient explanation of the behaviour of, for example, alkyl halides on hydrolysis. In the hydrolysis of methyl halides the kinetics are of the second order, corresponding to the " bimolecular " mechanism S_N2 but with tertiary butyl halides the kinetics are of the first order and independent of the concentration of the hydroxyl ion. With ethyl and isopropyl halides the kinetic study indicates a combination of first and second order reactions. In order to explain this, as well as observed solvent effects, it is suggested that there is a second mechanism proceeding in two stages, a slow ionisation in which the substituting agent does not take part, succeeded by a rapid reaction between the first formed carbonium ion and the substituting agent.

$$\mathrm{R}{}^{\times}_{\times}\mathrm{X} \xrightarrow{\text{slow}} \overset{\oplus}{\mathrm{R}} + {}^{\times}_{\times}\overset{\ominus}{\mathrm{X}} \qquad \overset{\ominus}{\mathrm{Y}}{}^{\times}_{\times} + \overset{\oplus}{\mathrm{R}} \xrightarrow{\text{rapid}} \mathrm{RY}$$

$$\text{or} \qquad \mathrm{RCl} \xrightarrow{\text{slow}} \overset{\oplus}{\mathrm{R}} + \overset{\ominus}{\mathrm{Cl}} \qquad \overset{\ominus}{\mathrm{O}}\mathrm{H} + \overset{\oplus}{\mathrm{R}} \xrightarrow{\text{rapid}} \mathrm{ROH}$$

The rate controlling ionisation is slow because it has to pass over an energy maximum which occurs at a certain critical extension of the bond and a certain critical degree of charge transfer. This mechanism is termed " unimolecular " because in the ionisation, the rate-determining change, only one molecule is undergoing covalency change and is designated $\mathrm{S_N1}$, and its kinetics will accordingly be of the first order.

THE MECHANISM OF ELIMINATION REACTIONS.—In the same journal (p. 657) Hughes and Ingold discuss the mechanism of elimination reactions and show that here too there is a similar duality. In the " bimolecular " mechanism ($\mathrm{E_2}$) a base, *i.e.* a proton acceptor, extracts a proton whilst an electron attracting group simultaneously separates.

$$\begin{aligned}\mathrm{Y}{}^{\times}_{\times} + \mathrm{H}{}^{\times}_{\times}\mathrm{CR_2}{}^{\times}_{\times}\mathrm{CR_2}{}^{\times}_{\times}\mathrm{X} \longrightarrow \mathrm{Y}{}^{\times}_{\times}\mathrm{H} + {}^{\times}_{\times}\mathrm{CR_2}{}^{\times}_{\times}\mathrm{CR_2}{}^{\times}_{\times}\mathrm{X} \longrightarrow {}^{\times}_{\times}\mathrm{CR_2}{}^{\times}_{\times}&\mathrm{CR_2} + {}^{\times}_{\times}\mathrm{X}\\ &\hookrightarrow \mathrm{CR_2}{}^{\times\times}_{\times\times}\mathrm{CR_2}\end{aligned}$$

X may be, for example, $\overset{\oplus}{\mathrm{N}}\mathrm{R_3}$, $\overset{\oplus}{\mathrm{S}}\mathrm{R_2}$, $\mathrm{SO_2R}$, Halogen, and Y may be, for example, $\mathrm{OH_2}$, $\mathrm{NMe_3}$, $\overset{\ominus}{\mathrm{O}}\mathrm{Ac}$, $\overset{\ominus}{\mathrm{O}}\mathrm{H}$, $\overset{\ominus}{\mathrm{O}}\mathrm{Et}$, and the following reactions are typical

$$\mathrm{NMe_3} + \mathrm{CH_2Ar.CH_2.}\overset{\oplus}{\mathrm{N}}\mathrm{Me_3} \longrightarrow \mathrm{H}\overset{\oplus}{\mathrm{N}}\mathrm{Me_3} + \mathrm{CHAr{:}CH_2} + \mathrm{NMe_3}$$

$$\overset{\ominus}{\mathrm{O}}\mathrm{H} + \mathrm{CH_3.CH_2.}\overset{\oplus}{\mathrm{S}}\mathrm{Me_2} \longrightarrow \mathrm{OH_2} + \mathrm{CH_2{:}CH_2} + \mathrm{SMe_2}$$

$$\overset{\ominus}{\mathrm{O}}\mathrm{Et} + \mathrm{CH_2Ar.CH_2Br} \longrightarrow \mathrm{HOEt} + \mathrm{CHAr{:}CH_2} + \overset{\ominus}{\mathrm{Br}}$$

In certain circumstances, however, a " unimolecular " ($\mathrm{E_1}$) mechanism is the controlling factor in which there is a slow ionisation followed by a rapid loss of proton from the carbonium ion.

$$\mathrm{H}{}^{\times}_{\times}\mathrm{CR_2}{}^{\times}_{\times}\mathrm{CR_2}{}^{\times}_{\times}\mathrm{X} \xrightarrow{\text{slow}} \mathrm{H}{}^{\times}_{\times}\mathrm{CR_2}{}^{\times}_{\times}\overset{\oplus}{\mathrm{C}}\mathrm{R_2} + {}^{\times}_{\times}\overset{\ominus}{\mathrm{X}}$$

$$\mathrm{H}{}^{\times}_{\times}\mathrm{CR_2}{}^{\times}_{\times}\overset{\oplus}{\mathrm{C}}\mathrm{R_2} \xrightarrow{\text{fast}} \mathrm{CR_2}{}^{\times\times}_{\times\times}\mathrm{CR_2} + \overset{\oplus}{\mathrm{H}}$$

The type of mechanism depends on the structure of the compound; the more R favours electron release from the carbon atom bearing X the more the "unimolecular" mechanism predominates, for example, the reactions

$$CH_3.CMe_2Br \longrightarrow \overset{\oplus}{H} + CH_2{:}CMe_2 + \overset{\ominus}{Br}$$
$$CH_3.CMe_2\overset{\oplus}{S}Me_2 \longrightarrow \overset{\oplus}{H} + CH_2{:}CMe_2 + SMe_2$$

proceed mainly by this mechanism.

BIOCHEMISTRY.

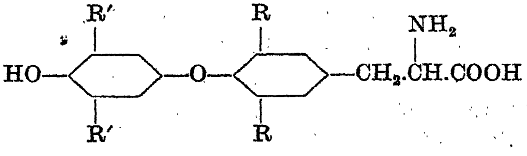
By W. O. Kermack, D.Sc., LL.D., F.R.S., Royal College of Physicians' Laboratory, Edinburgh.

During the past seven years, in spite of the war, research in biochemistry has continued on a surprisingly large scale and several advances of great importance fall to be recorded. Even in Great Britain, with all the strains of war, the material damage due to bombing, and the pressure on trained personnel, a considerable volume of research on the basic problems of biochemistry has continued to be published, and many significant advances have been made. The *Biochemical Journal*, though naturally reduced in size, has continued to appear, and it is doubtful whether a visitor from another planet inspecting its pages would deduce that the researches reported and the actual printing and publication had proceeded in a beleaguered island fortress. The *Annual Reviews of Biochemistry* have continued to be published yearly in America, and these substantial volumes make it plain how biochemical knowledge has continued to expand even in the unfavourable circumstances of a world almost completely involved in total war.

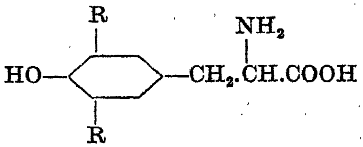
Among the highlights in the progress of biochemistry during these past seven years we may note the discovery of the structure of biotin, and the synthesis of this vitamin; the use of labelled nitrogen, carbon, oxygen, phosphorus and other elements to follow the course of metabolism; and the discovery of the extraordinary lability of the constituents of body tissues as demonstrated by Schœnheimer and his colleagues; the elucidation of the mechanism of thyroxin synthesis; the Fildes-Woods theory of chemotherapeutic action; the isolation of the penicillins and the elucidation of their structures; the further investigations of the phosphate cycle in muscle and bacteria, and the identification of the muscle protein, myosin, as probably the enzyme which catalyses the hydrolysis of adenosine-triphosphate and so liberates the chemical energy for muscle contraction.

These are all large subjects and space allows us to treat only one of them in this issue. The subject of the synthesis of thyroxin has been chosen as it raises a number of points of general interest and the results may prove of practical clinical importance.

THYROXIN—*General and Historical.*—Thyroxin was first isolated from animal thyroid by Kendall (*J. Biol. Chem.*, 1915, **19**, 25); and its constitution was ascertained and its synthesis achieved by the work of Harington and Barger (*Biochem. J.*, 1927, **21**, 169). It proved to be an amino acid of structure I (R=R′=I),



I



II

that is to say, it is a tetraiodothyronine (I, R=R′=H). Thyroxin is obviously a derivative of tyrosine (II, R=H) in the formal chemical sense, and it is natural to assume that in its synthesis in the animal body the starting material is tyrosine. Actually the only other iodine-containing organic compound so far found in nature is 3:5-di-iodotyrosine (II, R=I) which has been isolated from certain corals and sponges and is associated with thyroxin in the thyroid gland. The suggestion was advanced by Harington and Barger that thyroxin is produced *in vivo* from di-iodotyrosine, in a reaction between two molecules of the latter in which one of the amino acid side chains is eliminated and the diphenylether linkage is formed. In recent years a number of papers have appeared which have fully confirmed this hypothesis, and have shed much light on the details of the mechanism.

It was Baumann (*Z. Physiol. Chem.*, 1896, **22**, 1) who first demonstrated, just about fifty years ago, that thyroid tissue was peculiar in being rich in iodine, and investigation showed that this element was contained in a globulin-like protein now usually called thyroglobulin, which may, in fact, contain anything from 0 to 1 per cent. of iodine, low values being associated with goitrous conditions arising from iodine deficiency. It was natural, then, that many attempts should be made to introduce iodine into common proteins to produce, if possible, iodoproteins exhibiting thyroid activity. The iodination of proteins such as casein is easy enough, but all attempts to demonstrate true thyroid activity were unsuccessful. At a relatively early date, Oswald (*Z. Physiol. Chem.*, 1910, **70**, 310) succeeded in isolating 3:5-di-iodotyrosine from iodinated proteins (albumin, gliadin and casein). An interesting short historical account of the earlier unsuccessful attempts to produce biologically active iodoproteins is given by Reineke and Turner (*J. Clin. Endocrinol.*, 1943, **3**, 1).

In spite of repeated failures to produce an artificially iodinated protein with true thyroid activity, the quest was not abandoned, and positive results were claimed by Abelin in various papers between 1933 and 1939 (*Arch. Exp. Path. Pharmacol.*, 1933, **171**, 433; 1934, **173**, 146; 1934, **175**, 151; 1936, **181**, 250; 1938, **189**, 473). Although this result was in harmony with similar findings in America (Salter and Lerman, *Endocrinology*, 1936, **20**, 801), it was regarded with some degree of scepticism, more particularly as Abelin was unable to isolate thyroxin from the hydrolysis products of his iodinated protein. It seems probable, however, that Abelin's results gave the impetus to the experiments of Ludwig and von Mutzenbecher (1939) which were completely successful in their immediate purpose, and have led to a variety of practical and theoretical results.

The Experiments of Ludwig and von Mutzenbecher.—Ludwig and von Mutzenbecher (*Z. Physiol. Chem.*, 1939, **258**, 159) treated casein (100 g.), dissolved in sodium bicarbonate solution, with iodine (10 g.) the temperature being 37°. It will be noticed that the temperature and mildly alkaline reaction are approximately physiological. The iodinated casein, which contained 6–8 per cent. iodine and exhibited definite thyroid activity, was submitted to alkaline hydrolysis with baryta, and thyroxin was separated from the hydrolysate by methods already well explored in connection with the isolation of thyroxin from thyroglobulin. It was purified through the insoluble potassium salt and its identity established beyond doubt. Like the product isolated from thyroglobulin by alkaline hydrolysis, it was racemic, and the yield from 100 g. of casein was 100 mg.

Two explanations of these results have to be considered. The first hypothesis is that casein and other proteins contain a hitherto unrecognised amino acid, thyronine (I, R=R′=H), which undergoes tetraiodination. The second is that the thyroxin is formed from the di-iodotyrosine, known to be present in the iodinated protein, by some type of oxidative synthesis. In a letter in *Nature* which appeared shortly after the publication of the paper by Ludwig and von Mutzenbecher, Harington and Pitt Rivers (*Nature*, 1939, **144**, 205) stated that they had completely confirmed the claims of the German workers, and went on to discuss the two hypotheses. Against the first is the well-known difficulty of introducing four iodine atoms directly into thyronine; only the two hydrogen atoms adjacent to the hydroxyl are displaced and entry into the positions adjacent to the ethereal oxygen is unknown. The second hypothesis appears the more plausible. The oxidative synthesis of thyroxin from two molecules of di-iodotyrosine was in fact the biochemical process originally envisaged by Harington and Barger, and it was on the basis of this provisional theory that they first assigned the positions of the iodine atoms in thyroxin, positions later confirmed by synthesis. This biochemical genesis of thyroxin had later been supported by two further facts. An extensive search by Harington and Randall (*Biochem. J.*, 1929, **23**, 373) led them to conclude that the iodine present in the thyroid, not present as thyroxin, was present as di-iodotyrosine; no other iodine-containing organic compound, such as, for example, di-iodothyronine (I, R$=$H, R$'=$I) could be detected. The second piece of confirmatory evidence was the demonstration that optically active thyroxin isolated from thyroglobulin, after enzymatic hydrolysis, belonged to the *l*-series (Harington and Salter, *Biochem. J.*, 1930, **24**, 456) as do natural tyrosine and di-iodotyrosine. This observation, of course, in no way proves the hypothesis, but had natural thyroxin belonged to the *d*-series, the hypothesis would have had to be discarded.

Thus there was in 1939 a good deal of support for the view that in the thyroid gland two molecules of tyrosine were condensed together with the formation of one molecule of thyroxin, under conditions which involved the elimination of a side-chain. The postulated reaction appeared of a somewhat strange type, but one could assume that it was promoted and guided by suitable enzymes present in the thyroid. It now seems from the new results on the iodination of casein and other proteins that this particular reaction can proceed without any help from enzymes, under extremely mild conditions (*p*H about 8; temp. 37°) and further that it can occur with di-iodotyrosine still incorporated in protein molecules.

Much light was shed on the whole question by a second paper by von Mutzenbecher which appeared later in 1939 (*Z. Physiol. Chem.*, 1939, **261**, 253). Here he showed that thyroxin is formed from di-iodotyrosine simply by dissolving the latter in an equivalent of 0·1N sodium hydroxide solution, and keeping the solution (*p*H 8·8) at 37° for 14 days. He succeeded in isolating pure thyroxin in a yield of 0·24 per cent. calculated on the di-iodotyrosine employed. That the process involved is an oxidative one is indicated by the fact that thyroxin formation does not occur in presence of sulphite.

Recent Work on Thyroxin Synthesis.—It may be stated at once that this direct synthesis of thyroxin from di-iodotyrosine has been amply confirmed by several workers in America (*cf.* Johnson and Tewkesbury, *Proc. Nat. Acad. Sci.*, 1942, **28**, 73; Barkdoll and Ross, *J. Amer. Chem. Soc.*, 1944, **66**, 898), and by Harington and Pitt Rivers (*Biochem. J.*, 1945, **39**, 157) in this country. The latter have made a careful study of the effect of variation of the conditions on the yield of thyroxin and the amount of destruction of the di-iodotyrosine.

In the series of experiments in which di-iodotyrosine dissolved in sodium hydroxide solution was allowed to stand for 14 days at 38° and in which both the thyroxin formed and the di-iodotyrosine recoverable were determined, Harington and Pitt Rivers showed that the maximum yield, calculated on the di-iodotyrosine which disappeared, was obtained at *p*H 10. As the reaction appeared to involve oxidation, ferricyanide was tried but was unsuccessful; its presence resulted in no thyroxin being formed. Hydrogen peroxide at 38° did not much alter the yield, but reduced the time required from days to hours. The best net yield (4 per cent. of the tyrosine which could not be recovered) was obtained by carrying out the reaction on the boiling water-bath, using a very large excess (25 equivalents) of hydrogen peroxide, and having amyl alcohol constantly present. The object of this is to dissolve out the sodium salt of thyroxin immediately it is formed so as to protect it from destruction.

The chemical processes involved in the production of thyroxin from di-iodotyrosine are discussed by Johnson and Tewkesbury (*loc. cit.*). Harington (Pedler Lecture, *J.C.S.*, 1944, 193) agrees in general with their suggested mechanism, but considers that the actual oxidising agent is probably free iodine rather than hypoiodite. Under conditions more alkaline than *p*H 10, iodine tends to change into hypoiodite, and so Harington's view explains why the yield decreases as the *p*H is increased above 10.

Synthesis of Thyroxin "in vivo."—We have now to consider the bearing of these results on the problem of the mechanism of the production of thyroxin *in vivo*. First of all, a brief reference may be made to another method which has been applied in recent years to this question and has yielded important results. This consists in the employment of radioactive iodine as a tracer substance. The outstanding finding of this work is the great power of thyroid tissue to concentrate iodide within itself (*cf.* Hertz, Roberts, Evans, *Proc. Soc. Exp. Biol. Med.*, 1938, **38**, 510). This holds both for the thyroid tissue in the intact animal and for surviving tissue slices in bicarbonate-Ringer. The experiments prove that within a few hours the inorganic iodide thus concentrated in the thyroid tissue is to a large extent found in the form of di-iodotyrosine, and to a considerably smaller extent as thyroxin, the amount of the latter fraction of tracer iodine tending to increase for several hours. The results with tissue slices are particularly instructive (Morton and Chaikoff, *J. Biol. Chem.*, 1943, **147**, 1). When the tissue is minced, the rate at which the iodine is converted into the organic form is reduced; when the tissue is homogenised all the radio-iodine remains inorganic. Substances such as azides, hydrogen sulphide, hydrogen cyanide, and carbon monoxide which inactivate the cytochrome-oxidase system, inhibit the formation of di-iodotyrosine and thyroxin, but not the uptake of iodide by the tissue slices (Schachner, Franklin and Chaikoff, *J. Biol. Chem.*, 1943, **151**, 191). It is suggested that under the action of this oxidase system free iodine is liberated, and this would, in the mildly alkaline conditions, first iodinate any tyrosine present, and then convert the latter into thyroxin. In accordance with this idea, reduction of the oxygen tension reduces the rate of incorporation of the radio-iodine into the organic molecules, but rather unexpectedly, even at quite low tensions of oxygen, the rate was still considerable. A somewhat surprising finding in the surviving tissue experiments is that iodide itself, in quite small concentrations, inhibits the reaction. Other inhibitors of the formation of di-iodotyrosine and thyroxin are compounds such as thiourea and thiouracil, which have recently been found to exert powerful physiological and therapeutic actions. Their effects on the intact animal appear to be readily interpretable in terms of the power of these compounds to inhibit the formation of thyroxin.

As we have seen, there is good evidence for regarding free iodine as the agent which, in faintly alkaline solution, is capable *in vitro* of producing thyroxin from di-iodotyrosine. Under the same conditions, of course, free iodine rapidly converts tyrosine into di-iodotyrosine. The simplest assumption, then, with regard to the rôle of the thyroid in thyroxin formation is that it is a tissue which (*a*) has the power to trap iodide in virtue of an oxidase system which converts it into free iodine, and (*b*) contains a suitable supply of tyrosine, presumably incorporated in protein. The work we have been describing shows that under these conditions iodination of the tyrosine to di-iodotyrosine would proceed, and that the latter would to some extent at least undergo oxidative coupling to thyroxin.

All this we know could occur without assistance from enzymes. This, of course, does not exclude the possibility that some enzyme may be present which facilitates the conversion of di-iodotyrosine into thyroxin in the thyroid; the point is that, in theory at least, the enzyme could be dispensed with. Perhaps an important factor may be the protein in which the tyrosine is contained. It has been observed that the yield of thyroxin from iodinated proteins does not depend merely on the tyrosine content but also on the nature of the protein; silk fibroin, for example, though rich in tyrosine yields no thyroxin on iodination. As two molecules of di-iodotyrosine are necessarily required for one molecule of thyroxin, it may readily be believed that much will depend on the relative positions of the tyrosine units in the whole protein. The assumed mechanism suggests that the hydroxy group of one must be able to approach the point of attachment of the side-chain of another. It is possible that in the course of evolution the thyroid has perfected the manufacture of a protein satisfying this condition. Iodination would then proceed in the way suggested and even in the absence of any specific enzyme the di-iodotyrosine in this "thyroglobulin" would readily yield thyroxin by a mechanism essentially the same as that which operates *in vitro*. It would be interesting to know whether specially good yields of thyroxin could be obtained by iodination of thyroglobulin from goitrous thyroids of low iodine content.

The general idea that the biogenesis of thyroxin is not necessarily dependent on the action of special enzymes is supported by the proof that thyroxin may be formed in the body outside the thyroid (Morton, Chaikoff, Reinhardt, and Anderson, *J. Biol. Chem.*, 1943, **147**, 757). Radio-iodine was administered to rats from which the thyroids had been completely extirpated. After several hours the rats were killed and the livers and intestines analysed. To the fractions which ought to contain the di-iodotyrosine and thryoxin, authentic non-radioactive di-iodotyrosine and thryoxin were added. On repeated recrystallisation, these compounds showed a persistent and constant radioactivity, which all efforts failed to remove. This showed that radioactive isotopic compounds were present, and these must have been formed in the animals' tissues from the administered radio-iodine. It seems agreed that the possibility of the direct exchange of the radio-iodine for ordinary iodine in thyroxin or di-iodotyrosine under the conditions of the experiment can be excluded; thyroxin must therefore have been synthesised in tissues other than the thyroid, and though the capacity of these tissues in this respect is no doubt small, the result is of much interest and considerable significance from the point of view of the evolution of the thyroid and of thyroxin as an animal hormone.

Evolution of Thyroxin as a Hormone.—Means (*Ann. Int. Med.*, 1943, **19**, 567) discusses this problem as well as other questions connected with thyroid function. Iodine is present in sea water, and in primitive sea organisms, in presence of oxygen and oxidase systems iodine would be liberated locally. Our present knowledge suggests that di-iodotyrosine and in smaller amounts thyroxin would then inevitably be formed from the tyrosine present in the organic materials. One must assume that it happened that some obscure property of thyroxin resulted in its being able to stimulate the growth of the primitive organism, and so gave the thyroxin-containing organism an advantage over its neighbours. No doubt the process of natural selection would then induce a modification of the tissues of the organism rendering them more and more sensitive to the action of the compound, and at the same time would tend to provide for the more efficient production of the compound itself, by developing in one particular tissue the power to concentrate inorganic iodine. Thyroxin production might also be locally facilitated by the development of a special protein with the tyrosine units in good positions for the final oxidative coupling. In this way, the thyroid, with its typical iodoprotein, thyroglobulin, may have evolved from thyroxin, formed by chance in primitive aquatic organisms within which the conditions were analogous to those obtaining in the laboratory experiments of Ludwig and von Mutzenbecher.

Possible Practical Applications.—A few words may be added with reference to recent developments which seem to have promise of important practical results and therapeutic application. Von Mutzenbecher (*loc. cit.*) reported that his iodocasein increased in thyroxin content if further incubated in bicarbonate solution at 38°, and that other iodocasein prepared by iodination in ammonia solution at a low temperature, from which no thyroxin could be isolated, developed activity and yielded thyroxin if similarly incubated. Reineke and his colleagues (Reineke and Turner, *J. Biol. Chem.*, 1943, **149**, 555, 563; Reineke, Williamson and Turner, *ibid.*, 1943, **147**, 115) have developed this line a stage further and have succeeded in preparing iodoproteins with several times the activity of dry thyroid powder. The highly active iodocaseins were obtained by treating casein, dissolved in sodium bicarbonate solution, at 38–40° with sufficient iodine (about 17·6 g. per 100 g. casein) to ensure that the tyrosine in the casein takes up just two atoms of iodine per molecule. The addition of iodine, which took about

4 hours, was followed by incubation at 70° for 20 hours. The product, which contained about 7·5 per cent. of iodine, had about three times the activity of dried thyroid gland when tested on guinea-pigs, and 8·5 times when tested on tadpoles.

In a recent paper, Reineke and Turner (*J. Biol. Chem.*, 1945, **161,** 613) show that two other factors have a favourable influence on the yield, *viz.* the presence of certain salts or oxides of manganese, and vigorous agitation of the casein solution during incubation at 70°. This agitation presumably promotes the access of atmospheric oxygen and both it and the manganese apparently facilitate the oxidative coupling of the di-iodotyrosine to thyroxin.

Reineke, Turner, Köhler and Beezly (*J. Biol. Chem.*, 1945, **161,** 599) have determined the thyroxin in their iodinated casein both chemically and biologically by its action on guinea-pigs. Fairly concordant results are obtained, and they conclude that some of their preparations contain over 3 g. of thyroxin per 100 g. Of course, only a relatively small proportion of this, perhaps 400 mg., can actually be isolated as pure crystalline thyroxin. It would seem that these iodinated casein preparations are about four times as active and contain about four times as much thyroxin as the ordinary commercial dried thyroids.

The thyroxin appears definitely to be present in peptide combination and to exist as the natural *l*-thyroxin, though of course it is the less active *dl*-thyroxin that is isolated from it after alkaline hydrolysis. About three-quarters of the iodine is present as non-thyroxin iodine. Presumably it is present as di-iodotyrosine. This large amount of di-iodotyrosine may be connected with the surprisingly high biological activity of the iodocasein when tested on tadpoles; the modern view seems to be that iodotyrosine is inactive in the test, but it is just possible that iodotyrosine in suitable protein combination might form some thyroxin in the tadpole's body. The pharmacological properties of these highly potent iodinated proteins are said to be qualitatively the same as those of thyroid powder, and it seems quite possible that such products, which should be cheap and readily prepared commercially in large batches of a uniform quality, may come into therapeutic use on a considerable scale.

(The following review articles give further information on various aspects of the subject: Harington, Pedler Lecture, *J.C.S.*, 1944, 193; Harington, Croonian Lecture, *Proc. Roy. Soc.*, 1944, **B 132,** 223; Means, *Ann. Int. Med.*, 1943, **19,** 567; Young, *Ann. Rep. Chem. Soc.*, 1944, **41,** 240; Salter, *Physiol. Rev.*, 1940, **20,** 345.)

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., D.Sc., F.R.S.E., The University, Glasgow.

Petrography of Igneous Rocks.—In resuming these Advances after an interval of six years it is difficult to know where to begin. The World War has slowed down scientific publication everywhere, but unequally. Nevertheless, a large number of geological publications has accumulated, especially from the United States and the British Dominions, in which the paper situation has been easier than in this country. What is astonishing, however, is the flood of new geological works, excellently printed and illustrated on good paper, which have arrived from some of the occupied countries of Europe, many of which were published during the Nazi occupation.

With the plethora of new material one can only select items here and there to illustrate the general trend of geological and petrological thought during the last six years. It is hopeless to try to cover the immense field at all adequately.

I cannot do more than mention two magnificent petrological memoirs, both published in 1939, and both worthy of extended review : namely, L. R. Wager and W. A. Deer, " Geological Investigations in East Greenland, Part III. The Petrology of the Skaergaard Intrusion, Kangerdlugssuaq, East Greenland " (*Medd. om Grönland*, **105**, No. 4, 1939, 352 pp., 68 figs., 27 pls.) ; A. F. Buddington, " Adirondack Igneous Rocks and their Metamorphism " (*Geol. Soc. Amer., Mem.* 7, 1939, 354 pp., 30 figs., 21 pls.).

S. J. Shand has brought together the scattered data regarding the " Loch Borolan Laccolith, North-west Scotland " (*Journ. Geol.*, XLVII, 1939, 208–420), and has welded it into a connected account. The mass has the form of a laccolith which appears to rest on a floor of limestone. The rock-types near the base have developed melanite, diopside, and nepheline, and the variety called borolanite carries what appear to be pseudomorphs after leucite. The author accounts for the observed differentiation by the assimilation of limestone at the base of the laccolith.

Notwithstanding the comprehensive work of the late W. C. Brögger, research on the great Oslo petrographic province has begun again, and is being published under the general title " Studies on the Igneous Rock Complex of the Oslo Region." Part I is a memoir by O. Holtedahl on " Some Structural Features of the District near Oslo." The second part, by T. W. F. Barth, deals with " The Systematic Petrography of the Plutonic Rocks " (*Norske Videnskap.-Akad. Oslo, I. Math.-Nat. Kl.*, 1944, No. 9, 104 pp.), and represents a very complete and penetrating revision of Brögger's work, with the systematic description, nomenclature, and comparative petrography of the plutonic rocks of the province. The work is mainly based on Brögger's slides and chemical analyses supplemented, however, by much new material. One of the most valuable things in Barth's work is the critical study of the term "essexite." Essexite has long been used for the alkali-gabbros of the Oslo region, but Barth has no difficulty in showing that it is a misnomer. Oslo essexites are identical with Iddings's "kauaiite," and differ considerably from the original essexites of Essex Co., Mass.

A most useful collection of published "Chemical Analyses of Igneous and Metamorphic Rocks of Denmark, the Faeroes, and Greenland" has been made by S. Munck and A. Noe-Nygaard (*Danmarks Geol. Undersög.*, II Ser., No. 68, 1942, 105 pp.); 218 analyses are listed and a brief petrographical description of each rock is given. In addition, 37 analyses of the telluric irons of Disko and other places, and 4 of meteoric irons are given. This adds one more to the "national" collections of rock analyses which have been compiled for Great Britain, Switzerland, Sweden and Finland.

Some of the geological results of the Danish Expedition of 1939 to West Greenland are now coming to hand. A. Noe-Nygaard, for example, has published a comprehensive account of the "Plateau Basalts of Svartenhuk Peninsula" as Part III of a series entitled "On the Geology and Petrography of the West Greenland Basalt Province" (*Medd. om Grönland*, **137**, No. 3, 1942, 78 pp.). The plateau basalts are underlain by a thick basalt breccia made up of glassy basalts (sideromelane) often pillow-form, and tuffs which, in turn, rest on a basement of Pre-Cambrian and Cretaceous. Sills in the basement consist of dolerites with pigeonite and olivine. The plateau basalts proper include richly olivine-bearing lavas ("schönfelsite," "masafuerite," and oceanite), olivine-basalts, olivine-free basalts, andesitic basalts, and anorthoclase-trachytes. Six chemical analyses are available which show that, as in other parts of the North Atlantic Tertiary region, crinanite—olivine-basalt and tholeiitic magmas were simultaneously present. The evidence points to the origin of the lavas by subærial fissure eruption.

Part V of the same series is devoted to the description of "Two Major Doleritic Intrusions of the Nugssuak Peninsula" by S. Munck (*Medd. om Grönland*, **137**, No. 5, 1945, 61 pp.). One of the intrusions (Serfat) is a quartz-dolerite; the other (Atanikerdluk) is a quartz-bearing olivine-dolerite rich in titanium dioxide. The Serfat Sill is probably more than 100 m. thick. It is intruded by an ultrabasic sill 10–20 m. thick, which is to be described in another memoir. The Atanikerdluk Sill seems to be much thinner. Both are probably connected with the flood-basalt eruptions of the region.

R. A. Daly discusses " Volcanism and Petrogenesis as illustrated in the Hawaiian Islands " (*Bull. Geol. Soc. Amer.*, **55**, 1944, 1363–1400). Hawaiian lavas are regarded as differentiates, largely controlled by density, of a primary olivine-basalt magma. Juvenile gases in the volcanic conduits are believed to play an important part in differentiation, both as flux and as inciters of convection. To this action the trachytic lavas especially are assigned. Ankaramites and oceanites (picrite-basalt and chrysophyric basalt of American petrographers) are regarded simply as due to the settling of early-formed olivine. Alkaline and melilite-bearing lavas are believed to be due to reaction of primary basalt magma with sediments rich in carbonates. Incidentally, Daly adversely criticises Kennedy's conception of two kinds of primary basalt magma (olivine-basalt and tholetiie) on grounds which the writer of these notes thinks inadequate.

The important observations of G. A. Macdonald on " The 1840 Eruption and Crystal Differentiation in the Kilauean Magma Column " (*Amer. Journ. Sci.*, **242**, 1944, 177–89) are best summarised in his own words : " During the 1840 eruption of Kilauea volcano olivine-basalt almost devoid of olivine phenocrysts was erupted from a group of vents at high altitude nearly simultaneously with the eruption, from a group of vents at low altitude, of picritic basalt containing abundant large olivine phenocrysts. The difference in composition between the lavas of the two groups is largely or entirely caused by the presence of the olivine phenocrysts in the picritic basalt. Gravitative crystal differentiation in the magma column appears to have produced an upper portion devoid of olivine phenocrysts from which were derived the lavas of the upper vents, and a lower portion enriched in olivine phenocrysts which supplied the lavas of the lower vents."

It has been suggested that the well-exposed ring-dike and cone-sheet structures first described from the Tertiary igneous fields of the west of Scotland represent the roots of great shield volcanoes of Hawaiian and Icelandic type. Hence G. A. Macdonald's paper on " Ring Structures at Mauna Kea, Hawaii " (*Amer. Journ. Sci.*, **243**, 1945, 210–17) provides welcome evidence in favour of the above view. He writes : " Besides the radial rift zones characteristic of Hawaiian volcanoes, Mauna Kea also shows arcuate alignments of cinder cones along lines concentric to the eruptive axis of the volcano. These indicate the existence of arcuate fissures and groups of fissures, occupied beneath the surface by ring-dikes or cone-sheets."

H. T. Stearns has made a comprehensive study of the "Geology of the Samoan Islands" (*Bull. Geol. Soc. Amer.*, **56**, 1944, 1279–1332), especially the American island of Tutuila. The latter was built by five volcanoes over at least two parallel rifts trending N. 70° E. A caldera 6 miles across was formed by collapse of the top of the Pago volcano. It was partially filled by a thick series of tuffs and lavas, and several bulbous trachyte domes penetrated the caldera filling to the surface. About 600 dikes and 40 faults are recorded. Two of the dikes are unusual in that they contain oriented xenoliths of dunite; another contains bedded ash.

G. A. Macdonald follows with the "Petrography of the Samoan Islands" (*ibid.*, 1333–62). The five great shield volcanoes of Tutuila are all largely built of olivine-basalts, with picritic basalts, olivine-poor basalts, olivine-free basalts, andesine- and oligoclase-basalts also represented. The other islands have the same general composition. The post-caldera plugs consist of quartz-trachyte. All available chemical analyses of Samoan lavas are assembled. The suite is alkalic, but near the boundary with the alkali-calcic series with an alkali-lime index of 50·5. Differentiation probably proceeded mainly by crystal settling from the parent olivine-basalt magma.

In the memoir on the "Geology of Borabora, Society Islands" (*Berenice P. Bishop Mus. Bull.*, **169**, 1941, 43 pp.), J. T. Stark and A. L. Howland describe the lavas of Borabora as all olivine-bearing, ranging from basalts with 5 per cent. of olivine to oceanite with 50 per cent. Ten new analyses are provided. Borabora, like other islands of the Leeward Group, is the eroded cone of an extinct and formerly much larger volcano. Fragmental volcanic material is conspicuous by its scarcity. The lavas range in thickness from 1 to 15 ft. with an average of 6 ft. In many places ellipsoidal structures resembling "pillow-lavas" are found, which are ascribed to a kind of "billowing effect" in viscous flows due to local increases of gas pressure. The geological history seems to preclude submergence and therefore subaqueous origin for these structures. The authors have apparently overlooked an explanation which has been successfully applied to very similar phenomena in the Mull volcano of Scotland, namely, that the pillow structures are due to extrusion into long-standing crater lakes.

W. N. Benson has published a series of five papers on "The Basic Igneous Rocks of Eastern Otago and their Tectonic Environment." Part I (*Trans. Roy. Soc. N.Z.*, **71**, Pt. 3, 1941, 208–22) deals with the general tectonic environment. The Dunedin region exhibits a long and varied sequence of Late Tertiary igneous rocks, mainly of alkaline nature, which cover 110 sq. miles out of a total of 250 sq. miles mapped in detail. Part II (*ibid.*, **72**, Pt. 1, 1942, 85–110) describes the distribution of the eruptive rocks in relation to the regional tectonics, and the petrography and chemistry of the Late Tertiary igneous rocks of the relatively stable Dunedin region. The rock types include olivine-basalts of various types, mugearites, atlantites (melanocratic nepheline-basanites), olivine-theralite and olivine-nephelinite. Part III (*ibid.*, **72**, Pt. 2, 1942, 160–85) describes a sill of olivine-theralite which has undergone marked gravitational differentiation. The downward sequence of types in this sill recalls that of the Lugar Sill, with which the New Zealand occurrence is compared.

Part IV is divided into two sections, the first of which deals with the geology of the Mid-Tertiary basalts, tholeiites and dolerites of north-east Otago (*ibid.*, **73**, Pt. 2, 1943, 116–38). The second section discusses the petrology of these rocks (*ibid.*, **74**, Pt. 1, 1944, 71–123). The average magma of this sub-province resembles that of the British tholeiites and quartz-dolerites, but is richer in soda as is shown by the occurrence of pegmatoid analcite-syenite and anorthoclase-bearing olivine-basalt.

In a paper on "Cainozoic Petrographic Provinces in New Zealand and their Residual Magmas," W. N. Benson (*Amer. Journ. Sci.*, **239**, 1941, 537–52) distinguishes three associations, namely, (1) a rhyolite-dacite-andesite-basalt association in North Island; (2) basalts and andesites in Banks Peninsula, South Island; and (3) the olivine-basalt-trachybasalt-trachyte-phonolite association of the Dunedin region, described in Benson's papers above cited. The geological environment, tectonic history, and sequence of igneous events in each province are summarised. The North Island province is definitely of calcic character, just as that of the Dunedin district is definitely alkalic; but the Banks Peninsula province shows intermediate characters.

In a study of the "Petrology of the Tertiary Older Rocks of Victoria," A. B. Edwards (*Proc. Roy. Soc. Vict.*, LI, Pt. 1, 1939, 73–98) has shown that these igneous rocks are petrologically distinct from the Newer Series (see SCIENCE PROGRESS, XXXIV, 1939, 334). They consist of crinanites, olivine-titanaugite-dolerites and basalts, olivine-basalts, olivine-nephelinites, limburgites, monchiquites and camptonites, and possible acid differentiates such as tinguaites and phonolites. The rocks occur both as lavas and as dike-swarms

The suite could have been derived from an olivine-basalt magma comparable with Kennedy's crinanitic magma.

The "Dolerites of Tasmania," described by A. B. Edwards (*Journ. Geol.* **50,** 1942, 451–80; 579–610), are closely akin to those of the Karroo, and especially to those of Antarctica. They occur as huge sills, sometimes more than 1000 ft. in thickness, and as great dikes a mile or more wide, which penetrate horizontally-bedded Permo-Carboniferous and Trias-Jura strata. These intrusions underwent fractional crystallisation and gravitational differentiation after emplacement, which have given contrasted results in the sills as compared with the dikes. In the closed chambers of the sills sinking of the early crystals of magnesia-rich pyroxene led to slight absolute enrichment in iron of the residual magma which was displaced upwards. The iron-rich pyroxene thus formed was retained in the upper parts of the sills owing to the inability of its crystals to penetrate the crystal-clogged layers beneath. Thus peculiar iron-rich rocks were formed in the upper parts of the sills. In the great dikes, however, there was no such inhibitive action, and the concentration of iron in the residual magma was dissipated. The trend of differentiation in the dikes thus gave rise to andesitic rather than iron-rich types. This idea of the dependence of differentiation on the form of the magmatic chamber is of importance in petrological theory, as it seems to reconcile the conflicting views of Bowen and Fenner on the enrichment in iron of the residual liquids of basaltic magmas.

A. B. Edwards has also described "The Crinanite Laccolith of Circular Head, Tasmania" (*Proc. Roy. Soc. Vict.*, **53,** (N.S.), Pt. 2, 1941, 403–15) as a mass originally more than 500 ft. thick in its central part, which was injected into Permo-Carboniferous sediments. Like many other doleritic sills, it has a marked concentration of olivine and augite at about one-quarter of the thickness up from the base of the sill. The intrusion contains nepheline-rich segregations and veins.

"Macquarie Island: Its Geography and Geology" is the subject of a fine memoir by Sir D. Mawson (*Australian Antarctic Exped.* 1911–14. *Sci. Repts.*, Ser. A, Vol. V, 1943, 194 pp., 46 figs., 37 pls.). The island is situated 800 miles south-west of New Zealand. The oldest formation is a thick series of basaltic lavas and sills probably of Mid-Mesozoic age, possibly to be correlated with the large-scale dolerite intrusions of Tasmania and Antarctica. These rocks, with the associated tuffaceous greywackes, were subjected to folding, and a series of plutonic masses were injected into the cores of the folds. These latter consist of gabbro, norite, olivine-gabbro, troctolite, eucrite, allivalite, harrisite, harzburgite and dunite—in short, a series remarkably parallel with the Tertiary plutonic masses of Skye and Rum. Comparison with the Tertiary suite of the West of Scotland is made more exact by the occurrence of teschenites, analcite-basalts, etc., associated with tholeiitic types.

A " Report on Rocks from West Antarctica and the Scotia Arc " by G. W. Tyrrell (*Discovery Reports*, XXIII, 1945, 37–102) consists of five papers dealing respectively with the petrography of the South Shetland Islands; Graham Land peninsula and Adelaide Island; the Elephant and Clarence Group; the Shag Rocks west of South Georgia; and the South Sandwich Islands. Mr. J. M. Wordie has supplied a Foreword which brings together the geological results of these investigations. The discovery of a new volcano (Penguin I) in the South Shetlands by J. W. S. Marr; the identification of specimens from the vicinity of the Shag Rocks, the unknown part of the Scotia Arc, as tremolitic greenstones which can be correlated with similar greenstones from Clarence I and Tierra del Fuego, are important new points. The work has completely confirmed the diagnosis of E. Suess—that the Scotia Arc is a vast protrusion of Circum-Pacific structures and rocks into the heart of the South Atlantic.

A considerable contribution to the geology and petrology of the Sunda Islands, with many new analyses, has been made by H. A. Brouwer in a paper: " Geological and Petrological Investigations on Alkali and Calc-alkali Rocks of the Islands Adonara, Lomblen, and Batoe Tara " (*Geol. Exped. to the Lesser Sunda Islands*, II, 1940, 94 pp.). The islands consist mainly of the products of extinct and still-active volcanoes, pyroxene-andesites and olivine-basalts in Adonara and Lomblen, with trachybasalts and trachyandesites from the younger volcano on Lomblen. Batoe Tara is the upper part of a volcano emitting potash-rich lavas mainly leucite-basanite. Brouwer considers that the lesser Sunda Islands afford one of the clearest examples of a simple relationship between tectonic and volcanic evolution.

Numerous intrusions of the charnockite series occurring in the western margin of the Eastern Ghats in Southern Jeypore are discussed by H. Crookshank (*Rec. Geol. Surv. India*, **73**, Pt. 3, 1938, 398–434). He shows that their metamorphism is similar to that of the older gneisses, with even more marked stress phenomena. It is concluded that the hypersthene gneisses and gneissic soda granites are of igneous origin and belong to the charnockite series. Solutions given off by the charnockite magmas have effected a marked degree of alkali metasomatism.

"The Charnockite Series of Bastar State and Western Jeypore," according to P. K. Ghosh (*Rec. Geol. Surv. India, LXXV, Prof. Paper*, No. 15, 1941, 55 pp.), represent impure calcareous sediments rich in iron and magnesium, now metamorphosed to diopside-gneisses. Rise of temperature and formation of hypersthene from diopside led to the basic charnockites. Some stress at this stage produced garnet from diopside and plagioclase, and further stress led to the appearance of green and blue hornblende. Finally, there was an influx of alkaline fluids, the dominance of potash in which led to the formation of acid charnockites. The basic and ultrabasic members of the series are thus ascribed to thermal and dynamic metamorphism, the acid members to a process of assimilation and replacement.

B. Rama Rao has discussed "The Charnockite Rocks of Mysore (Southern India)" (*Mysore Geol. Dept. Bull.*, No. 18, 1945, 199 pp.). He deals fully with the field geology, mineralogy and petrography of this enigmatic series of rocks. He shows that myrmekitic texture, microperthitic intergrowths, dusty inclusions in quartz and feldspars, and granulitic structure, often thought to be distinctive of the charnockite series, are characteristic of all the formations present, and are due to regional metamorphism which has affected all rocks in common. Rao regards the basic and ultrabasic members as undoubtedly of igneous origin, but the acid and intermediate types may comprise not only acid intrusions but also the invaded rocks. Charnockites are "essentially reconstructed rocks, and have resulted from several phases of recrystallisation due to thermal metamorphism at different periods."

ZOOLOGY. By William Holmes, M.A., D.Phil., The University, Oxford.

Nephridia and Cœlomoducts.—The science of comparative anatomy can never claim a grander achievement than that of bringing within a single scheme the morphology of the excretory and reproductive systems of all triploblastic animals. Fifty years ago Professor E. S. Goodrich drew the attention of British scientists to the theory that the cœlom of the higher Metazoa has evolved from the genital follicles of the lower acœlomate types; at the same time he drew the fundamental distinction between "nephridia," which develop from regions of the body more superficial than the lining of the cœlom, and "cœlomoducts," which are extensions of the cœlomic cavity itself by centrifugal outgrowth from its walls. In doing so he made a major contribution to a study which now, fifty years later, he has reviewed in "The Study of Nephridia and

Genital Ducts since 1895" (*Quart. J. Micr. Sci.*, **86**, 1946, 113–392). Here all the morphological information accumulated in this period is reviewed, the various forms of the organs are illustrated by drawings, and their relationships are demonstrated by comparative diagrams: the analysis gives repeated confirmation of the principles outlined by Goodrich in 1895.

As a result of the studies summarised in this review our morphological knowledge of the nephridial organs is profound, but in contrast what we know of their physiology is fragmentary and often inconclusive. Kromhout (*J. Morphol.*, **72,** 1943, 167–82) has made "A comparison of the protonephridia of the fresh-water, brackish-water and marine specimens of *Gyratrix hermaphroditus*." The three forms of this turbellarian differ in the structure of the protonephridial system. The complete system, as seen in the fresh-water form, consists of flame cells, syncitial tubules enveloped by "paranephrocytes," and an ampulla which leads to the excretory pore. In the brackish-water form the tubules are less distinct and the paranephrocytes are absent; in the marine form tubules, paranephrocytes and ampullæ are all lacking. On Kromhout's interpretation the flame cell may be regarded as the organ concerned in the extraction of waste products from the tissues, and the ampullæ and tubules are the parts of the protonephridial system concerned in osmo-regulation, since the water-balance problem is more serious in fresh water. It may be taken for granted that since most excretory organs extract waste products in solution then they must play some part, however small, in maintaining the water-balance of the animal. We may perhaps conclude with Kromhout that the nephridia in *Gyratrix* play more than an incidental part in osmo-regulation, and deal with all water taken in inevitably from a hypotonic environment; nevertheless, it should not be forgotten that in the euryhaline form *Gunda* earlier work has attributed to the epithelium of the gut the chief rôle in the control of the water content of the animal.

EXCRETION IN THE EARTHWORMS.—The mode of excretion and osmo-regulation practised by the earthworms has been studied from many aspects, but so many processes have been considered to contribute to the whole mechanism that our knowledge remains imperfect; the functions of the nephridia have proved surprisingly elusive. However, since the publication of Stolte's review (Bronn's "Tierreich," Band 4, Abt. 3, Buch 3, Lief. 4) a most impressive attack on the problem has been made by Bahl (*Quart. J. Micr. Sci.*, **85**, 1945, 343–90). Bahl had previously made a thorough investigation of the morphology of the nephridial system of the Indian earthworms (see Goodrich, *supra*), and the worm *Pheretima* was the main subject of his present study. Although the nephridia of this worm differ from those of the British earthworms in some respects, most of Bahl's conclusions may be generalised with the reservations noted in his discussion. Bahl describes in detail all the biochemical techniques of which he made use, so that his experiments may be repeated and checked.

Analysis of the urine (nephridial exudate) of the worm showed that the chief nitrogenous waste-products are urea and ammonia; no uric acid is present. The cœlomic fluid, freed from corpuscles, was then investigated; it contains both urea and ammonia, the former being present at a slightly lower concentration than in the urine. This is at the same time the first demonstration that the cœlomic fluid contains nitrogenous excreta and an indication that the nephridia may derive some urea from a source other than the cœlom. Since the nephridia are highly vascular structures it is significant that urea was found in the blood at about the same concentration as in the cœlomic fluid; ammonia was also present, but at a lower concentration. The blood is thus shown to play a part in the transport of excretory products as well as in that of oxygen.

The chloragogen tissue, lying in the intestinal wall, has often been credited with a hepatic function in the excretory processes of the earthworm. Bahl demonstrated that the gut wall does in fact contain a higher percentage of urea than is found in the body-wall, cœlomic fluid or urine, and concludes that the chloragogen tissue is an important site, if not the central organ, of urea formation. The "renal" functions of the nephridium were then investigated. It was concluded that the fluid entering the open nephridial funnels is free from corpuscles, but is identical with the cœlomic fluid in its content of colloidal protein; this protein is, however, reabsorbed into the body during the passage of the fluid down the nephridial tubes with the formation of the urine. If urine is excreted into the tubes from the blood vessels it is presumably a protein-free filtrate. There is a large-scale reabsorption of chlorides as well as of protein during the passage of urine through the tubules, and the urine as finally excreted is hypotonic both to the blood and to the cœlomic fluid; the nephridium thus plays an important part in controlling the water-balance of the worm.

The ciliated middle tube of the nephridium has long been known to contain brownish granules, and on the basis of an earlier biochemical study these have always been accepted as containing guanine, for which excretory product the nephridium was considered to serve as a "kidney of accumulation." But by spectroscopic and other tests Bahl has shown that these granules are in fact composed of hæmochromogen, and he concludes that the nephridia serve as storage kidneys for the products of blood destruction.

It will be recalled that in standard accounts of the excretory processes of the earthworm a considerable rôle is attributed to phagocytic cells, of which many are found in the body cavity. Liebmann (*J. Morphol.*, **71**, 1942, 221–50) has followed up his earlier work by a study of the corpuscles in the cœlomic fluid. These he divides into two categories: eleocytes and leucocytes. The eleocytes are formed and liberated by division of the chloragogen cells; they are non-amœboid, and contain lipide and possibly glycogen, and in their earlier developmental stages protein also, all in the form of granules. The leucocytes are phagocytic; they feed on broken-down eleocytes, bacteria and other parasites, and the main channels for the excretion of such material is by the formation of the "brown bodies." The structure of these bodies, which usually contain larval nematodes, gregarine cysts and old setæ and are excreted through the dorsal pores in the hind segments of the worm, is described by Tu (*Zool. Jahrb., Abt. Anat.*, **63**, 1937, 73–124).

According to Liebmann the eleocytes are not concerned in excretion; they migrate from the wall of the alimentary canal to all parts of the body loaded with food material, which they liberate to the tissues. They are particularly active when the body of the earthworm has been severed and regeneration is in progress. Leibmann does not exclude the possibility of urea formation by the chloragogen tissue, but regards its rôle in nutrition, via the eleocytes, as being its main function. Indeed, in a later paper (*J. Morphol.*, **73**, 1943, 583–610) he concludes that the chloragogen tissue "appears to determine regeneration in its various aspects." It will be seen that Bahl's investigation of the excretory processes does not exclude the phagocytes from a part in excretion, but it does not require them. Liebmann, on the other hand, casts considerable doubt on the traditional interpretation of the functions of the chloragogen tissue. Thus in spite of these advances our knowledge of the process of excretion in the earthworm remains incomplete.

THE NERVOUS SYSTEM OF THE EARTHWORM.—Running nearly the whole length of the ventral nerve cord of the earthworm are three "giant" nerve fibres. One of these fibres is median and dorsal, the other two lie laterally on either side of it. These fibres constitute in their structure an exception to the general neurological principle of the unity and distinction of neurons, for each of them is formed of a chain of segmental axonal units, and each of these units is itself a syncitium, formed of the fused axonic processes of numerous nerve cells located in the segmental ganglia on the nerve cord. Morphologically these fibres are well known and Rushton has now made a detailed investigation of their physiological properties (*Proc. Roy. Soc.*, **B 132,** 1944, 423–37). The segmental units in the chain of which each fibre is composed are separated from each other by a septum, which may be considered as comparable with a synapse. But these septa are unlike the usual polarised synapses of vertebrates in that both median and lateral fibres can conduct impulses in either direction. The impulse travels to both ends of the fibre when it is directly stimulated at any point. Another unusual characteristic is the fact that nerve impulses suffer no appreciable delay (comparable with synaptic delay) when they cross the septa in the process of transmission along the fibre: thus each fibre, although segmental in structure, is functioning as a single unit in impulse transmission.

It has long been known or guessed that the earthworm giant fibres subserve the transmission of the nerve impulses that cause the rapid shortening reaction of the " frightened " worm, causing it to retract hastily into its burrow. The nerve impulse passes rapidly along the fibres, by virtue of their large diameter, and excites the motor nerve branches of each segment as it passes through the ganglia. If both median and lateral fibres can conduct impulses in both directions, why are three fibres necessary to mediate the shortening reaction? Rushton has now shown (*Proc. Roy. Soc.*, **B 133,** 1946, 109–20) that in the intact worm sensory stimulation of the head region—that is, anterior to the posterior border of the clitellum—causes an impulse to pass in the median fibre only, while stimulation behind that point produces an impulse in the two lateral fibres but not in the median. Thus although both median and lateral fibres are unpolarised they are excited by sensory fibres covering distinct peripheral zones.

Bullock (*J. Neurophysiol.*, **8,** 1945, 55–71), working on the same problem by similar methods in the United States, has arrived independently at conclusions which are in most respects in agreement with those of Rushton. He has further demonstrated that when the animal is elongated the rate of conduction in the giant fibres is greater than when it is in the shortened condition.

The visceral nervous system of the earthworm has been reinvestigated by Millot (*Proc. Roy. Soc.*, **B 131,** 1943, 271–95 and 362–73). The nerve supply to the gut is derived from two distinct sources:

one channel comes from the peripharyngeal commissures, and the other, previously undescribed, arises from the whole length of the ventral nerve cord by way of the segmental ganglia. Millot makes the most interesting observation that these two distinct visceral systems are antagonistic in their action, one increasing the tone of the gut, the other decreasing it. The analogy with the condition in mammals is made even more striking by the fact that he finds evidence of chemical mediation in the transmission of the nerve impulses to the gut muscles, the nerves increasing the tone of the gut being cholinergic, those decreasing it adrenergic. In a third paper (*Proc. Roy. Soc.*, **B 132,** 1944, 200–12) he describes his studies of the nervous control of secretion by the intestinal digestive glands; the secretion of protease at the anterior end of the intestine is under nervous control, though the possibility that some of the control is non-nervous is not excluded.

A NEW CRUSTACEAN ORDER.—Pennak and Zinn have described a new entomostracan *Derocheilocaris typicus* in a paper entitled "Mystacocarida, a new order of Crustacea from the intertidal beaches in Massachusetts and Connecticut" (*Smithsonian Misc. Coll.*, **103,** 1943, No. 9).

Sixty-five specimens of this animal, all females, were discovered in the capillary waters, between the sand grains, of the intertidal beaches of the regions investigated. At first the authors believed it to belong to the Copepoda, but further study showed that it could not be attributed to this group. Its most striking differences lie in the number of segments comprising the head, thorax and abdomen, and in the structure of the appendages which, except for the first and second antennæ, differ markedly from those of Copepods. An unusual feature is that the genital pore is in the first thoracic segment. Five larval stages are described. The authors conclude that this new crustacean must constitute a new Order, the Mystacocarida, which they consider as close to the Copepoda but more primitive than any known living crustacean group.

CŒLENTERATE NEMATOCYSTS.—Two sides of the problem of the mode of action of nematocysts have recently been reinvestigated. First, what is the natural stimulus that excites the cnidoblast? and, second, what is the mechanism by which the nematocyst is discharged? Pantin has studied "The Excitation of Nematocysts" in *Anemonia sulcata* (*J. Exp. Biol.*, **19,** 1942, 294–310). He concludes that the cnidoblasts are independent effectors, since the nematocysts are only discharged at the site of stimulation, and there is no sign of the spreading of reaction such as would indicate a physiological connection between the cnidoblasts and the nervous system. The primary effective stimulus is always mechanical—discharge is " easily " produced by solid food, but by inert solids only when the mechanical stimulus is greater than that which is effective if the stimulating solid is a food substance. This indication that chemical sensitisation plays some part in the normal reaction to food was confirmed by further experiments. Most food solutions and solutions of food derivatives do not cause a discharge, but they sensitise the cnidoblasts so that they react more readily to a given mechanical stimulus. Pantin further concludes that the sensitising substance is a lipoid strongly adsorbed on to protein, and that sensitisation is normally by direct contact between the solid containing the substance and the cnidoblast.

The problem of the mechanism of discharge of the nematocyst is discussed by Kepner *et al.* in a study of " The Structure, Development and Discharge of the Penetrant of *Pelmatohydra oligactis* " (*J. Morphol.*, **72,** 1943, 561–84). This paper may conveniently be read in conjunction with a brief summary of earlier work on " The Nematocysts of Hydra " by Baker (*Sch. Sci. Rev.*, 1938, 261–7). In the type of nematocyst studied by Kepner the part of the organ which is ejected on stimulation, as seen after discharge, consists of a filament or " stinging thread " based on a shaft which is provided with three large stylets and three clockwise spiral ridges bearing spines; the stinging thread forms most of the length of the discharged penetrant. The main point in which Kepner differs from earlier workers is in regard to the structure and mode of formation of the stinging thread. It was formerly considered that the whole penetrant in the undischarged state is an invaginated tube, and that discharge consists of an evagination by which the wall of the penetrant that was internal before discharge becomes external after it. Kepner believes, however, that the filamentous part of the penetrant, which is coiled up in the undischarged state, is not a tube, but a solid rod of a gel-like colloidal material. The initial step in discharge is a widening of the mouth of the capsule as a result of mechanical pressure (the rôle of the cnidocil remains uncertain), and there is a consequent release of the operculum. Water enters the lumen of the cyst and transforms the jel-like rod into a fluid. This change in the physical state of the colloid generates an internal pressure; the fluid colloid is forced out *before* the eversion of the shaft and its stylets, and the fluid as it emerges undergoes a second transformation of physical state, becoming again a gel-like rod, but longer and thinner than before: as it emerges it is marked by the spiral " rifling " of the spines on the unevaginated shaft. The last stage in discharge is the evagination of the shaft and its armature. This paper also contains some observations on the development of cnidoblasts, of which the penetrant is described as a "secretion product."

ENTOMOLOGY. By A. D. LEES, M.A.,Ph.D., Agricultural Research Council, Unit of Insect Physiology, Cambridge.

THE INSECT CUTICLE.—In recent years considerable progress has been made in our understanding of the process of hardening and darkening which takes place in the cuticles of many insects after moulting or pupation. It had previously been thought that darkening was due solely to the formation of melanin products as a result of the oxidation of tyrosine. But in 1940 M. G. M. Pryor (*Proc. Roy. Soc.*, **B 128**, 378 and 393) drew attention to the fact that the cockroach ootheca, an object containing no chitin, but which hardens and darkens in a manner reminiscent of insect cuticle, is secreted by two glands, one of which secretes a water soluble protein and the other a dihydroxyphenol. The latter is oxidised to the corresponding quinone and reacts with the protein to form a resistant material, sclerotin. This "tanning" process, resulting from the introduction of cross-linkages into the protein molecule, is thus very similar to the familiar tanning of collagen by benzoquinone: both processes lead to a darkening and toughening of the material. Pryor also demonstrated the presence of reducing substances, probably dihydroxyphenols, in the hard exo- and epicuticles of several insects, and he suggested that normal cuticular hardening was due to a similar reaction. The blood of insects also contains a phenol which does not, however, react with protein. This phenol is probably "dopa" or tyrosine and, after deamination, it will serve as the source of the cuticle dihydroxyphenols. Although the cuticle may contain protein and phenols no darkening takes place until the latter is oxidised by enzymes to the corresponding quinone. In the larva of the blowfly *Sarcophaga* the inner layer of the epicuticle contains an oxidase and the future exocuticle dihydroxyphenols (R. Dennell, *Nature*, 1944, **154**, 57). At pupation darkening commences where enzyme and substrate meet and gradually spreads inwards. In *Sarcophaga* the deposition of phenol in the cuticle coincides with the appearance of a related phenol in the blood, arising from the oxidation of tyrosine by tyrosinase secreted by small blood cells, the œnocytoids. In spite of the presence of both enzyme and substrate, however, no melanin is deposited in undrawn blood, and it has been suggested that oxidation is held in check until pupation by the presence of a dehydrogenase.

Almost simultaneously with Pryor's work, G. Fraenkel and K. M. Rudall (*Proc. Roy. Soc.*, 1940, **B 129,** 1) advanced a rather different view of cuticle hardening, as a result of chemical and X-ray studies of the blowfly cuticle. They observed that during the hardening of the puparium the water content of the cuticle falls, and that a higher degree of chitin orientation and a closer packing of the puparial substance is achieved. How far such changes are secondary to the process of "tanning" has not yet been decided.

Pryor's conclusions are supported by the work of M. Lafon (*Ann. Sci. Nat., ser. Bot. et Zool.*, 1943, **11,** 113) on the chitin and protein content of a variety of arthropod cuticles. In different species of insects the proportion of chitin and protein remains fairly constant, about 30 per cent. of the total cuticle weight being due to chitin and rather more than 30 per cent. to protein. There is no connection between chitin content and hardness. In the Decapod Crustacea, on the other hand, the cuticle is heavily impregnated with mineral salts and chitin may form as much as 75 per cent. of the total organic matter, protein being reduced to little more than 10 per cent. Lafon draws the conclusion that protein has little structural value in the Crustacea since rigidity is achieved by impregnation with salts. It is interesting that in the Myriapoda, the Chilopod exoskeleton resembles the insect cuticle in richness in protein and, as in insects, hardening can be ascribed to a tanning reaction. The Diplopods, on the other hand, have thick calcified cuticles and the protein content is correspondingly low.

During the tanning of protein with quinones the material goes through a series of colour changes from light brown to mahogony and finally black. The natural course of pigmentation during the darkening of the puparium of *Calliphora* resembles such changes much more closely than does the immediate blackening which takes place on immersing the puparial cuticle in tyrosine (Lafon, *loc. cit.*). In some insects, however, it is likely that both reactions go forward concurrently. Thus in the darkening of the puparium of *Drosophila*, two processes can be distinguished, a browning reaction, probably due to tanning, and a blackening, due to melanin deposition (C. H. Waddington, *Proc. Zool. Soc.*, 1941, **A 111,** 173). These reactions are under genetic control, both the quantities of the end products and the timing of the reactions being variable. The gene "straw," for example, reduces both phases of pigmentation while "ebony" reduces the tanning of the puparium, which is therefore abnormally pallid, but increases the pigmentation of the adult fly.

Cuticle Permeability.—It has long been known that the insect cuticle is an effective barrier to the passage of water through it, and that this is a property of the epicuticle or outermost, lipoid-containing layer. As a result of work on the cuticle of the blowfly larva H. Hurst (*Trans. Farad. Soc.*, 1943, **39**, 390; *Brit. Med. Bull.*, 1945, **3**, 132) advanced the view that the epicuticle was essentially a lipo-protein mosaic and that the action of fat solvents such as kerosene in facilitating the entry of polar liquids through the cuticle was due to the displacement of " labile " lipid within the cuticle framework.

Recent work (V. B. Wigglesworth, *J. Exp. Biol.*, 1945, **21**, 97; J. W. L. Beament, *ibid.*, p. 115) on a variety of insects has greatly clarified our ideas on this subject. In contrast to Hurst's views it has been found that the epicuticle is a layered system and that most insects are waterproofed by a uniform layer of wax, about $0{\cdot}25\mu$ in thickness, overlying the general framework of the epicuticle. The wax film is too thin to be easily seen but its presence can be convincingly demonstrated by indirect means. If insects are rubbed with certain abrasive mineral dusts water loss through the cuticle is enormously increased. In such insects the epicuticle appears quite untouched in ordinary microscopic preparations but if the insect is immersed in ammoniacal silver solutions the polyphenols underlying the wax layer reduce the silver at the points where the wax has been abraded. In soil insects, such as wireworms, the movement of the insect through the soil particles causes a natural abrasion of the cuticle: the wax layer in insects taken from the soil is often heavily scratched and the insects themselves are exceedingly permeable to water. Treatment with wax solvents, such as chloroform, and wax detergents also greatly increases evaporation from the cuticle.

A point of great interest is that living insects are able largely to recover their impermeability if they are rubbed with dust and then kept in moist air to prevent them from drying up. This is accomplished by the deposition of wax on the *outer* surface of the cuticle over the damaged regions, the wax being secreted by the pore canals which extend through the cuticle from the epidermal cells. Such an observation draws our attention most strikingly to the fact that even the outermost layers of the cuticle are alive.

In the normal insect the long wax crystallites are probably closely packed and oriented vertically to the cuticle surface. If the temperature is raised, however, the wax molecules are subjected to increasing thermal agitation and their close alignment is lost, thus permitting water to escape more rapidly. The temperature at which such changes take place if intact insects are heated, varies widely in different species and depends on the nature of the waterproofing lipoid. In insects from dry environments, such as the blood-sucking bug *Rhodnius* or the mealworm *Tenebrio*, evaporation begins to increase dramatically between 50° and 60° C. In a less resistant insect, such as the leaf-eating caterpillar of the sawfly *Nematus*, this "critical temperature" is much lower, namely about 38° C.; and in the cockroach about 30° C. Insects may not lay down the same wax at all stages in the life history. Thus the larva of *Pieris* secretes a wax which permits increased evaporation at about 40° C.; the pupal wax, on the other hand, has a critical temperature higher by nearly 20° C. and is much more impermeable —a necessity if the pupa is to retain its limited water supply.

The waxes can be shown to be responsible for waterproofing by extracting and spreading them on artificial membranes. The evaporation of water from such films shows an abrupt increase at the same temperature as from the insects from which they were obtained. The extracted lipoids have graded properties, ranging from hard apolar waxes in *Rhodnius* and *Tenebrio*, through softer, lower melting-point waxes, as in *Nematus*, to the cockroach which is waterproofed by a mobile oil or grease with polar properties. The higher melting-point waxes are much better waterproofing agents than the lower melting-point members.

An efficient waterproofing system is an essential feature in the economy of most small terrestrial arthropods, and the cuticular waxes of insects in fact provide most effectively for the passive retention of water. The cuticles of other arthropods, such as ticks, are probably waterproofed by waxes in a very similar manner. But as well as the ability to retain water passively, ticks also possess the additional faculty of actively resisting desiccation and of taking up water through the cuticle from moist air (A. D. Lees, *Parasitology*, 1946, **37**, 1). This secretory mechanism is dependent on the integrity of the epidermis and disappears if the tick is killed or injured. Although different species of ticks show great diversity in their powers of resisting desiccation—a circumstance which is due to the type of wax secreted—all are able to take up water through the cuticle; indeed, the rate of uptake is much the same in species such as *Ixodes ricinus* and *Dermacentor andersoni*, even though the latter is far more impermeable. It seems as if the cuticle possesses asymmetric properties: the wax layer forms an effective barrier to the loss of water by evaporation, but it in no way hinders the active uptake of water in the reverse direction.

NEW SYNTHETIC INSECTICIDES.—A spectacular advance has been made in the field of economic entomology by the discovery of new chemicals which satisfy two of the prime requirements of good insecticides, namely, great toxicity to insects and the possession of lasting or residual properties. Two chemicals which have proved outstanding are DDT (2,2-*bis* (parachlorphenyl) 1,1,1-trichlorethane) and gammexane (the gamma isomer of benzene hexachloride). It is a curious coincidence that these materials should have appeared at a time when supplies of the vegetable insecticides pyrethrum and rotenone were cut off from this country and North America. The need for efficient substitutes during the war years has provided a potent stimulus to research on these insecticides.

Both DDT and gammexane are stable oil-soluble solids and are relatively non-toxic to man. Against insects both chemicals may function as contact and stomach poisons while gammexane has, in addition, a fumigant action. Gammexane has proved the more toxic against those insects, such as the housefly, in which a comparison has been made (R. Slade, *Chem. Trade J.*, 1945, **116**, 279), but the toxic properties of deposits of DDT are much more lasting. The following examples illustrate a few of the many uses of these new insecticides.

In the field of medical entomology (P. A. Buxton, *Trans. Roy. Soc. Trop. Med. and Hyg.*, 1945, **38**, 367) great progress has been achieved in controlling outbreaks of the human body louse, *Pediculus humanus corporis*. Adequate measures for killing lice in clothes have been available for many years, but it has been difficult to prevent reinfestation. Dusting the clothes with DDT will not only kill all the lice present but will prevent reinfestation for a week or two. The effectiveness of this procedure, when rigidly applied, was shown during the Naples typhus epidemic of 1944. An even more effective way of utilising the persistence of DDT lies in the use of garments impregnated with DDT from solutions or emulsions. Such "proofed" garments will continue to kill lice for several weeks in spite of repeated laundering, and their issue to the allied armies invading Europe in 1945–46 proved a very successful means of combating lousiness.

The use of DDT holds out great promise as a means for reducing mosquito populations, and perhaps even for the eventual control of malaria in rural communities. DDT as a solution or emulsion when sprayed on water is exceedingly toxic to mosquito larvæ, but the persistence of such films on large bodies of water under natural conditions is very variable. When there is insufficient time for control by larvicides, as in military campaigns, the control of the adult becomes vital. DDT oil sprays dispersed from aeroplanes over large tracts of jungle may immediately reduce the population of adult mosquitoes by as much as 98 per cent. (A. W. Lindquist and W. C. McDuffie, *J. Econ. Ent.*, 1945, **38**, 545); and in addition to killing mosquitoes on the wing there may be a reduction of night-biting Anophelines which are apparently killed after alighting on vegetation which has received a DDT deposit. In confined spaces the release of mists dispersed by compressed gasses, as in the freon "bomb," is a highly successful method of killing adults; but perhaps the most promising and economical method for the control of the adult is the deposition of residual films of DDT on the walls of houses and other resting-places of mosquitoes. Such films have no repellent effect and are highly toxic to mosquitoes settling on them. Moreover, this toxicity will persist under natural conditions for three months or more (see Discussion in Buxton, *loc. cit.*). It appears possible that if DDT is applied on an extensive scale a permanent reduction in the outside breeding population may be brought about. This was indicated, for example, in tests against *Anopheles quadrimaculatus* in rice-growing areas in North America (J. B. Gahan and A. W. Lindquist, *J. Econ. Ent.*, 1945, **38**, 223). This method may not be so successful, however, against species of mosquitoes which are disinclined to settle on walls, etc., after obtaining a blood meal.

Against domestic pests such as bedbugs which retire to crevices and are difficult to reach with ordinary sprays, a residual DDT spray offers the only effective alternative to fumigation. The toxicity of such deposits depends on the type of surface to which they are applied (S. Barnes, *Bull. Ent. Res.*, 1945, **36**, 273); tests in infested houses and army barracks have indicated, however, that a satisfactory deposit will remain highly toxic for many months (A. H. Madden, A. W. Lindquist and E. F. Knipling, *J. Econ. Ent.*, 1945, **38**, 265). DDT residual sprays do not appear to be outstandingly effective against cockroaches or crickets.

DDT has found many uses in the veterinary field. An interesting example is in the control of flies in cow byres and elsewhere (P. N. Annand *et al.*, *J. Econ. Ent.*, 1944, **37**, 125). Houseflies which are brought in with the cattle readily settle on the walls of the byre and can be killed there by a residual DDT deposit. The stablefly *Stomoxys* also settles on the walls after obtaining a blood meal and is killed. Complete protection against *Stomoxys* can also be secured by spraying cattle with DDT solutions, but the initial toxicity to this fly soon disappears. On the other hand, against the hornfly *Lyperosia* which never leaves its host except to oviposit in the dung, an extraordinary reduction in the permanent population can be achieved by one DDT spray application to the backs of the cattle. Other successful uses in this field include the spraying of vegetation to eradicate ticks (Annand *et al.*, *loc. cit.*) and its incorporation in sheep and cattle dips. Tests in this country have shown that by combining a DDT dip with a suitable attractant, sheep may be turned into a "fly-trap" for the sheep blowfly *Lucilia*. The flies are killed by the DDT retained in the fleece before they can oviposit (J. B. Cragg, *Nature*, 1945, **155**, 394).

The application of the new insecticides to problems of agricultural entomology has given somewhat variable results. It is clear that the toxicity to different insects varies enormously. A few examples may be given, mostly from the extensive American work (Annand *et al.*, *loc. cit.*). DDT is very toxic to soft-bodied larvæ such as the cabbage caterpillars *Pieris* and *Autographa*, and the codling moth (C. Graham, *J. Econ. Ent.*, 1945, **38**, 272). DDT sprays are very successful against the Colorado beetle, *Leptinotarsa*, and against the onion thrips. DDT can be effectively used in combination with a trapping device against the Japanese beetle, *Popillia* (G. S. Langford, M. H. Muma and E. N. Cory, *J. Econ. Ent.*, 1945, **38**, 199). In Europe the residual effect of DDT has been shown to be promising in the control of the apple blossom weevil, *Anthonomus pomorum*, which, because of its mode of feeding on the flower-buds, cannot be controlled with lead arsenate. On the other hand, DDT is of little use against the cotton boll weevil, *Anthonomus grandis*, or against the Mexican bean beetle, *Epilachna*; and it is not very poisonous to most species of aphids. Gammexane has been used on a large scale in England as a successful substitute for derris dusts in the control of the flea beetle, *Phyllotreta* (Slade, *loc. cit.*) and of the pollen beetle, *Meligethes*, on seed crops. And unlike DDT, gammexane is exceedingly poisonous to locusts when incorporated in baits.

An aspect of this subject which will probably be studied increasingly is the general effect on animal communities if indiscriminate use is made of these powerful new insecticides (V. B. Wigglesworth, *Atlantic Monthly*, 1945, **176**, 107). In connection with the use of DDT as a mosquito larvicide, it is already known that, as well as other insects, DDT will kill fish, tadpoles and smaller aquatic organisms if the dosage is sufficiently high (P. M. Eide, C. C. Deonier and R. W. Burrell, *J. Econ. Ent.*, 1945, **38**, 492). In the complex communities supported by cultivated plants the indirect effects of DDT treatments may be serious. Thus it has often been observed that orchards and crop plants such as sugar cane (J. W. Ingram, *J. Econ. Ent.*, 1944, **37**, 144) sprayed with DDT develop outbreaks of aphids and red spiders: these pests are resistant to DDT but their natural predators are exterminated.

Little is yet known of the mode of action of these insecticides. The rapid collapse and unco-ordinated, drunken movements of insects treated with DDT suggest that this poison is acting primarily on the peripheral nervous system. After touching one tarsus of the housefly with DDT, the effects can be seen spreading to the other legs according to a regular pattern; and amputated legs in DDT solutions may show sustained twitching movements (R. Laüger, H. Martin and P. Müller, *Helv. Chim. Acta*, 1944, **27**, 892). From the chemical aspect these authors have suggested that the CCl_3 grouping in the DDT molecule is responsible for its lipoid solubility, necessary for its rapid penetration through the cuticle, while the toxic properties are conferred by the linked *p*-chlorbenzene rings. A recent comparison of the toxicity of DDT analogues to *Cimex* and *Pediculus* has not, however, fully borne out this theory (J. R. Busvine, *Nature*, 1945, **156**, 169). It is probable that fresh advances will be made in the light of our increasing knowledge of the insect cuticle.

NOTES

Miscellanea

The following were elected to be Fellows of the Royal Society at the meeting held on March 21 : Agnes Arber, botanist, Cambridge ; Wilson Baker, professor of organic chemistry, University of Bristol ; G. L. Brown, Medical Research Council ; G. R. Cameron, professor of pathology, University College Hospital, London ; F. Dickens, director of research of the North of England Branch of the British Empire Cancer Campaign ; H. J. Emeléus, reader in inorganic chemistry, University of Cambridge ; Sir Frank Engledow, professor of agriculture, University of Cambridge ; E. B. Ford, reader in genetics, University of Oxford ; R. A. Frazer, National Physical Laboratory ; C. D. Gibb, chief engineer, Messrs. C. A. Parsons & Co., Ltd. ; E. A. Guggenheim, lecturer in chemical thermodynamics, Imperial College of Science and Technology ; R. Hill, Agricultural Research Council ; E. Marsden, physicist, New Zealand ; W. G. Penney, assistant professor of mathematics, Imperial College of Science and Technology ; J. T. Randall, professor of physics, University of St. Andrews ; R. O. Redman, chief assistant at the Radcliffe Observatory, Pretoria ; A. R. Richardson, mathematician, Cape Town ; L. Rosenhead, professor of applied mathematics, University of Liverpool ; J. A. Sinton, consultant malariologist to the War Office ; H. H. Storey, plant pathologist, Nairobi ; H. W. Thompson, University demonstrator in chemistry, Oxford ; J. W. Trevan, director of the Wellcome Physiological Laboratories ; L. R. Wager, professor of geology, University of Durham ; F. M. R. Walshe, physician to the National Hospital, Queen Square, London ; C. M. Yonge, regius professor of zoology, University of Glasgow.

Sir Ian Heilbron, professor of organic chemistry at the Imperial College of Science and Technology, has been awarded the Priestley Medal of the American Chemical Society. This medal is awarded every three years for distinguished services to chemistry.

The Gold Medal of the Royal Astronomical Society has been awarded to Prof. Jan H. Oort, director of the Leyden Observatory, for his investigation of the dynamics and rotation of the galactic system. A Jackson-Gwilt Medal and Gift has been awarded to Mr. H. W. Newton, of the Royal Observatory, Greenwich, for his work on the sun, notably on the relation between solar flares and magnetic storms.

Sir Edward Appleton, secretary of the Department of Scientific and Industrial Research, has been awarded the Faraday medal of the Institution of Electrical Engineers, and also the Trasenster Medal of the Association of Engineers of Liége.

Other medals whose award has been announced during this year include those of the Geological Society of London: the Wollaston Medal to Prof. Emmanuel de Margerie, the Murchison Medal to Dr. Leonard Hawkes, and the Lyell Medal to Dr. R. H. Rastall; the James Alfred Ewing Medal of the Institution of Civil Engineers to Prof. R. V. Southwell, Rector of the Imperial College of Science and Technology; the new Henry Stopes Memorial Medal of the Geologists' Association to Mr. A. S. Kennard, retiring President of the Association; the Colwyn Gold Medal of the Institution of the Rubber Industry to Dr. W. J. S. Naunton, of Imperial Chemical Industries, Ltd.; and the Bessemer Gold Medal of the Iron and Steel Institute to Mr. J. S. Hollings, of Guest Keen Baldwins Iron & Steel Co., Ltd.

Mr. D. N. Lowe has been appointed secretary of the British Association in succession to Dr. O. J. Howarth who retires after the annual meeting in July.

Mr. F. H. Rolt has been appointed superintendent of the Metrology Division, and Mr. A. Fage superintendent of the Aerodynamics Division of the National Physical Laboratory, in succession to Mr. J. E. Sears and Mr. E. F. Relf respectively.

Dr. Julian S. Huxley is executive secretary of the Preparatory Commission of the United Nations Educational, Scientific and Cultural Organisation.

Prof. J. D. Cockcroft is to be director of the new Research and Development Establishment at Harwell, Didcot.

The following University appointments have been announced: Dr. L. Aitcheson, professor of industrial metallurgy in the University of Birmingham; Dr. J. Allen, Jackson professor of engineering in the University of Aberdeen; Prof. E. Ashby, Harrison professor of botany in the University of Manchester; Mr. E. G. Bowen, Gregynog professor of geography and anthropology at the University College of Wales, Aberystwyth; Dr. F. C. Chalklin, professor of

Physics, Canterbury College, New Zealand; Prof. S. Chapman, Sedleian professor of natural philosophy in the University of Oxford; Dr. V. J. Chapman, professor of botany in Auckland University College, New Zealand; Prof. V. Gordon Childe, professor of prehistoric European archæology in the University of London; Dr. L. Hunter, professor of chemistry at University College, Leicester; Dr. R. J. W. LeFèvre, professor of chemistry in the University of Sydney; Dr. J. S. Mitchell, professor of radiotherapeutics in the University of Cambridge; Mr. E. F. Nash, professor of agricultural economics at the University College of Wales, Aberystwyth; Prof. M. H. L. Pryce, Wykeham professor of theoretical physics in the University of Oxford; Prof. Wilson Smith, professor of bacteriology at University College Hospital Medical School; Dr. C. M. White, professor of fluid mechanics and hydraulic engineering at the Imperial College of Science and Technology; Prof. V. C. Wynne-Edwards, regius professor of natural history in the University of Aberdeen; Dr. J. F. T. Young, professor of physics in the University of Manitoba.

We have noted with great regret the announcements of the death of the following scientific workers: Prof. T. Barbour, professor of zoology, Harvard University; Prof. H. Bateman, F.R.S., professor of mathematics, theoretical physics, and aeronautics, California Institute of Technology; Rai Bahadur Sir Upendranath Brahmachari, professor of tropical medicine, Carmichael Medical College, Calcutta; Dr. J. Brittain, director of research on explosive projectiles, Royal Arsenal, Woolwich; Dr. F. Bullock, secretary and registrar of the Royal College of Veterinary Surgeons; Prof. E. G. Coker, F.R.S., emeritus professor of civil and mechanical engineering, University of London; Mr. Frank Crowther, chief plant physiologist, Sudan Government; Prof. A. E. Dixon, emeritus professor of chemistry in the National University of Ireland; Prof. J. V. Dubský, professor of analytical chemistry in the Masaryk University of Brno; Major S. S. Flower, O.B.E., formerly director of the Zoological Gardens, Giza; Prof. J. E. S. Frazer, emeritus professor of anatomy in the University of London; Prof. J. Stanley Gardiner, F.R.S., emeritus professor of zoology, University of Cambridge; Prof. E. S. Goodrich, F.R.S., emeritus professor of zoology, University of Oxford; Prof. A. W. Grabau, professor of palæontology in the National University of Peking; Lieut.-Col. S. P. James, C.M.G., F.R.S., past-president of the Royal Society of Tropical Medicine and Hygiene; the Rt. Hon. Lord Keynes, C.B., economist; Dr. F. W. Lanchester, F.R.S., consulting engineer;

Prof. G. N. Lewis, For. Mem. R.S., professor of chemistry in the University of California; Mr. C. M. Lloyd, economist; Mr. A. H. D. Markwick, soil engineer; Dr. F. R. C. Reed, geologist, Cambridge; Mr. R. C. Shannon, entomologist; Sir Harold Stiles, emeritus regius professor of clinical surgery in the University of Edinburgh; Prof. R. Stockman, emeritus professor of materia medica and therapeutics in the University of Glasgow; Mr. H. Wright, metallurgist.

Two important conferences between delegations from scientific societies and associations throughout the Empire are being held during the period June 17–July 20. First is a conference arranged by the Royal Society, which opens in London on June 17, moves to Cambridge on June 22, goes to Oxford on June 29 and concludes in London on July 6. It is followed immediately by an Official Commonwealth Scientific Conference to be held in London from July 9 to July 20. A Review Conference, arranged by the Imperial Agricultural Bureaux and postponed from 1941, will also be held during the period July 9–July 27. The Royal Society Conference, which is to be opened by His Majesty the King, is intended to provide an opportunity for exchange of views upon scientific problems of immediate practical importance to various parts of the Empire and to discuss means of achieving the greatest practicable measure of collaboration between scientists in various parts of the Commonwealth in the solution of these problems. The Official Conference which follows will consider the detailed measures to be taken to organise collaboration in research of direct interest to the governments concerned.

The Conferences owe their origin to the recommendations of an informal committee appointed by the Royal Society in 1941 to consider post-war collaboration within the Commonwealth and will consider, as part of their agenda, the possibility of setting up a permanent office in London on the lines of the British Central Scientific Office set up as a war measure at Washington in 1941 for the interchange of scientific information between the United Kingdom and the United States of America. Both Conferences are primarily intended for the discussion of administrative problems, but during the evenings the delegates to the Royal Society Conferences will be free for informal discussions of purely scientific problems. Collaboration in scientific matters is no less important for the future well-being of the members of the Commonwealth than collaboration in economies, defence and foreign policies. The delegates are assured of a sincere welcome and equally sincere wishes for a satisfactory outcome of their deliberations.

The Science Masters' Association held its Forty-third Annual Meeting at the City of London School from April 15 to April 18, over 500 members being present.

In his Presidential Address, Prof. E. N. da C. Andrade, F.R.S., gave a stimulating account of the " Rise of Modern Science," and concluded with a plea for the teaching of more History of Science in the schools. Prof. H. S. W. Massey, F.R.S., lectured on Atomic Energy, and the Bishop of Truro on " The Scientist and Worship." Dr. C. L. Wilson urged the introduction of small-scale methods into the School Practical Chemistry Courses, and Dr. A. E. Alexander explained how the techniques of " Surface Chemistry " have been applied to various chemical, physical and biological problems. Dr. C. H. Waddington discussed the aspects of Biology which might be considered as fundamental, Dr. George Salt dealt with the importance of field work in this branch of Science, and Mr. F. J. Anscombe showed how statistics might be introduced into the School Science Curriculum. Mr. Ritchie Calder took as his subject " Science and the Press."

Mr. H. E. Dance, H.M.I., demonstrated the effectiveness and economy of the Film Strip as a teaching aid, and Mr. Richard Palmer discussed the place of Broadcast Talks in Science teaching, with recorded extracts from past talks to illustrate his points. A discussion on " The Effects of Examinations upon Science Teaching " was opened by Mr. J. L. Brereton and Mr. J. A. Lauwerys.

At a Joint Meeting with the Illuminating Engineering Society, Mr. W. R. Stevens and Mr. S. S. Beggs gave a demonstration lecture " Light and Your Eye," which was remarkable for the range and ingenuity of its experiments. Co-operation between the two societies in the future may result in demonstrations of this type being available for use in schools.

The extensive exhibition of apparatus and experiments devised by Members of the Association was of great interest, and a display of School Note-books and home-made Wall Charts was a welcome innovation. Scientific Instrument Manufacturers and Educational Publishers co-operated in making the Meeting a valuable and stimulating experience.

Iodine Facts for April 1946 (pp. 95 + 1 map. Iodine Educational Bureau, Stone House, Bishopsgate, London, E.C.2) is entirely devoted to a discussion of goitre in all its aspects. As is well known simple goitre is mainly an iodine deficiency disease. The thyroid exercises its physiological effects through thyroxine, formed in the gland from iodine taken in from the blood-stream and released, possibly in a changed form, into the blood as required. In the absence of an adequate supply of iodine the epithelial cells lining the follicles of the gland expand and, with continued shortage, a hyperplastic process occurs. The cells multiply and may ultimately fill the follicular spaces, converting the whole gland into a mass of functionless cellular tissue and causing myxœdema. The quantity of iodine in the whole human gland is very small—only about 10 mgm. and hyperplasia occurs when the weight falls below about half this value. There is evidence, however, that iodine deficiency is not the only cause of goitre, e.g. arsenic, fluorine, calcium and an unknown substance contained in cabbage act as goitrogenic agents when present in abnormal relationship to other constituents of diet. The obvious remedy for incipient goitre is the use of iodised salt, and statistics show that the addition of small quantities of potassium iodide to common salt can cause a remarkable diminution in the incidence of the disease. Use of iodised salt commenced in the Canton Vaud, Switzerland, in 1924, when 77 per cent. of the population was affected; by 1937 this percentage had been reduced to 21. Over the same period the corresponding figures for the population of Lausanne were 55 per cent. and 1 per cent. Equally good results have been obtained in the U.S.A., for example, in Michigan State, where 38·6 per cent. of the school children showed signs of goitre in 1924 and only 2·9 per cent. in 1936. The intakes of iodine recommended in various countries differ a good deal but are, in all cases, very minute. In the U.S.A., which has adopted the largest intake, it is only 0·182 gm. per year; in Switzerland it is 0·027 gm., and in Poland 0·013 gm. It is estimated that in England and Wales alone there are some 500,000 cases of thyroid enlargement in persons of ages 2–20 inclusive, and in view of these figures the Goitre Sub-Committee of the Medical Research Council of Great Britain recommended that a trace of iodine should be added to all the common salt consumed in the British Isles. So far no effect has been given to the recommendation.

The *Journal of Scientific Instruments* for April 1946 contains an interesting paper by K. Hoselitz, of the Permanent Magnet Association, Sheffield, descriptive of old and new alloys suitable for permanent magnets and of consequential changes in manufacture and design. It is desirable to design a permanent magnet so that the total energy of the field in the gap is a maximum, and it may be shown that, ignoring leakage, this energy is proportional to $B_mH_mV_m$ where B_m and H_m are the induction and field in the magnet and V_m the volume of the magnetised material. Thus the working point should be such as to make B_mH_m, a maximum, and the alloy used should give as high a value for this product as possible. The older alloys—chrome and tungsten steel—gave values of the product in the neighbourhood of $2{\cdot}9 \times 10^5$ with coercivity (H_c) about 70. The values for cobalt steel increase with the cobalt content and, for the percentage composition C = 0·9, Cr 3–6, W 5–6, Co 35, the product is $9{\cdot}5 \times 10^5$ with $H_c = 250$. In 1931 Mishima discovered the properties of the iron nickel aluminium alloys, and of these alnico (Al 10, Ni 17, Co 12, Cu 6) gives B_mH_m 17×10^5 and H_c 500. Later, in 1938, Oliver and Shedden (*J. Sci. Inst.*, **15**, p. 193) showed that, by allowing these alloys to cool in a magnetic field, the value of B_mH_m is much increased in the direction of the field and much diminished in perpendicular directions. Alnico V (Al 8, Cr 14, W 24, Mo 3) gives $H_c = 525$ and $B_mH_m = 40 \times 10^5$ along the line of the field, but only 300 and 4×10^5 perpendicular to it; while Alcomax II, which differs from Alnico V only in containing 11 per cent. Cr and 6 per cent. Mo gives $H_c = 470$ and $B_mH_m = 43 \times 10^5$ along the field. In passing it may be remarked an alloy of 23 per cent. Co and 77 per cent. Pt gives $H_c = 3650$ and B_mH_m 46×10^5. The new materials make it possible to use a much smaller length/cross-section ratio than was previously possible. Horse shoe magnets are made with a short bar of the magnetised alloy at the crown of the U and with limbs and pole-pieces of soft iron. Why the iron nickel aluminium alloys should have such a high coercivity is not fully understood; but it is certainly dependent on conditions of high internal strain in the lattice structure. The alloys are very difficult to work and desired shapes must be produced by casting, or for small and intricate designs, from sintered powders. By this latter means bar magnets only 0·08 in. long and 0·01 sq. in. cross-section have been made.

The *Bell System Technical Journal*, July–Oct. 1945, contains a long and impressive paper by R. O. Mindlin on the *Dynamics of Package Cushioning*. It occupies 100 pages of the *Journal* with over 4 pages devoted to definitions of the symbols employed. The investigation, which is largely mathematical, was intended primarily to determine the design of packages for large vacuum tubes, but the formulæ obtained, and the curves plotted from them, apply to any form of mounting intended to protect equipment from the effects of sudden change of velocity. A simple example of the kind of problem considered is given in the first part of the paper: "To define the tension springs for a 21 lb. article to be packaged so as not to exceed 35 g in a 5-foot drop." The treatment covers packaging systems with linear, cubic, tangential, bi-linear, hyperbolic and anomalous elasticity.

The same number of the *Journal* contains an account of a lecture by J. R. Pierce, dealing with *Physical Limitations in Electron Ballistics*. Limitations to the production and use of electron beams, in which both small and large currents are required, are considered. Electron microscopes use beams of the low current type, and the resolving power obtainable is limited by unavoidable aberrations produced by the electrostatic or magnetic lenses. The least distance between two points which can be resolved by a microscope is directly proportional to the wave-length of the radiation and inversely as the numerical aperture. The wave-length of 30,000-volt electrons is about $1{\cdot}2 \times 10^{-5}$ times that of sodium light, but the numerical aperture cannot be made greater than 0·01 while that of an ordinary microscope with oil immersion is about 1·4. Thus aberrations limit the smallest detail resolvable by the electron microscope to 1/1000th of that of a microscope using visible radiation. In the cathode ray tube it is necessary to concentrate a large current electron beam on as small area as possible on a receiving screen. This area is enlarged by the thermal velocities of the electrons leaving the heated filament since these velocities have components perpendicular to the direction of the beam. Further limitations are imposed by the space charges and the difficulty of dissipating the energy of the electrons when they are suddenly stopped.

The *Bell Laboratories Record* maintains the high standard for which it was distinguished before the war. Selecting two or three from the many interesting articles which have appeared this year we may note a description of "Duplex" quartz crystals in the February number. These are small quartz bars which can be made to vibrate in the fundamental transverse mode with two nodes each 0·224 of the length of the bar from the nearer end. This type of vibration cannot be excited electrically in a single bar, but by bonding two thin rectangular plates together, one on top of the other, with a thin metallic interfacial layer and applying an alternating potential difference to the outer faces it is possible to make one plate contract while the other expands, thus producing the desired alternating curvatures. The device is used to give relatively low frequencies, *e.g.* from 1 to 10 kc. per sec. The March issue includes an interesting paper by R. M. Bozorth, describing the effects of stress on the magnetic properties of iron, nickel and 68 permalloy (*i.e.* an alloy containing 68 per cent. nickel and 32 per cent. iron).

Stress, in particular tension, affects the alignment of the small saturated magnetic domains and so has a marked effect on the shape of the hysteresis curve. Permalloy, for example, becomes saturated at much lower fields ; for nickel higher fields are required ; for iron the effects are rather complex. With increasing tension and constant field strength the induction of 68 permalloy increases till the elastic limit of the material is reached and then diminishes almost to zero. The effect is not small, for in the example given with $H = 0{\cdot}1$ œrsted and zero stress B was about 2000 gauss ; increasing the stress to 10 kg. wt. per sq. mm. caused B to increase to 12,800 gauss (*i.e.* almost the saturation value), while on doubling this stress B fell sensibly to its initial value The April number of the *Record* contains photographs and a short description of the "Bat," a radar-controlled glided bomb carrying half a ton of expolsive, which is guided to a target by radio waves emitted by the glider and reflected back to it by the target. Many thousands of such bombs were manufactured, and it was used with success against shipping in the Japanese war.

Dr. Alexander Wood has written a very pleasant and informative little book describing the genesis and growth of the Cavendish Laboratory (Cambridge University Press, pp. 58 + 8 photographs, 1946. Price 2*s.* 6*d.*), which should be much appreciated both by those having a personal interest in the Laboratory and those interested in the human side of the development of modern physics. It is strange, however, that in his account of the origin of the Laboratory Dr. Wood should mention Clifton's Laboratory at Oxford and ignore the Physical Laboratory at University College, London, where routine teaching in practical physics was provided for the first time under the direction of Carey Foster in the session 1866–67 five years before the Cavendish professorship was founded and eight years before the Cavendish Laboratory was available for partial occupation. There is an authoritative account of the development of the Laboratory during the period in which J. J. Thomson and Rutherford brought it world-wide fame, and the story closes with the building of the extensions made possible by the gift of £250,000 from the late Lord Austin. These, which include a High Tension Laboratory and a four-storied Research Wing, were occupied by the Government during the war, but are now available for their proper use.

Arthur Stanley Eddington Memorial Lectureship

Arthur Stanley Eddington was one of the few recent scientific thinkers of the first rank who have attempted to think out a view of the world which would do justice to all fields of knowledge and experience. Of world-wide fame as an astronomer and mathematician, he combined great powers of scientific and philosophical analysis with a deep religious faith and a practical mysticism which found expression in his active and life-long membership of the Society of Friends, and in his intense concern for the ways of peace. These qualities, together with his humility and simplicity, made it possible for him to understand the needs and perplexities of ordinary thinking men. And the simple and vivid style in which he lectured and wrote enabled him to communicate to non-specialists an appreciation of the essence of scientific thought as well as of some of the most recondite advances in physics and astronomy. What makes Eddington such an important figure in contemporary thought is not that the synoptic view of the world which he put forward is necessarily the one which will ultimately be found to be nearest the truth, but that he saw that a synoptic view was necessary, and that we cannot be content with a departmentalisation of knowledge in which science appears to be disconnected from the other fields of human enquiry—philosophical and religious.

We desire that there should be for Eddington a fitting memorial—one that would maintain and further his own concern for relating the scientific, the philosophical and the religious methods of seeking truth, and which would help to develop that insight into the unity underlying these different methods which was the characteristic aim of his work. We believe that the foundation of an Arthur Stanley Eddington Memorial Lectureship, providing for periodical lectures on some aspect of contemporary scientific thought considered in its bearings on the philosophy of religion or on ethics, to be delivered in Cambridge or elsewhere and to be published, would be well calculated to achieve this object.

To establish this memorial lectureship will require a capital sum of about £2,500. An Appeal Committee has been formed in Cambridge to receive subscriptions, which may be sent to Barclays Bank Limited, Cambridge, for the credit of "The Arthur Stanley Eddington Memorial Lectureship" account, or to the Hon. Secretary, Dr. W. H. Thorpe, Jesus College, Cambridge, or to the Hon. Treasurer, Howard Diamond, A.C.A., 45, St. Barnabas Road, Cambridge. The Hon. Secretary will be pleased to send to those interested details of the plan proposed for administration of the Lectureship for which the Royal Society, Trinity College, Cambridge, and the Society of Friends have agreed to appoint Trustees.

We warmly commend this appeal.

Joseph Barcroft
E. W. Birmingham
Max Born
R. B. Braithwaite
W. Maude Brayshaw
G. A. Chase
Henry Dale
Howard Diamond
Herbert Dingle
Winifred Eddington

Wilson Harris
G. B. Jeffery
Martin Johnson
Rufus M. Jones
Harold Spencer Jones
Kathleen Lonsdale
E. J. Maskell
A. Victor Murray
C. E. Raven
F. J. M. Stratton

Hilda Sturge
W. H. Thorpe
Henry T. Tizard
G. M. Trevelyan
Elsie Watchorn
A. N. Whitehead
Edmund T. Whittaker
Alex. Wood
H. G. Wood

ESSAY REVIEW

THE IMPACT OF CHEMISTRY ON CIVILISATION. By V. W. Slater, B.Sc., F.R.I.C., M.I.Chem.E. Being a Review of **What Industry owes to Chemical Science.** Third edition. Issued with the Authority of the Council of the Royal Institute of Chemistry. [Pp. viii + 372, with 13 plates.] (Cambridge : W. Heffer & Sons, Ltd., 1945. 18*s.* net.)

It took a world war to make British industry, both chemical and non-chemical, realise the value of chemical science ; a series of articles on *What Industry owes to Chemical Science*, by Richard B. Pilcher and Frank Butler-Jones in *The Engineer* from December 1916 to July 1917, gave an insight to the general public into the contribution which chemists were then making in all branches of industry. These articles were published in book form in 1918 with a minor revision in 1923. The second world war has given more emphasis to physics, but both between the wars and during the second chemistry has continued to play an important part in the adaptation of industry to the growing needs of modern civilisation. Industries which were more or less stationary for centuries have changed rapidly and new processes have become outdated almost before they were in full production. The advance has been great beyond all imagining and broad indications of this have already been given in Stephen Miall's *History of British Chemical Industry*, 1931, and G. T. Morgan and D. D. Pratt's *British Chemical Industry*, 1938. For some years there has been a demand for a third edition of *What Industry owes to Chemical Science*, this time by over fifty contributors and issued by authority of the Royal Institute of Chemistry. To call it a third edition is a necessary courtesy to the original authors who formulated the scheme ; the book is substantially a new work.

The chemist is no longer confined to the chemical industry nor to his laboratory. This is well illustrated throughout the book by references to his work in the many aspects of a firm's activities : a watchful eye is kept on fuel consumption, water supply and effluent disposal, on materials of construction and on the welfare of the worker. Chemical engineering and biochemistry are other branches of his work. Then again the contribution of science is not only utilitarian and economic, but may be æsthetic: the cosmetic industry " owes its position to the artistic utilisation of the discoveries of chemical science."

The first impression on looking at the contents page was that the book was a mere catalogue—a heterogeneous collection of such an array of subjects presented by so many writers to a non-scientific public as to suggest an unsatisfying list of unadorned achievement. Such, however, is not the case as the reader will rapidly discover in the first few chapters, and, in fact, one's interest is held throughout the work by the story of the contribution of chemistry to industry—to all phases of human activity—so interwoven into our lives that we begin to wonder how any civilisation could have existed without it. In the chemical industry itself " it is not too much to say that the maintenance of the standard of daily life is largely dependent on adequate supply of such substances " (technical chemicals).

The study and practice of chemistry has enabled man to make better use of chemical substances provided by Nature, and to augment the production of, or reproduce artificially, such substances to meet a need greater than natural supply. Thus many drugs such as quinine, morphine, digitalin, caffeine, atropine, cocaine, have been isolated from former herbal remedies. Plants and animals provide a variety of products for which industry can find greater use after their refinement by chemical processes: *e.g.* oils, fats and waxes used for soap manufacture, for paints and for edible purposes, and cellulose from wood as the starting-point for a range of products from paper to lacquers. Coming to inorganic chemicals, to give typical examples only, crude barytes is bleached to a fine white pigment, sodium nitrate is separated from the crude caliche of Chile and pure oxygen is obtained from the atmosphere. Chemical science has recently introduced a process to obtain drinking water from sea water by the use of barium and silver zeolites, successfully used by airmen forced down over the sea during the war. The limited supply of natural silk has been met by the self-contained chemical industry for the production of rayon. Vitamins have been isolated, identified and synthesised. Penicillin is a notable recent example of the discovery of a specific use for a natural product and the chemist's success in establishing its large-scale production. Adrenaline, Nature's drug for raising the blood pressure and first isolated from the adrenal glands, has been synthesised from catechol, derived from benzene.

It is probably in the synthesis of substances which do not occur in Nature that the greatest or most spectacular advance has been made. Man has outdistanced Nature in the range of sulphonamides for specific bactericidal use, and in the "cocaines" each with its specific application in local anæsthesia. Stainless steels are other examples in an entirely different sphere and to this may be added the whole range of plastics and synthetic resins. There is hardly an industry which is not now dependent more or less on chemical science: materials for roads and building, laundering, treatment of timber, textiles, metals and alloys, paints—the contribution to all these is evidenced by the Research Associations supported by the industries concerned and by the Government. Such is the heterogeneous background from which the contributors have been selected to present a coherent story of the impact of chemistry on modern civilisation.

The opening chapters of the book show how food is produced with increased yield, how it is preserved, transported and stored, how its nutrition value is augmented. Thus we read about fertilisers and feeding-stuffs, water, bread, milk, edible oils and fats, sugar and beverages, together with canning and cold storage. The story of the fixation of nitrogen in a later chapter (Heavy Chemicals) is of special interest at this period of world food shortage. We are reminded that "in 1898 Sir William Crookes, in a notable address to the British Association, brought home to the scientific world the fact that unless some method was devised of fixing nitrogen from the air into a plant assimilable form or there was a halt in the rate of population increase, food shortage and even starvation were in front of mankind as a whole." The first commercial attempt as early as 1842, in France, converted potassium carbonate to the cyanide. Then came the cyanamide process leading to ammonia. We read of the electric discharge method for nitric acid and finally the Haber-Bosch process for the direct production of ammonia from nitrogen and hydrogen at high pressure in the presence of a catalyst. The sections on drugs, disinfectants and antiseptics show how pain, discomfort and death have been retarded by chemical science. Clothing and shelter have been provided for a wider public by advances in chemical knowledge described in the sections on textiles, leather, cement and paints. It is also interesting to read how much paper, printing inks, pencils and photography as means of communication, duplication and recording owe to the science. The final chapter, "Transport," gives a picture of the chemist testing fuels, countering corrosion in ships, preventing fires, supplying anti-freeze for car radiators and generally improving the materials of construction for all means of transport by land or sea; reference to aircraft is given elsewhere in the book.

With so many writers the standard naturally varies from chapter to chapter. The supervision by the Publications Committee of the Royal Institute of Chemistry has ensured that no section is unduly laboured and a large amount of material has been compactly arranged into a relatively small space. There is a little duplication, but not enough to matter. The history of the dyestuffs industry with the work of Hofmann, W. H. Perkin and J. P. Griess is given under "Dyestuffs" and under "Bleaching, Dyeing, Printing and Finishing." The story of the discovery in 1820 of quinine by Pelletier and Caventou with its subsequent allied developments, and of the introduction of organic arsenical compounds by Ehrlich in the section on "Drugs," are repeated under "Technical and other Chemicals." The section on textiles deals mainly with the application of research to industry and might have served as a general introduction to the whole book.

Covering such a wide field it is to be expected that some false ideas will be repeated. This is presumably where the writers have had to rely upon literature references instead of having personal experience. It is also evidence of the danger of using patent specifications as scientific documents. To give specific examples; hydrogen peroxide is not prepared by industry from barium peroxide and carbon dioxide—such a process was patented by Askenasy in 1928 but was never developed on a large scale; nor could persulphates, which can cause dermatitis, be used for "toilet purposes."

A fair proportion of each section is devoted to historical development, extremely useful as showing how industry has reached its present position and how it may be further influenced by chemistry in the future. Useful bibliographies are given at the end of each chapter, but it is a pity that these are not dated as, in some cases, they are old and have been superseded by more recent work. The illustrations are portraits of thirteen men of renown, each of whom has been a pioneer in a particular field laying the foundation of a new branch of chemical or allied industry. They are aptly chosen and sometimes include a facsimile signature.

The book does not cater for the specialist in any particular industry, but rather gives him an insight into other branches, just as it gives the intelligent general reader a reliable and interesting survey of all branches. This is especially valuable now that scientific knowledge has become so complex that individuals are more and more compelled to work in restricted fields. This book is remarkably free from the failing of other works of its kind in its general reliability with regard to actual practice and it can be recommended to a wide public.

REVIEWS

MATHEMATICS

Waveform Analysis. A guide to the interpretation of periodic waves including vibration records. By R. G. Manley, B.Sc. [Pp. xii + 275, with 103 figures, including about 150 waveforms, and 39 tables.] (London: Chapman & Hall, Ltd., 1945. 21*s*. net.)

This book is designed to give practical and theoretical aid to those who have to analyse large numbers of waves containing a small number of harmonics. Three main methods are discussed.

The Envelope Method.—This was developed by the author for the analysis of waves containing two or three harmonics. Smooth curves are drawn through the peaks and troughs, to find a low-frequency surge or remove a high-frequency ripple; beats are analysed by examining the distribution of the peaks and variations in amplitude of the oscillations, and so on. The theory of the method is carefully worked out, and its power within its limited scope is amply demonstrated by a number of interesting examples.

The Method of Superposition.—This involves the elimination of certain of the harmonics by adding or subtracting different parts of the curve in order to find the remaining harmonics, and can be carried out graphically or numerically.

The Numerical Method of Harmonic Analysis.—This consists of fitting a trigonometrical polynomial through a finite number of points of the curve or, what amounts to the same thing, approximating to the Fourier coefficients by mean values of the type

$$a_k(\mathrm{N}) = \frac{2}{\mathrm{N}} \sum_{s=1}^{\mathrm{N}} f\left(\frac{s\pi}{\mathrm{N}}\right) \begin{matrix}\cos\\ \sin\end{matrix} \frac{ks\pi}{\mathrm{N}} .$$

The author begins with a thorough discussion of the synthesis of curves composed of two or three harmonics. There is a chapter on Fourier series, a brief account of the Lissajou figures, and some practical hints on mechanical processes.

The reviewer has only a few minor criticisms to make.

On p. 56 the fact that the sum of two unsymmetric functions may be symmetric is overlooked. The distinction should rather be between symmetric and skew-symmetric functions.

On p. 175 the author appears to say that the Hurwitz-Liapounoff (or Parseval) theorem breaks down if the function is discontinuous, whereas in fact it holds so long as the square of the function is integrable. The explanation is that he first considers mean square convergence, where

$$\lim \int \{f(x) - \mathrm{S}_n\}^2 \, dx = 0,$$

and later essentially uniform convergence. The former property holds even when the Gibbs phenomenon is present.

On pp. 253–5 there is a sketch proof of Poisson's theorem that

$$\lim_{r \to 1-0} \sum A_n(x)\, r^n = \tfrac{1}{2}\{f(x+0) + f(x-0)\}$$

whenever the right-hand side exists. This does not in itself prove Fourier's theorem since the convergence of $\Sigma A_n(x)$ is not established.

The reviewer would have liked to see a complete proof of some version of Fourier's theorem. For example, if the curve is sufficiently smooth (apart from discontinuities) for Dirichlet's formula for the partial sums to be integrated by parts it is easy to show that the Fourier series converges. A proof of this character would be sufficiently general for practical purposes and at the same time remove some of the mystery surrounding the theorem in elementary books.

The mathematics is presented with great care and attention to detail, and the book is written in an easy and readable style. The printing is excellent, the diagrams well drawn, and the reviewer found only one misprint.

L. S. B.

Advanced Mathematics for Technical Students. Part I. By A. GEARY, M.A., M.Sc., H. V. LOWRY, M.A., and H. A. HAYDEN, D.Sc. [Pp. viii + 419, with 262 figures.] (London: Longmans, Green & Co., 1945. 12*s.* 6*d.*)

THIS book, which is Vol. I of a two-volume course, covers the work corresponding roughly to the mathematics for Part I of the Engineering Degree Examination of London University, with the addition of some co-ordinate geometry and elementary differential equations. The book is also suitable for students preparing for Higher National Certificate in Engineering, but in many places includes work not usually covered by such students. Naturally some previous knowledge is assumed.

The book contains much that is excellent, the subject matter is attractively presented and the diagrams are neat and pleasing to the eye. Among the sections to be highly commended are the section on complex numbers, including the applications of complex numbers to electrical engineering, and the section on approximate integration which is one of the best that has been written on this subject.

The order of the chapters and the relative importance given to some sections call for criticism. The approximate solution of equations, one of the tools of the technician, should appear at an early stage and applications be made wherever possible throughout the book. Nearly 100 pages separate the applications of integration from integration itself. Curvature (including the length of curves) finds itself sandwiched between two chapters on co-ordinate geometry. The chapter on complex numbers appears too late. The co-ordinate geometry which occupies a large part of the book is confined almost entirely to the straight line and conic sections, much of it of a highly academic character, as instanced by the work on pole and polar. A more general treatment of the properties of curves would be more useful to the average engineer.

This book will be of considerable value to students working under guidance.

L. T.

PHYSICS

Experimental Electronics. By Ralph H. Müller, R. L. Garman and M. E. Droz. [Pp. xvi + 330, with 177 figures.] (London: George Allen & Unwin, Ltd., 1945. 21*s.* net.)

The time is long since past when the study of electronics was the concern only of the communication engineer and the physicist, and, to-day, electronic instruments are being increasingly used in almost every branch of science. During the war years, university instruction in electronics, in this country, has been dominated by the needs of the armed forces, and many universities are, at the present time, reorganising their courses in order to cover the much wider field of peace-time applications. The book under review, first published in the United States in 1942, is therefore particularly welcome, since it is written by three members of the Chemistry Department of New York University and is concerned exclusively with the non-communication uses of electronic devices.

After an introduction, which deals with the elements of circuit theory and gives some useful information on radio components, successive chapters are devoted to triodes, photoelectric cells, power supplies, multi-electrode valves, gaseous tubes, valve voltmeters, amplifiers, oscillators and cathode-ray tubes. The approach to the subject is essentially practical, and the book contains full descriptions of some seventy experiments which the reader may carry out for himself. It is safe to say that anyone, who works intelligently through these, will have gained a very useful knowledge of electronic instruments. The use of mathematics is avoided, but numerous footnotes and general references at the end of each chapter refer the reader to original articles and standard text-books where detailed mathematical treatments may be found. The subject matter is thoroughly up-to-date and the book can be unreservedly recommended to anyone interested in electronics.

C. W. O.

An Introduction to the Theory and Design of Electric Wave Filters. By F. Scowen, B.Sc., A.Inst.P. [Pp. xi + 164, with 60 figures.] (London: Chapman & Hall, Ltd., 1945. 15*s.* net.)

It is fortunate that Mr. Scowen has chosen to write a thoroughly practical elementary treatise on the subject of electric wave filters, for whilst the basic theory of this subject may readily be obtained from several sources, data relating to practical design are scarce.

However, it must not be thought that the author has neglected basic theory; in fact, his book contains three chapters written specifically to supply the requisite ground-work. These are headed Mathematical Introduction, Electrical Introduction and Introduction to the Ladder Network. They are followed by nine sections devoted to the classical theory of constant k and m-derived filters.

In the reviewer's opinion, however, the most important part of the book is contained in the last seven sections, which give practical design information including the use of templates and a valuable discussion on the properties of actual circuit elements. This section is particularly welcome because, although the classical theory of electric wave filters assumes the existence of ideal circuit elements, comparatively few writers discuss the problem of constructing such networks from circuit elements which are far from ideal.

In the last section there is a useful description of methods of measuring the characteristics of filters.

Finally, there is one criticism which should be made about the bibliography. Although the author makes a remark in his preface to the effect that too few articles are published describing the new work that has been done on electric wave filters, he omits any mention in the bibliography of the most important recent contributions, notably those of Cauer, Cocci and Piloty.

G. S. B.

Modern Petrol Engines: With Special Reference to Automobile, Aircraft and Stationary Types.

By Arthur W. Judge, A.R.C.S., D.I.C. [Pp. viii + 509, with 304 figures, including 8 plates.] (London: Chapman & Hall, Ltd., 1946. 36*s.* net.)

The petrol engine has been increasingly popular since its first introduction, and to those who remember it in the days of tube-ignition and automatic inlet-valves its development has been remarkable. Its cycle of operation has remained unaltered, but in reliability, fuel economy, and power-output, the modern petrol engine has reached such a high state of perfection that no major improvements are to be expected. The book under review shows that this is the result of scientific research into the complicated process of combustion, and to metallurgical research which has produced special steels and light alloys.

The title of the book gives no indication of the value of its contents; it is definitely not an elementary work. Theory and practice have been so successfully combined that the book will be of great value to engineering students of National Certificate Standard and to university students in mechanical engineering who may wish to specialise in internal-combustion engineering. Designers and draughtsmen of automobile and aircraft engines will appreciate the chapters on supercharging and the cooling of engines. These chapters contain a wealth of information that could only be compiled by an expert.

The introductory chapter deals concisely with the history of the internal combustion engine, and with automobile and aircraft engines following the introduction by Daimler in 1883 of the four-stroke petrol engine. Chapter 2 commences with an explanation of the combustion process, illustrated by the usual equations of chemical reactions that occur when combustion takes place. The remaining portion discusses the phenomena common to the combustion of petrol in a modern engine. The chapter on thermodynamics assumes a knowledge of mathematics and the theory of heat as applied to engine cycles. Having this knowledge, the reader will be better able to digest the information contained in the chapter on petrol-engine performance.

Carburation and petrol injection, the two-cycle engine, and engine types, ignition problems and effects are discussed at length, and are fully illustrated. An interesting description of the sleeve-valve engine, together with that of the latest design of aero-engine, is supplemented by performance curves for engines of the poppet and sleeve-valve type having the same piston displacement; these curves show the superiority of the sleeve-valve.

The efficient lubrication of engines is of vital importance, and builders and users have the choice of standard lubricants, blended to suit the different types of engines. These standard grades are marketed by firms who make comprehensive and continuous tests of their products both in their laboratories and on actual engines. In the chapter on "lubrication," the theory of lubrication is explained, together with some of the standard tests. Quantitative results to which certain oils should conform are given.

A short chapter is devoted to a lucid explanation of the basic principle of operation of the gas turbine and of jet propulsion, of which several systems are illustrated by line diagrams. The subject is of topical interest, and in view of its importance and the scarcity of published information regarding present practice, this chapter is of fundamental importance to an understanding of later development.

A valuable feature of the book is the inclusion of 153 references to books and original papers by authors whose work has contributed to the development of the modern engine. These references add to the value of the book, and the author is to be commended for his painstaking and skilful selection. All the illustrations are of ample size, the charts and tables are appropriate. The photographic reproductions are well selected and of unusual clarity. The index will be found to be efficient. J. O.

Surveying for Young Engineers. By S. WRIGHT PERROTT, M.A.I., M.Inst.C.E. [Pp. viii + 232, with frontispiece, 2 maps, and 95 figures, including 12 plates.] (London: Chapman & Hall, Ltd., 1946. 10*s*. 6*d*. net.)

THE book is an attractive little volume of an elementary or introductory character, written for the young and inexperienced surveyor. The author has endeavoured to describe field operations in such a manner as to enable the budding engineer to envisage these operations before tackling them himself.

The first nine short chapters are devoted to chaining, to the keeping of the field-book, and to the use of simple instruments such as the cross-staff and optical square. Six chapters are devoted to levelling and contouring. They include notes on the adjustments of the dumpy level, and the keeping of the two usual forms of level book. Five chapters deal with the measurement of angles, with descriptions of the Abney level, prismatic compass and transit theodolite. The idea of co-ordinates is explained, but no attempt is made to deal with methods of "balancing" closed traverses, as those would be beyond the scope of the book.

The methods of setting out circular curves by means of the chain and tape, or by means of deflection angles, are explained, and some of the more common formulæ are derived. Some space also is devoted to the procedure adopted in the field for the setting-out of cuttings and embankments slope stakes, foundations of culverts, small bridges, etc.

A chapter is devoted to map-reading, but though the author's intentions are good, the reviewer doubts if many readers will take the trouble to follow the advice given to experiment with maps and photographs. In fact, with Map No. 2 and the photographs in the book being printed on different pages and being differently oriented, the advice is not easy to follow.

Though necessarily in conformity with the authorised economy standards, the book is excellently produced with "flexiback" binding. The print is clear, and the volume contains twelve plates, mostly of surveying instruments and including two photographs of the Llangollen district, in addition to two maps and other illustrations in the text.

W. N. T.

METEOROLOGY

Oceanography for Meteorologists. By H. U. Sverdrup. [Pp. xv + 246, with 70 figures and 4 charts]. (London : George Allen & Unwin, Ltd., 1945. 12*s.* 6*d.* net.)

This is a delightful book to be recommended not only to the meteorologists to whom it is directly addressed but to all physicists interested in the large-scale application of physical laws, in this case to the oceans and their interplay with the atmosphere above. Such a book could have come from few people and none more appropriate than Sverdrup who is now professor of oceanography in the University of California, but who has for long been in close touch with meteorology and has made important contributions to the latter subject.

The plan of the book is shortly as follows. An introductory chapter deals with the heat budget of the atmosphere and shows that the motions of the atmosphere, which are a necessary consequence of the observed thermal equilibrium of the atmosphere, are determined in quite an important way by the processes of heat and water-vapour exchange proceeding at the underlying water surface. Oceanography proper is then introduced by a thorough consideration of the physical properties of sea water and ice followed by a short description of oceanographic observations. Physical oceanography is next discussed in a number of chapters under such headings as—radiation ; exchange of heat and water vapour between the atmosphere and the sea ; salinity, temperature and density ; ocean currents resulting either from mass distribution or wind ; and water masses. Finally, the author gathers together a number of strands in a chapter on the interaction between the atmosphere and the oceans, a chapter in which more questions are posed than are capable of answer at present, and which will remain as a challenge as well as an aid to the meteorologist anxious to develop his subject.

The chapter which to the reviewer singled itself out as the most significant to the meteorologist is that dealing with heat and water-vapour exchange between the atmosphere and the sea. Energy exchange is involved in both processes with the evaporative exchange normally far the greater (about 90 per cent. of the total). The latter flow of energy to the atmosphere is later made effective when the water vapour condenses as cloud and rain with the liberation of its latent heat, and this indirect reaction of the oceans upon the atmosphere (and vice versa, *e.g.* the effect of cloud on the radiation inflow to the sea) is one of many examples which might be given to show that the inter-relations of oceanography and meteorology are far from simple. Occasionally there appears to be a more direct and impressive relation, as for example in the occasional southward extension of the El Niño current off the coast of Peru in February and March, turning a normally dry season in Peru (average rainfall 4 mm. per month) into one of torrential and erosive rain with falls up to 400 mm. per month ! Although such a violent change in the atmospheric circulation can rarely be directly traced to oceanic influence, there is a distinct possibility that a more intensive physical study of oceanic and meteorological anomalies will ultimately provide the meteorologist with valuable pointers to variations in seasonal weather characteristics.

P. A. S.

CHEMISTRY

Modern Chemistry: Some Sketches of its Historical Development. By A. J. Berry, M.A. [Pp. x + 240]. (Cambridge: at the University Press, 1946. 10*s*. 6*d*. net.)

"THE study of the historical evolution of scientific theories is essential to a proper understanding of their significance and import." On this sound principle, Mr. Berry, whilst disavowing any claim to have written a history of Chemistry, gives us a series of essays in which he has attempted "to focus attention on the development of some of the newer branches of chemical science." His book consists of nine chapters with the titles: Classical Atomic Theory; Electrochemistry; Stereochemistry; Radioactivity; Elements, Isotopes and Atomic Numbers; Some Experimental Studies on Gases; Some Problems of Solutions; Some Essential Features of Chemical Change; A Retrospect.

The treatment is discursive and not intended to be exhaustive; but, not less for these reasons, the author has succeeded in making his essays pleasantly easy to read. Moreover, he often brings out some interesting but less-known aspect of his subject. Very wisely he always emphasises the achievement of the former generations of chemists. The student, almost inevitably engrossed in *recent advances*, easily forgets this and becomes more aware of their errors; it is valuable to remind him—and the rest of us—how excellent their experimental work was in its context, and how their theoretical views, partially erroneous though they may now seem, constitute the evolutionary material for current theories, which in their turn are certain to need revision later on.

There are some mistakes or near-mistakes that should be put right in a second edition. They are mainly trivial, such as the appearance of the van 't Hoff isochore with an unconventional sign (pp. 161, 198), or of the Gibbs-Helmholtz equation in a dimensionally questionable form (p. 49), or the implication that 1·42 A. is the typical aromatic C-C distance (p. 91). Rather more serious ones occur on pp. 113–14, where, for instance, the neutron is described as an "uncharged hydrogen atom," and where the reader may be misled as to the identity of the "very penetrating radiation." The passage about esterification and hydrolysis (p. 206) appears confusing.

Modern Chemistry can be strongly recommended: to students in particular, as supplying a general background to the usual Chemistry degree course; and also to the general scientific reader as an interesting and scholarly piece of writing. The book is well produced by austerity standards, has adequate indexes, and represents good value for money.

J. C. SPEAKMAN.

An Introduction to X-Ray Metallography. By A. TAYLOR, M.Sc., Ph.D., F.Inst.P., with a foreword by SIR LAWRENCE BRAGG, O.B.E., F.R.S. [Pp. xi + 400, with 240 figures, including 45 plates.] (London: Chapman & Hall, Ltd., 1945. 36*s*. net.)

LAUE's discovery in 1912 of the diffraction of X-rays by crystals provided a tool of unprecedented power for the examination of the fine structure of matter. The development of the science of X-ray analysis which followed has had its effect not only on nearly every branch of pure science but on many applied sciences as well. Nowhere have these advances been more important than in their applications to metallurgical problems. Soon after the development of the Debye-Scherrer powder method of analysis, the early workers in this field, including Westgren, Owen, Bradley and Polanyi began to make striking discoveries of immediate importance to metallurgists. Nowadays the X-ray method takes its place beside the standard thermal and microscopic methods of the science. But a new development of this kind always creates one special difficulty. The physicist or the chemist who is skilled in X-ray analysis may know but little of the intricate details behind the metallurgical problem. Or the experienced metallurgist who wishes to use the X-ray method may not be well equipped in the fundamentals of that subject. The chief value and importance of Dr. Taylor's book lies in the way in which it brings these two sciences together.

He begins with a number of rather brief chapters on the fundamentals of X-ray crystallography—generating apparatus, the space lattice and its diffraction effects, experimental methods, and a discussion of the structure factor and intensities. I found this part of the book a little disappointing. It is, however, perhaps inevitable that the phrase "it can be shown" must occur frequently in any brief treatment of such a subject. And in fairness it must be remembered that the fundamental simplicity of most metal structures makes any elaborate or comprehensive discussion of the principles of X-ray analysis rather unnecessary. These chapters probably supply all that is necessary.

After a short account of the crystal structures of the metals we come to the main body of the work. A long and very interesting chapter on the study of thermal equilibrium diagrams by X-ray methods covers a great range of work, much of it recent development which has hitherto only been described in original scientific papers. These topics include the determination of phase boundaries, defect lattices, electron compounds and the Hume-Rothery rule, the concept of Brillouin zones, superlattices, and the order-disorder transformation theory. Some of these subjects are necessarily dealt with in outline only, but the exposition is remarkably lucid. Ternary systems and recent X-ray work on the iron-carbon system conclude the chapter. Illustrative examples are numerous, many of them based on the original work of the author and Dr. Bradley.

The two following chapters deal very fully with the measurement of grain size by optical and X-ray methods, and the study of grain orientation, including glide phenomena, fibre structure and age-hardening. There follows a brief chapter on the application of X-ray methods to the study of refractory materials, and finally another brief chapter on radiography, including notes on the use of gamma-rays and microradiography. The book concludes with a very extensive and useful set of indexes, appendices and tables, occupying 95 pages.

The book is well planned and arranged, with apparently very few mistakes (ϕ is misplaced in Fig. 31*a*; the temperature is not always quoted in Table XLVI). It covers a great deal of entirely new ground and should prove to be extremely useful to a wide circle of readers.

J. M. Robertson.

Lubricating and Allied Oils. By Elliott A. Evans. Third edition. [Pp. ix + 210, with 39 figures.] (London: Chapman & Hall, Ltd., 1945. 15*s.* net.)

Lubrication is more of an art than a science, and it has been usual in the past to produce either mathematical works on the hydrodynamical problems concerned with lubricating films, or alternatively chemical treatises on the chemistry of the oils usually employed. Neither type got very far in solving practical problems, but this book does indicate that the chrysalis stage is ending and that lubricating problems are at last yielding to scientific attack.

It is really rather surprising that with modern developments in the accuracy of machining it is still necessary to run in a bearing. The author mentions that Bowden's "stick-slip" theory was developed during a period of enforced idleness in the Alps and arose out of consideration of the friction of skis on snow. As a result of his subsequent experiments Bowden came to the conclusion that the actual area of contact between two copper plates a foot square was only a fraction of a square millimetre, and that the excessive pressure thus engendered resulted in instantaneous local welding of the surfaces due to the intense heat generated.

The petrol and lubricating oil consumption of a 12 H.P. engine running at 40 miles per hour is given in a curve, a threefold increase in viscosity only reduces the miles per gallon from 34·5 down to 33. The method of cutting grooves in bottom bearings was thought in the past to improve oil distribution. It is now shown to have an adverse effect by reducing the fluid pressure automatically built up in the oil film by the rotation of the shaft and consequently the thickness of the oil film.

"Carbon" deposits are shown to be oxidised oil products containing about 70 per cent. carbon in a combined form. The ash contains about the same percentage of iron, but the presence of calcium and silica indicates that dust removal from the air could be improved.

In the selection of lubricants the author advises "it is prudent to err on the extravagant side than in parsimony." It is a curious statement to make in a scientific book and illustrates the fact that lubrication has yet to grow out of some of its alchemical beliefs. Viscosity index is mentioned in a number of places but does not seem to be defined.

The present edition represents twelve further fruitful years of lubrication research and is a very readable book.

M. B. Donald.

An Introduction to the Chemistry of Cellulose. By J. T. Marsh, M.Sc., F.R.I.C., F.T.I., and F. C. Wood, Ph.D., F.R.I.C., F.T.I. Third Edition. [Pp. xi + 525]. (London: Chapman & Hall, Ltd., 1945. 32*s.* net.)

The outstanding characteristic of this book is its readability. It can be read from cover to cover without the usual impulse to "skip" so often felt with regard to books on highly specialised subjects and without the mental irritation that accompanies a dish of highly compressed and condensed factual information. This somewhat rare achievement in scientific literature results from a close adherence to the limits the authors have set themselves as to the scope of the work and from the simplicity and directness of the English in which it is written.

The division of the book into five parts, three of which are concerned with industrially important properties of cellulose and derivatives, would suggest some degree of bias towards the utilitarian and technological aspects of the subject at the expense of the more fundamental chemical aspects. The bias, if it exists, is nevertheless not pronounced and the section devoted to molecular structure amply compensates in quality for what it lacks in quantity. Here is no tedious recording of prehistoric experimentation or the faithful piecing together of exploded hypotheses. If the aim of the authors has been to acquaint the "scientific layman" with the main trend of present-day views on the molecular structure of cellulose then in this section of their book they have succeeded admirably.

One feels, indeed, that the book is intended primarily for just such a reader who is "lay" in the sense of not being a cellulose chemist either industrially or academically. The academic carbohydrate chemist will expect to find a fuller presentation of the pros and cons of current hypotheses on fine structure while the industrial chemist whose job is cellulose might consider the treatment to be rather superficial and lacking in substance. Nevertheless, it is certain that both types of expert will benefit in refreshment of mind by a perusal of this book.

If one item is to be singled out as of exceptional value it is the inclusion of a chapter on "Chain Molecules." It was a happy thought to put cellulose in its right setting as a member of the class of linear high polymers. In this chapter comparisons are made with vinyon, nylon, natural silk, alginates, chitin, wool and artificial protein fibres, and a brief résumé of high polymer chemistry is given.

The only blemish of serious import in this excellent "Introduction" lies in the inadequacy of the system of reference to the literature. References are incorporated in the text and to seek out a back reference is like looking for the proverbial needle. The value of the book would be greatly enhanced by the addition of a comprehensive bibliography to which reference is made by number.

STANLEY PEAT.

Forensic Chemistry and Scientific Criminal Investigation. By A. LUCAS, O.B.E., F.R.I.C. Fourth edition. [Pp. 340.] (London: Edward Arnold & Co., 1945. 25*s.* net.)

THIS book was first published in 1921, and its value has been demonstrated by the fact that it has now entered upon its fourth edition. The present work has been brought to date, alterations and additions have been copiously made, and some 200 fresh references have been added. The bibliography is most extensive and is of high value both to the routine, and research, worker. It is regrettable that on page 9 the reader is faced with an errata slip.

The introductory chapter is devoted to the general procedure in connection with the reception, and examination, of articles together with recommendations for framing reports on the completion of examination. The subsequent chapters cover a very wide field and include such subjects as the examination of blood stains, various stains upon clothing, counterfeit coins, documents, dust, dirt, glass, explosives, fibres, woven fabrics, string, rope, finger-prints, foot-prints, tracks of vehicles, and erased numbers on metal. There are also chapters dealing with fires and insurance frauds, cartridges and projectiles, photography, X-rays, ultra-violet rays, and infra-red rays. Other subjects include the preservation of the human body after death, robbery from letters and parcels, and tobacco. Chapter XIII, which contains some 58 pages, embraces the subject of toxicology.

In the description of the examination of blood stains, the author devotes three pages to the hæmin crystal test, but he has overlooked the importance of the hæmochromogen crystal test generally regarded as superior to, and more constant under varying conditions than, the former test. He makes reference to the diagnostic importance of the spectrum of hæmochromogen in the examination of extracts of blood, but omits a description of the simple combined method of microscopic and microspectroscopic examination of hæmochromogen crystals so commonly in use at the present time. In this connection, there is no mention of Takayama's solution, or of the solution recommended by Ferreira.

There are some additional minor points which would sustain criticism, but the book as a whole strikes a high standard and may certainly be regarded as a comprehensive and authoritative work for all whose interests impinge the realm of forensic chemistry and scientific investigation.

JOHN GLAISTER.

BOTANY

Chromosome Atlas of Cultivated Plants. By C. D. DARLINGTON, D.Sc., F.R.S., and E. K. JANAKI AMMAL, D.Sc. [Pp. 397.] (London: George Allen & Unwin, Ltd., 1945. 12*s.* 6*d.* net.)

THE authors have rendered a very considerable service to plant geneticists in bringing together in a convenient and easily available form all the known chromosome numbers of the cultivated plants of the world and of their wild relatives. The main section of the book, occupying 310 pages, is a tabulated list of the plants, their popular or vernacular names where these exist, their somatic chromosome numbers, the authority for the chromosome count, the uses for which the plants are cultivated and their geographical distribution.

The systematic arrangement of the families of the Angiosperms follows, with minor exceptions, the order proposed by Hutchinson in his *Families of Flowering Plants.* The genera within the families, or within tribes into which the families are subdivided, are arranged in ascending order of basic chromosome numbers, and individual genera are subdivided in a similar way where necessary. An alphabetical index to the genera and families renders reference easy and rapid. In all, over 11,000 species are included in the catalogue, but about 1000 of the more useful species are without chromosome numbers and a further 2000 are summarised without individual names, " owing to their uniformity of number or lack of cultural interest." The known chromosome numbers of seed plants range from 6 (in species of *Callitriche, Crepis* and *Crocus*) to about 500 in a species of *Kalanchoë.* The largest number counted with certainty seems to be 308 in the Black Mulberry (*Morus nigra*).

Where two accounts differ in number for the same species both are quoted, except where error is obvious. Where accounts agree the most recent important one is quoted. A full bibliography, occupying 28 pages, is given. The uses of the plants are indicated by a system of initials signifying the group of use, or uses, to which each plant is put. The groups are quite general, for example, medicinal plants, grains and pulses, perfumes, textile fibres and woods, but may prove helpful. Two somewhat curious categories are carnivorous plants and parasitic and semi-parasitic plants, the remainder of the horticultural plants (grown for decoration, curiosity, etc.) not being further subdivided.

The main body of the work is preceded by a short introduction in which Dr. Darlington sets forth his ideas of the origins of cultivated plants and the place of chromosome studies in the task of unravelling the past history of these plants. Vavilov's evidence for the centres of origin being the centres of greatest diversity is briefly reviewed, and then it is shown that the evolution of the special characteristics of the cultivated plants is in the first place a consequence of cultivation. Man has exercised unconscious selection by the tillage, sowing and harvesting conditions he has imposed on the plants. Not infrequently the weeds of the crop were similarly modified and sometimes replaced the primary crop when cultivation spread into environments less favourable to it. Finally, the kinds of variation selected in the development of cultivated plants can be made out from a comparative study of the chromosome numbers of the cultivated plants and their wild relatives.

D. G. CATCHESIDE.

PHYSIOLOGY AND MEDICINE

The Living Body. By C. H. BEST, M.A., M.D., D.Sc.(Lond), F.R.S., F.R.C.P.(Canada), and N. B. TAYLOR, M.D., F.R.S.(Canada), F.R.C.S. (Edin.), F.R.C.P.(Canada), M.R.C.S.(Eng.), L.R.C.P.(London.) Second edition. [Pp. xxii + 571, with 16 coloured plates and 283 figures.] (London: Chapman & Hall, Ltd., 1945. 24*s*. net.)

THERE is little to criticise in the subject matter of this book, which is accurate and up to date. The illustrations are helpful and particularly well chosen, though most of the half-tone pictures have suffered badly in reproduction. Care is taken to describe the tissues of the body and their microscopic structure, while many aspects of physics and chemistry which have a direct bearing on physiology receive preliminary treatment. Superficially this seems to render the book independent of the fundamental sciences, but it is questionable whether anyone unacquainted with them could use it to the best advantage. Indeed, although the book is a skilful and almost irreproachable summary of the important facts of physiology, and although these are presented in a simple way, there are too many of them. Information is delivered in a succession of brief sentences, nearly every one conveying a fact, and the effect on a reader unfamiliar with the subject must be quite overpowering. The book should be of most use to persons of trained intelligence (*e.g.* science students), who desire an authoritative but not too technical account of physiology. It should also serve well as a reference book for nurses, and others who need a reliable authority to back up an elementary knowledge. For more advanced students it furnishes a thorough survey of the ground which, with the help of lectures, larger text-books and practical work, they must cover in greater detail.

W. H. NEWTON.

Radium Therapy. Its Physical Aspects. By C. W. WILSON, M.Sc., Ph.D., F.Inst.P. [Pp. xii + 224, with 97 figures, including 14 plates, and 30 tables.] (London: Chapman & Hall, Ltd., 1945. 18*s*. net.)

THIS excellent book supplies a need acutely felt by medical radiologists and physicists attached to hospitals. It has obviously been written by a physicist well versed in the many problems which confront all workers in the field of radium therapy, and its publication at the present moment is very timely, in view of the expansion of radiation therapy which must occur as a result of the post-war implementation of the Cancer Act of 1939.

After chapters devoted to the physical properties of those members of the radium family which are of interest in medicine, the author proceeds to discuss very thoroughly the theory, methods and apparatus of gamma-ray dosimetry. This is followed by a lucid and competent survey of the three chief techniques employed in using radioactive substances for the treatment of patients, namely: by applicators distributed over the surface to be treated; by distributing containers in the natural cavities of the body, or implanting them in the affected tissues; or by the use of canalised beams of radiation from strong sources placed some distance away from the body.

Within the limits which he has set himself, the author has written a singularly useful book. It should be regarded as indispensable by all students of medical radiology and practising radium therapists.

The reviewer, however, regrets that Dr. Wilson did not include a chapter or chapters devoted to the production and properties of artificial radio elements. Even before the beginning of the War some indication of their possible usefulness in physiology and medicine was already clear. As a result of the intensive work in nuclear physics during the War, it is now certain that the scope and possibilities of radiation therapy have been immeasurably enlarged.

Some minor blemishes in an otherwise excellent book can be expected to disappear from the next edition. Among these are a few misprints, an occasional innocuous misstatement and some examples of slip-shod English (e.g. *wheeled by* for *wheeled past*, p. 24, *wire-rope cable*, p. 164, energy *involved* in protection, p. 209, etc.)

F. L. H.

Topley and Wilson's Principles of Bacteriology and Immunity. Vols. I and II. Third Edition revised by G. S. Wilson, M.D., F.R.C.P., D.P.H., K.H.P., and A. A. Miles, M.A., F.R.C.P. [Pp. xii + 2054 + xliv (index), with 302 figures and 207 tables.] (London: Edward Arnold & Co., 1946. 60*s.* net.)

The production of a new edition of Topley and Wilson in the middle of a war and in the austerities of peace is an achievement which ensures the gratitude of all British bacteriologists who are concerned with the paucity of personnel available to rebuild a flourishing industry. The work is, of course, primarily intended for the instruction of medical bacteriologists, but the generalities are so well presented that the volumes are likely to be of great service in wider spheres.

The present edition is the work of Wilson and A. A. Miles who took the place of the late W. W. C. Topley on his appointment to the Agricultural Research Council.

Wilson remarks in his preface on the difficulties experienced from the lack of European and Japanese literature and on the one-sided picture this entails. To counteract this the authors have given the data in unusual detail, have augmented the bibliographies, and have been "perhaps unduly cautious in drawing our conclusions." In view of this confession it would be unreasonable to complain but proper to sympathise. It makes no difference to the value of the work as a reference book, in fact it may be an advantage, but the undue massing of pros and cons in an argument of admitted complexity must leave even a senior student at a loss for the guidance of a text book. However, for another edition, we hope that the times will be more suited to "careful leisurely recapitulation" and anticipate that all sections will then attain the level of the "classification" chapter, in which a masterly consideration of evidence and policy leads to precise and agreeable conclusions.

During the past ten years there has been a large influx of biochemists into bacteriology with a specialised literature so vast that it has to be sorted out by a number of newly created Review Journals. Inevitably the two authors from sheer lack of time have availed themselves of these facilities. One feels in this a certain nostalgic regret that we cannot have the personal Topley-Wilson pronouncement to which we have grown accustomed, in all branches of a rapidly expanding subject. A little purely biological criticism would not come amiss, and, we have no doubt, this will be available in due course.

So far as is known to the Reviewer there is no work by two authors in any language which can compare with Topley and Wilson for accuracy, judgement and fluency. These characters are well maintained in the new edition. As a reference book there can be little which is not presented adequately with full indications for further reading. As a text-book it is frankly much wider and more detailed than ordinary text-books in medical bacteriology and, no doubt, such a mass of knowledge is not required by many students. Topley and Wilson were, however, concerned with a higher education in medical bacteriology, and this book sets the standard they wished to attain. It is well known that they succeeded, and it is to be hoped that the publication of this new edition heralds the re-establishment of the Diploma course which can claim to have attained considerable national importance.

Paul Fildes.

Penicillin : A Dramatic Story. By Boris Sokoloff, M.D. [Pp. viii + 167.] (London: George Allen & Unwin, Ltd., 1946. 7*s.* 6*d.* net.)

The discovery and development of penicillin have led to a flood of popular scientific literature, for, in these days, the man in the street takes a keen interest in scientific discovery and its possible effect on his daily life. While most of this literature has been frankly journalistic, there have been some efforts, of which this small book is one, to present the facts in a simple, readable, and at the same time accurate form.

The author covers a great deal of ground from the pioneering days of Lister and his technique of antiseptic surgery, right up to the present time and the achievement of penicillin. He tells us how penicillin was born in London, developed at Oxford, and then, when clinical trial had shown its importance, was taken to America to be manufactured in bulk. He tells briefly of the effect of the new drug on a number of infective conditions. But so rapid has been the advance in the last year that much of this section is already out of date. The discovery of penicillin has stimulated a widespread interest in the metabolic products of the moulds and higher bacteria, and some of the new substances are reviewed.

In a book written by an American for the American public, it is pleasing to find that due credit is given to British pre-eminence in this great discovery. There are a number of inaccuracies, but these are not such as to alter the main trend of the story. The language is American, and many of the expressions, such as "Gave the mould the works," are expressive but sound odd to the scientist on this side of the Atlantic.

G. C. Hickie.

BOOKS RECEIVED

(*Publishers are requested to notify prices.*)

Advanced Mathematics for Technical Students, Part I. By A. Geary, M.A., M.Sc., Head of the Mathematics Department, Northampton Polytechnic, H. V. Lowry, M.A., Head of the Mathematics Department, Woolwich Polytechnic, and H. A. Hayden, D.Sc., Head of the Mathematics Department, Battersea Polytechnic. London : Longmans, Green & Co., 1945. (Pp. viii + 419, with 262 figures.) 12*s*. 6*d*.

Leçons et Problèmes sur la Théorie des Corps déformables. Par André Mercier, Dr. ès Sc., Professeur de Physique théorique à l'Université de Berne. Paris : Gauthier-Villars, 1943. (Pp. 156, with 34 figures.)

Waveform Analysis. A Guide to the Interpretation of Periodic Waves, including Vibration Records. By R. G. Manley, B.Sc., Vibration Engineer, Silentbloc Ltd. London : Chapman & Hall, Ltd., 1945. (Pp. xii + 275, with 103 figures and 39 tables.) 21*s*. net.

Oceanography for Meteorologists. By H. U. Sverdrup, Professor of Oceanography, University of California. London : George Allen & Unwin, Ltd., 1945. (Pp. xvi + 246, with 70 figures and 4 charts.) 12*s*. 6*d*. net.

Atomic Artillery and the Atomic Bomb. By John Kellock Robertson, F.R.S.C. New York : D. van Nostrand Co., Inc. ; London : Macmillan & Co., Ltd., 1945. (Pp. xiv + 173, with 8 plates and 23 figures.) 12*s*. 6*d*. net.

Electric Discharge Lamps. By H. Cotton, M.B.E., D.Sc., M.I.E.E., Professor of Electrical Engineering at University College, Nottingham. Vol. XII of a Series of Monographs on Electrical Engineering, under the Editorship of H. P. Young, M.I.E.E., M.A.I.E.E. London : Chapman & Hall, Ltd., 1946. (Pp. xvi + 435, with 216 figures, including 12 plates.) 36*s*. net.

An Introduction to the Theory and Design of Electric Wave Filters. By F. Scowen, B.Sc., A.Inst.P., Post Office Research Station. London : Chapman & Hall, Ltd., 1945. (Pp. xii + 164, with 60 figures, including 1 folding plate.) 15*s*. net.

An Introduction to X-Ray Metallography. By A. Taylor, M.Sc., Ph.D., F.Inst.P. London : Chapman & Hall, Ltd., 1945. (Pp. xii + 400, with 240 figures, including 45 plates and 2 folding plates, and 47 tables.) 36*s*. net.

Surveying for Young Engineers. By S. Wright Perrott, M.A.I., M.Inst.C.E., late Professor of Civil Engineering, University of Liverpool. London : Chapman & Hall, Ltd., 1946. (Pp. viii + 232, with frontispiece, 2 maps, and 95 figures, including 12 plates.) 10*s*. 6*d*. net.

Modern Petrol Engines. With Special Reference to Automobile, Aircraft and Stationary Types. By Arthur W. Judge, A.R.C.S., D.I.C., A.M.Inst.Aut.E., A.F.R.Ae.S., M.S.A.E. London : Chapman & Hall, Ltd., 1946. (Pp. viii + 509, with 304 figures, including 8 plates and 1 folding plate.) 36*s*. net.

Aircraft Engines, Vol. I. By Arthur W. Judge, A.R.C.S., D.I.C., A.M.Inst.Aut.E., M.S.A.E., A.F.R.Ae.S., late Senior Technical Assistant, Air Ministry. Second edition. London: Chapman & Hall, Ltd., 1945. (Pp. xii + 492, with 308 figures and 33 tables.) 28*s.* net.

Experimental Electronics. By Ralph H. Müller, Professor of Chemistry, R. L. Garman and M. E. Droz, Assistant Professors of Chemistry, New York University. Prentice-Hall Chemistry Series. New York: Prentice-Hall, Inc.; London: George Allen & Unwin, Ltd., 1945. (Pp. xvi + 330, with 177 figures and 3 tables.) 21*s.* net.

Modern Chemistry. Some Sketches of its Historical Development. By A. J. Berry, M.A., Fellow of Downing College, and University Lecturer in Chemistry, Cambridge. Cambridge: at the University Press, 1946. (Pp. x + 240.) 10*s.* 6*d.* net.

Les derniers Progrès de la Chimie. Par Roger Simonet, Professeur agrégé. Paris: Éditions Balzac, 1944. (Pp. 343, with 1 plate.) Frs. 75.—.

Le Charbon: son Histoire, son Destin. Par Eugène Schneider, Membre de l'Institut. Paris: Librairie Plon, 1945. (Pp. 350, with 15 plates.) Frs. 160.—.

General and Inorganic Chemistry for University Students. By J. R. Partington, M.B.E., D.Sc., Professor of Chemistry in the University of London, Queen Mary College. London: Macmillan & Co., Ltd., 1946. (Pp. xxiv + 916, with 345 figures.) 36*s.*

Colloids. Their Properties and Applications. By A. G. Ward, M.A., formerly Scholar of Trinity College, Cambridge. London and Glasgow: Blackie & Son, Ltd., 1945. (Pp. viii + 133, with 6 plates and 28 figures.) 5*s.* net.

Chemistry for Junior Forms. By A. C. Cavell, M.A., B.Sc., Senior Science Master at Uppingham School. London: Macmillan & Co., Ltd., 1946. (Pp. viii + 296, with 111 figures.) 5*s.*

What Industry owes to Chemical Science. Third edition. Based on the scheme of the First and Second Editions by Richard B. Pilcher, O.B.E., formerly Registrar and Secretary of the Royal Institute of Chemistry, and Frank Butler-Jones, B.A., A.R.I.C. Issued by authority of the Council of the Royal Institute of Chemistry. Cambridge: W. Heffer & Sons, Ltd., 1945. (Pp. viii + 372, with 13 plates.) 18*s.* net.

A Dictionary of Metallography. By R. T. Rolfe, F.R.I.C., Chief Metallurgist, Messrs. W. H. Allen, Sons & Co., Ltd., Bedford. London: Chapman & Hall, Ltd., 1945. (Pp. viii + 243.) 15*s.* net.

An Introduction to the Chemistry of Cellulose. By J. T. Marsh, M.Sc., F.R.I.C., F.T.I., and F. C. Wood, Ph.D., F.R.I.C., F.T.I. Third edition. London: Chapman & Hall, Ltd., 1945. (Pp. xii + 525.) 32*s.* net.

Lubricating and Allied Oils. By Elliott A. Evans, Chief Chemist to C. C. Wakefield & Co., Ltd. Third edition. London: Chapman & Hall, Ltd., 1945. (Pp. x + 210, with 39 figures, including 3 plates, and 5 tables.) 15*s.* net.

Le Présent et l'Avenir des Matières plastiques. Par Jean Delorme, Ingénieur-Chimiste E.C.I.L., Ingénieur-Conseil. Grenoble and Paris: B. Arthaud, 1944. (Pp. 300, with 88 figures.)

Forensic Chemistry and Scientific Criminal Investigation. By A. Lucas, O.B.E., F.R.I.C., formerly Director, Chemical Department, Egypt. Fourth edition. London: Edward Arnold & Co., 1945. (Pp. 340.) 25*s.* net.

Chromosome Atlas of Cultivated Plants. By C. D. Darlington, F.R.S., and E. K. Janaki Ammal, D.Sc. London: George Allen & Unwin, Ltd., 1945. (Pp. 397.) 12*s.* 6*d.* net.

Pure Cultures of Algæ. Their Preparation and Maintenance. By E. G. Pringsheim, Department of Botany, Queen Mary College, University of London. Cambridge: at the University Press, 1946. (Pp. xii + 119, with 1 plate and 8 figures.) 7*s.* 6*d.* net.

An Introduction to Industrial Mycology. By George Smith, M.Sc., F.R.I.C., Research Demonstrator, Department of Biochemistry, London School of Hygiene and Tropical Medicine. Third edition. London: Edward Arnold & Co., 1946. (Pp. xvi + 271, with 143 figures.) 20*s.* net.

Petit Atlas des Mammifères. I. Monotrèmes, Marsupiaux, Édentés, Rongeurs. II. Ongulés. III. Cétacés, Siréniens, Pinnipèdes, Carnivores, Chiroptères. IV. Insectivores, Primates. Par Paul Rode, Dr. ès Sc., Assistant au Muséum national d'Histoire naturelle. Paris: Éditions N. Boubée & Cie., 1942–44. (I: pp. 47, with 12 plates; II: pp. 61, with 18 figures and 12 plates; III: pp. 65, with 6 figures and 12 plates; IV: pp. 81, with 12 figures and 12 plates.)

Petit Atlas des Oiseaux. I. Passereaux. II. Passereaux (fin.), Rolliers, Pics, Perroquets, Rapaces nocturnes. III. Rapaces diurnes, Pigeons, Gallinacés, Rales, Grues. IV. Bécasseaux, Goélands, Canards, Hérons, Impennes, Ratites. Par L. Delapchier, Membre du Conseil de la Société française d'Ornithologie. Paris: Éditions N. Boubée & Cie., 1942. (I: pp. 39, with 16 plates; II: pp. 35, with 12 plates; III: pp. 39, with 12 plates; IV: pp. 63, with 12 plates.)

Petit Atlas des Poissons. I and II. Poissons marins. Par Léon Bertin, Dr. ès Sc., Agrégé de l'Université, Sous-Directeur de Laboratoire au Muséum national d'Histoire naturelle. III. Poissons des Eaux douces: Espèces françaises. Par le Dr. Jacques Pellegrin, Professeur au Muséum national d'Histoire naturelle. Paris: Éditions N. Boubée & Cie., 1942–43. (I: pp. 51, with 12 plates; II: pp. 31, with 12 plates; III: pp. 57, with 13 figures and 12 plates.)

Petit Atlas des Amphibiens et Reptiles. I. Apodes, Urodèles, Anoures, Rhynchocéphales, Chéloniens, Crocodiliens. II. Lacertiliens, Ophidiens. Par F. Angel, Assistant d'Herpétologie au Muséum national d'Histoire naturelle. Paris: Éditions N. Boubée & Cie., 1942. (I: pp. 47, with 12 plates; II: pp. 51, with 12 plates.)

Petit Atlas des Fossiles. I. Fossiles primaires et triasiques. II. Fossiles jurassiques et crétaciques. III. Fossiles tertiaires et quaternaires. Par Georges Denizot, Dr. ès Sc., Professeur à l'Université d'Aix-Marseille. Second edition. Paris: Éditions N. Boubée & Cie., 1943. (I: pp. 33, with 18 plates; II: pp. 34, with 22 plates; III: pp. 32, with 20 plates.)

Inventaire des Oiseaux de France. Par Noel Mayaud, avec la collaboration d'Henri Heim de Balsac et Henri Jouard. Paris: André Blot. (Pp. viii + 211.)

La Vie des Sauterelles. Par L. Chopard, Sous-Directeur du Laboratoire d'Entomologie au Muséum d'Histoire naturelle. Paris: Librairie Gallimard, 1945. (Pp. 204, with 18 plates and 15 figures.) Frs.150.—

L'Origine des Espèces. Par Émile Guyénot, Correspondant de l'Institut, Professeur à l'Université de Genève. No. 141 of the Series "Que sais-je?" Paris: Presses Universitaires de France, 1944. (Pp. 128, with 19 figures.)

Les Climats et l'Organisme humain. Par Émile Duhot, Professeur à la Faculté de Médecine de l'Université de Lille, Médecin des Hôpitaux. No. 171 of the Series "Que sais-je ?" Paris : Presses Universitaires de France, 1945. (Pp. 128.)

Le Moteur Vivant. Par le Dr. Paul Chauchard, Directeur adjoint à l'École pratique des Hautes Études. No. 181 of Series "Que sais-je ?" Paris : Presses Universitaires de France, 1945. (Pp. 128, with 23 figures.)

La Genèse de l'Humanité. Par C. Arambourg, Professeur au Muséum national d'Histoire naturelle. No. 106 of Series "Que sais-je ?" Paris : Presses Universitaires de France, 1943. (Pp. 136, with 36 figures.)

Comment se défend l'Organisme. Par Léon Binet, Professeur à la Faculté de Médecine de Paris. No. 5 of Series "Que sais-je ?" Paris : Presses Universitaires de France, 1943. (Pp. 119, with 12 figures.)

Génétique et Hérédité. Par Maurice Caullery, Professeur honoraire à la Sorbonne, Membre de l'Institut. No. 113 of Series "Que sais-je ?" Paris : Presses Universitaires de France, 1943. (Pp. 128, with 32 figures.)

L'Évolution physiologique. Étude des Pertes de Fonctions chez les Micro-organismes. Par André Lwoff, Chef du Service de Physiologie Microbienne à l'Institut Pasteur. Paris : Hermann & Cie., 1944. (Pp. 308, with 12 figures and 33 tables.)

The Living Body. A Text in Human Physiology. By Charles Herbert Best, M.A., M.D., D.Sc.(Lond.), F.R.S., F.R.C.P.(Canada), Professor and Head of Department of Physiology, and Norman Burke Taylor, M.D., F.R.S.C., F.R.C.S.(Edin.), F.R.C.P.(Canada), M.R.C.S.(Eng.), L.R.C.P.(Lond.), Professor of Physiology, University of Toronto. Revised edition. London : Chapman & Hall, Ltd., 1945. (Pp. xxii + 571, with 16 coloured plates and 283 figures.) 24*s.* net.

Radium Therapy. Its Physical Aspects. By C. W. Wilson, M.Sc., Ph.D., F.Inst.P., Physicist in the Department of X-Ray and Radium Therapy, Westminster Hospital. London : Chapman & Hall, Ltd., 1945. (Pp. xii + 224, with 97 figures, including 14 plates, and 30 tables.) 18*s.* net.

Topley and Wilson's Principles of Bacteriology and Immunity. Vols. I and II. Revised by G. S. Wilson, M.D., F.R.C.P., D.P.H., K.H.P., Professor of Bacteriology as Applied to Hygiene, London School of Hygiene and Tropical Medicine, and A. A. Miles, M.A., F.R.C.P., Professor of Bacteriology, University College Hospital Medical School. Third edition. London : Edward Arnold & Co., 1946. (Pp. xii + 2054 + xliv (index), with 302 figures and 207 tables.) 60*s.* net.

Penicillin. A Dramatic Story. By Boris Sokoloff, M.D. London : George Allen & Unwin, Ltd., 1946. (Pp. viii + 167.) 7*s.* 6*d.* net.

Pasteur. Par Henri Mondor. Paris : Éditions Corrêa & Cie., 1945. (Pp. 189, with 9 plates.) Frs.150.—.

The Endeavour of Jean Fernel, with a List of the Editions of his Writings. By Sir Charles Sherrington, O.M. Cambridge : at the University Press, 1946. (Pp. x + 223, with 27 figures, including 7 plates.) 16*s.* net.

Conférence européenne de la Vie rurale 1939. La Maison et l'Aménagement ruraux. Rapport préparé sous les auspices du Comité d'Hygiène. Genève : Société des Nations, 1939. (Pp. 62.)

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SCIENCE PROGRESS

THE ENTOMOLOGY OF STORED PRODUCTS

By J. W. MUNRO, M.A., D.Sc.

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In those branches of commerce which deal in commodities of vegetable, or of animal, origin a greater or less degree of infestation by insects occurs, and periodically infestation may become sufficiently extensive or intensive as to have considerable economic importance. This happens notably in times of war when normal trading is upset and stocks of commodities accumulate in the exporting countries for lack of shipping or in the importing countries seeking to build up reserves.

The writer has concerned himself with this branch of applied entomology over a number of years and it may be useful at this stage in the development of commercial entomology, when, under Government ægis, a considerable expansion of work in this field may be looked for, to give some account of the nature of the scientific problems presented and such advance as has been made in dealing with them.

Insect infestation of stored produce is seen in its simplest form in peasant agriculture in tropical or sub-tropical countries. In East Africa, for example, infestation can be traced from the occurrence of such insects as the grain weevils (*Calandra*) and the grain moth (*Sitotroga*) in the ripening crop, through the development of a typical insect community in the village (where the grain is stored in wicker baskets on poles some three feet above ground, or suspended from a tree) to the much heavier infestation to be seen in bagged maize at the railway awaiting transport to the coast. Here, in the merchant stores or "go-downs," many commodities meet and infestation becomes complex; the insect communities typical of grain, or of pulses, or of ground-nuts, become mixed.

In our warehouses the insect population is even more complex and consists, in part, of populations of insect species which, originally exotic, are now endemic; and, in part, of populations, sometimes of the same, sometimes of other species which have only recently arrived in this country. At present it is hardly possible to distinguish these two parts of the population, although Philip [1] in a genetical analysis of three small populations of the beetle *Dermestes vulpinus* collected in a London warehouse was able in one of them to distinguish individuals of Kenya origin from individuals of London origin. On the other hand, some species in these warehouse populations, like the nutmeg weevil *Araeocerus*, cannot survive our winter and their persistence in our warehouses is wholly dependent on fresh introductions every season.

The first systematic study of the insects and mites infesting stored products in this country was carried out by a committee of the Royal Society [2]. It dealt solely with grain pests and issued ten reports dealing with the airtight storage of grain and its effects on certain insects; with the Tyroglyphid mites and with certain Hymenopterous parasites of grain pests.

The main recommendation made for the control of grain weevils and mites—the storage of grain in airtight silos or granaries so that the insects die of asphyxiation—has not yet proved practicable, but the scientific results contained in these reports, dealing with the respiration of grain insects and with the biology of the mites, have been of value to later workers.

It may be convenient in describing recent progress in this field of applied entomology to consider two periods of development, the first, from 1929 until the war, when work in this field was almost confined to the Imperial College, and the second, when the Department of Scientific and Industrial Research entered the field (at first in close collaboration with the College), to be followed later by the Ministry of Food.

In the first period the work was largely exploratory, designed to assess, or define, the nature of the scientific problems and to give "first aid" in the control of moths of the genera *Ephestia* and *Plodia* which were then causing serious concern in the cocoa, tobacco and dried fruits trades.

In starting these investigations the work of the American and German entomologists was available, notably that of Back and Cotton in the United States on fumigation and Zacher's admirable monograph on stored products entomology published in Abderhalden's *Handbuch der Arbeitsmethoden*.

After surveys to assess the nature of insect infestation in the cocoa, tobacco, and dried fruits trades had been carried out, it was possible to draw up a preliminary research programme comprising work in systematic entomology, and insect biology, and in the physical chemistry of insecticides, particularly fumigants. Later, work in insect physiology was included in the programme. During this period a special laboratory, including insectaries and constant temperature chambers for the rearing of insects and special chambers for work on fumigants, was gradually built up and equipped. Staff was recruited and trained for the work and before war broke out the exploratory work had virtually been completed—except for the grain trade—and an extensive programme of work in entomology and in physical chemistry had been planned and embarked on.

In the second period, except that a survey of grain insects was added to those carried out and the same attention was then given to the problems of the grain trade as had previously been given to others, the general research programme drawn up and begun by the College was continued under Government by the Department of Scientific and Industrial Research.

SURVEY WORK

The object of the surveys carried out was to ascertain the nature of the scientific problems and the possibility of rendering "first aid." Such work is largely fact finding, but during the course of it valuable insect material has been collected and a measure of the "practical" problem and of the scientific problems underlying it has been obtained. Surveys of infestation affecting the timber, cocoa, tobacco, dried fruits and grain trades have now been made. The survey of insects infesting timber was carried out by the writer in 1924 with the assistance of Dr. R. C. Fisher. Surveys of the cocoa, tobacco, and dried fruits industries were carried out during the years 1929 and 1931 and that of the grain and flour-milling trade in 1938–39.

One generalisation from these surveys seems permissible, namely, that accumulation of large stocks of a commodity and prolonged storage of it are the chief factors which promote rapid increases of the insect population. It seems, too, that a high percentage of insect parasitism in a community is an index of long-standing infestation. This assumption is supported by many observations, of which one may be cited. Some years ago a parcel of Afghan raisins which had been in store for seven years was found to be heavily infested by *Cryptophagus* beetles, and these beetles were heavily parasitised by an Encyrtid fly *Zeteticontus*. No such heavy infestation by *Cryptophagus* has been seen since, nor has the parasite *Zeteticontus* been seen or recorded again in this country.

SYSTEMATIC ENTOMOLOGY

Work in systematic entomology was first undertaken when the exact identification of the species of moths of the genus *Ephestia* infesting tobacco became urgent. A revision of this genus and its allies *Plodia* and *Strymax* was carried out. Apart from its value in easing correct identification of species, it has given us a better understanding of the genus itself. Since then and especially during the war valuable systematic work has been done on such groups as the beetles of the families Ptinidæ and Lathridiidæ by Hinton [3]. The same author has published the first part of a monograph of the beetles associated with stored products [4]. Corbet and Tams [5] have published keys for the identification of the Lepidoptera infesting stored food products and, while the authors point out that they have not based these keys on phylogenetic relationships, their work affords a basis for such phylogenetic studies and meanwhile is invaluable to the practising entomologist. Hinton [6] has studied the larval stages of three species of the genus *Ephestia* and has also described the larvæ of the more important Lepidoptera associated with stored products.

BIOLOGICAL WORK

Much of the early biological work done was exploratory and was concerned mainly with the rearing of large cultures of insects for experimental purposes. In the course of this work, when an attempt had been made to hasten the rate of multiplication of stocks, it was found that the "speeded-up" stocks were sterile. A study of this problem showed that the sterility was owing to retardation of spermatogenesis and reduction of mobility of the sperm when the insects were reared at temperatures above 27° C. [7].

The flour-beetles (*Tribolium*) have long been favourite subjects in experimental work on the theory of insect populations. Chapman, Park, Holdaway, MacLagar and, more recently, Crombie [8] have published extensive papers discussing such problems as competition and rate of growth of populations in these insects living in various media—mainly flour. Recent work on the nutrition of insects living in flour, referred to later, may assist in the further interpretation of these studies in which cannibalism—almost certainly the result of nutritional deficiency—is perhaps unduly weighted as a factor regulating population decline.

Meanwhile, population studies on a larger scale have been carried out in the warehouses themselves. Richards and Waloff [9], studying a population of *Ephestia elutella* living on bulk grain, conclude that in the specialised environment of a granary physical conditions do not vary enough to be a major influence on the trend of the population. The most important effect of variations in temperature is on larvæ resting in the dormant stage known as *diapause*. The chief factor controlling the size of the population is a disease, probably bacterial, commonly known as "wilt." These population studies are supported by investigations on the behaviour of *Ephestia elutella* breeding on grain, on development and nutrition, on diapause and on longevity and fecundity, this last an extension, under field conditions, of Norris's work.

PHYSIOLOGICAL WORK

Physiological work in this field of entomology has been concerned with the physiology of the insect, with the physiology of grain as the insects' environment and with the physiology of insecticidal action.

Insects living in stored products afford convenient subjects for the study of nutrition, and good progress has recently been made. Gay [10] showed that the larva of the bacon-beetle (*Dermestes vulpinus*) requires cholesterol for its proper growth and Fraenkel and Blewett have included the study of the sterols as factors in insect-nutrition and have given special attention to the vitamins of the B-groups. In a study of the metabolism of *Tribolium*, *Ephestia* and *Dermestes* they have shown that these insects which normally live on dry food acquire a substantial, or, at very low humidities, the greater part of the water found in the body from their food [11]. In man only about one-ninth of his daily requirements of water is derived from food combustion. Interesting confirmation of the suggestion that the rôle of intracellular symbionts in insects may be to provide accessory food substances has been found in a comparison of the vitamin requirements of the beetles *Tribolium* and *Ptinus*, on the one hand, and the beetles *Lasioderma* and *Sitodrepa* on the other [12]. In foods lacking *i*-inositol or *p*-aminobenzoic acid *Tribolium* and *Ptinus* fail to develop, while *Lasioderma* and *Sitodrepa* survive. If, however, the eggs of *Lasioderma* and *Sitodrepa* are sterilised, their larvæ cannot survive on this diet, although they develop normally on a diet containing the accessory food factors.

The physiological changes taking place in grain during storage and their interrelationships with insects infesting it was included in the original programme of the Imperial College. Work was begun on it jointly between the College and the Department of Scientific and Industrial Research and has been continued by that Department.

The first need was to devise instruments for making measurements in grain stored in bulk, and suitable apparatus and methods for the measurement of temperatures and humidity and for sampling the intergranular air and the grain itself were devised by Henderson and Oxley [13]. Methods were also devised for the measurement of the co-efficient of diffusion of carbon-dioxide through wheat.

Akin to this preparation for studying the physiology of grain is the devising of a gasometric apparatus for estimating carbon dioxide. The metabolism of insects in grain is vastly more rapid than that of the grain itself and this suggested that the carbon dioxide production of grain might be a measure of the degree of infestation. This has been tested by Howe and Oxley [14] and has in fact proved a useful method for determining the degree of insect infestation and the progress of infestation in grain and other commodities. The following table, modified from one given by the authors, indicates the relative importance of the insects listed in increasing carbon dioxide output and consequent heating of grain.

TABLE

NUMBER OF INDIVIDUALS PER POUND OF GRAIN WHICH WILL PRODUCE 1% CO_2 IN 24 HOURS AT 25° C.

Insect.	No. of Individuals per lb.	Insect.	No. of Individuals per lb.
Calandra:			
1st stage larva	160	*Rhizopertha* adults	60
4th stage larva	6–10	*Tribolium* adults	80
Pupæ	30	*Silvanus* adults	200
Adults	20	*Ephestia* mature larvæ	7

This apparatus is already being used in practice on a considerable scale and is of course a useful tool in population studies.

The need for physiological work on insecticidal action was first emphasised when a fraction of heavy naphtha was under investigation as an insecticide against bed-bugs. It was found that in fractions in which the paraffins had been removed—because it was assumed they acted merely as diluents—the toxicity to insects was less than in the ordinary fractions containing paraffin. This suggested that the paraffins perhaps assisted the passage of the toxic unsaturated constituents through the insect cuticle and led to a study by Hurst [15] of some of the factors affecting the permeability of the cuticle. Since the war studies of the insect cuticle, histological by Dennell, physiological by Fraenkel and Rudall and by Wigglesworth and other workers, have been continued on a considerable scale. Their findings have been summarised by Lees in the last issue of this journal (Vol. 34, No. 135, p. 609).

An interesting contribution to our knowledge of the mode of action of insecticides has recently been made in a study of " inert " dusts [16]. Mineral dusts have been used for many years in the control of grain insects, but the mechanism of their action was not understood. It has now been confirmed that inert dusts kill insects by desiccation. The desiccation is caused by the destruction of the fine uniform layer of wax overlying the framework of the cuticle. The magnitude of the effect of the minute perforations caused by minute particles of dusts is, the authors suggest, intelligible, since it has been shown that the rate of diffusion of a vapour through many small holes in a plate is much greater than that through a single hole of the same total area.

PHYSICAL CHEMISTRY

One of the most important means of controlling insects infesting stored products is by fumigation. When the writer first investigated this method of control, fumigation practice seemed wholly empirical and in particular the methods of assessing its efficiency were unsatisfactory. The "practical man" relied on "the dead insect." The writer, while agreeing that the insect was dead, wanted to know why it died. This led to an investigation of the behaviour of fumigants along three main lines:—study of the application of fumigants to ships and warehouses, study of the methods of determining fumigants, and study of the absorption of fumigants both by the commodities treated and by the fabric of the fumigation chamber or warehouse and the various containers—bags, fruit-boxes, etc.—of the commodities [17, 18]. The more important fumigants investigated were hydrogen cyanide, ethylene oxide, methyl allyl bromide, trichloro-acetonitrite, methyl bromide and the chlorinated hydrocarbons.

Considerable advances have been made in the determination of fumigants both in the air space and in fumigated goods. These have emphasised the high importance of sorption of the fumigant by the goods and by the walls and fabric of the building or chamber. Sorption not only affects the amount of free fumigant available to destroy the insects, it affects the time of airing or ventilation required after fumigation and the residual fumigant left in the goods, either in its original form or after chemical change, *e.g.* cyanhydrins formed during fumigation with hydrogen cyanide.

Work recently published and experiments just completed in the Imperial College throw light on the mechanism of the sorption of organic vapours used as fumigants on wheat. Lubatti [19, 20] summarises our present knowledge as follows:

"Fumigants are sorbed by wheat very slowly. Three main factors are involved, (*a*) permeability of the seed coats, (*b*) diffusion and sorption in the endosperm, (*c*) chemical reactions which may take place in the tissues of the grains. The seed coats form a continuous envelope relatively impervious to gases, and they appear to be the main factor controlling sorption. In wheat which has been deprived by abrasion of the seed coats, sorption appears to be of the same order as that in ground wheat.

"The investigations of Barrer and Rideal and those of Wicke have established that in certain sorbents the rate of sorption is governed by the laws of diffusion. This is true in wheat with vapours which do not undergo a chemical decomposition or are only slightly reactive. Examples of such vapours are those of ethylene dichloride and hydrogen cyanide. Equilibrium with these is substantially reached in 15–20 days, and the experimental curves follow closely those calculated from Fick's law.

"The forces which retain ethylene dichloride and hydrogen cyanide in wheat are predominantly physical (van der Waals). Accordingly, sorption decreases with increase in temperature. It has also been found that the amount of fumigant sorbed is approximately the same at any time-concentration product, whether the concentration is kept constant or allowed to fall, provided that the average concentrations are the same.

"Other vapours, such as those of ethylene oxide, react with the constituents of the wheat grains. The reaction does not usually take place with one specific chemical compound. Many constituents of the tissues take part in the process, in which enzymic action may also assist. The rate of decomposition of the fumigant in the course of these reactions is apparently unimolecular.

"With chemically active vapours, sorption increases with an increase in temperature. The time-sorption curve, calculated from experiments carried out with falling concentration, gives lower values than those obtained in experiments performed at an equivalent even concentration.

"An increase of the moisture contents of wheat brings about an increase of sorption with all the fumigants studied. This also may be connected with changes of permeability and structural rearrangements in the tissues of the wheat grain. Moisture content is a factor as important as time, concentration of vapour or temperature."

With attention now being given to the fumigation of viable seeds, *e.g.* onion seed and potato for the control of eelworms, the advances made in determining the proportions of the residues left after fumigation, whether these are loosely or firmly held, or are chemically changed, is of high importance. In seed fumigation the water content is an important factor aiding absorption especially when fat-soluble fumigants (*e.g.* methyl bromide) are employed and when the seeds are rich in fat (*e.g.* onion seeds with a fat content of 20 per cent.)

In some circumstances fumigation of warehouses, or of the goods housed in them, is impracticable and resort has to be had to an insecticidal spray. In the course of experimental work in the use of an atomised pyrethrum-white-oil spray for the control of the moth *Plodia interpunctella* in warehouses it was found that on falling the spray formed a fine film on exposed surfaces and that moths and caterpillars alighting or crawling on this film became moribund and died. Further investigation of this spray by Potter [21] showed that a film of insecticide of about 0·0053 g./sq. in. is lethal to the full-grown larvæ of *Ephestia elutella* and *Plodia interpunctella* and that a film of this weight can, under diffused light, retain its toxicity for periods up to 20 days.

When these results were first announced, they were received with considerable scepticism, first because the value of the method was doubted, but chiefly because pyrethrum rapidly loses its toxicity when exposed to the air. The prolonged toxicity of the pyrethrum in this spray has not yet been explained, and it may be that the explanation can come only through investigations in the more recondite fields of colloid chemistry and surface physics.

The use of this pyrethrum-white-oil spray to produce a "residual toxic film" has, however, led to a great extension of the use of this method in many fields and particularly in the control of mosquitoes and other flies. The insecticide D.D.T., because of its persistent toxicity, is especially suited for this form of application. To the general public, perhaps, D.D.T. appears the startling discovery, but scientifically the discovery of the toxicity to insects of the para para isomer of dichloro-diphenyl-trichloroethane is much less important than the advances made in our knowledge of the application of insecticides. One of the chief of these advances is Potter's discovery of the "residual film."

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THIS SUBJECT OF GRANITE

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A CENTURY and a half ago James Hutton, the founder of geology as a science, confessed himself to have been "particularly anxious about this subject of granite." By the power of field-evidence he had made a decisive intervention in the classic debate between the Neptunists and the Plutonists. His demonstration of tongues of granite clearly interjected into limestones in Glen Tilt showed that granite could not be a chemical precipitate, as the Neptunists proposed, but had solidified from a melt. Granite was thus an igneous rock. Hutton settled the problem of the granite veins of Glen Tilt and probably settled it for all time, but he did not settle the problem of all granites. Since his time, a great many geologists have likewise been anxious about this subject of granite, and it may truly be said that to-day the origin of granite is the most lively problem in geology. The conflict of opinion is startling as can be illustrated by the following: in 1942–44, Niggli of Zürich and the writer of this article independently considered the subject and gave their findings in dissertations of considerable length—their findings were diametrically opposed though they were based upon the same corpus of apparent facts. These findings represent not the opinions of two particularly pugnacious individuals, but the tenets of two powerful and active schools. There is no cause here for tears, since if the protagonists keep their tempers they will learn a great deal good for them. It is proposed to set out the main lines of the debate.

First has to be decided what is meant by the term granite. There are a great number of technical definitions available and all of them include the word igneous. If they are used then the question at issue has been already settled, for the modern debate is largely concerned with whether granite is or is not an igneous rock—that is, whether it has or has not consolidated from a melt. If we admit a doubtful genetic control we fetter ourselves without advantage. It is accordingly best to base our definition on properties determinable in a lump of granite which could just as well have been acquired by purchase or gift as by field-work. If this is admitted then we cannot do better than adopt the *New English Dictionary* definition: "granite is a granular crystalline rock, consisting essentially of quartz, orthoclase-felspar and mica, much used in building." From inspection of the specimen, there could be added to this that the grain-size is large enough for the individual minerals to be recognised and, from microscopic observation, that the constituent crystals had interfered with one another's free development during their growth. This definition can be properly based on the loose specimen of granite. We have to enquire into the proposals for producing such a rock.

The Neptunists, headed by Werner of Freiberg, had decided that granite was a chemical precipitate; Hutton had shown that granite was an igneous rock crystallised from molten rock-substance. But even so, at no time have all granites been accepted as igneous. Thus Lyell proposed early in the last century that granite was produced by the transmutation of rock in depth through the agency of fluids acting under high temperatures and at great pressures—this is the celebrated metamorphic theory. At the same time there were developing in France those ideas concerning granitisation and felspathisation that have come to be the characteristic glory of French geology. Basing their conclusions on the investigation of many granite-contacts in Brittany and Normandy, Central France and the Pyrenees, an illustrious line of French geologists—Fournet and Élie de Beaumont among the earlier, Michel-Lévy and Lacroix among the later—has provided a body of contributions fundamental for the discussion of this subject of granite. We may glance at these in the merest summary.

The French emphasised that there are many kinds of granite contacts; in some the country-rocks are sharply delimited from the granite, in others the country-rocks gradually pass into granite through a transitional zone of metamorphic and felspathised rocks. In this latter type, abundant transfer of material from the granitic zone into the metamorphic zone could be demonstrated. They correlated style of contact with depth of granite formation, clean and definite in the upper levels, hazy in the lower. They related this to the operation in depth of a granitisation-process involving either an intensification of metamorphism or the activity of a granitic melt which itself produces the metamorphism. In either operation, they invoked the action of energetic fluids and gases—mineralisers, volatiles or emanations—that made the country-rock as it stood over into granite or prepared it for an advance of the granitic melt. They envisaged conditions under which metamorphic rocks could become igneous rocks. It will help in what follows if we define the process of granitisation mentioned above: it is that by which solid rocks are converted to rocks of granitic character without passing through a liquid stage.

But whilst the French school was producing these brightest ornaments of its genius, an authoritative, and eventually authoritarian, creed was being promulgated in Germany. Hére Rosenbusch had shown that at a small granite contact in the Vosges, no transfer had taken place and he extended this conclusion to all granite contacts. He interpreted the felspathised and granitised zones observed by the French as due to dynamic metamorphism—granites had been sheared during episodes of crustal folding. Rosenbusch was one of the pioneers of microscopic petrography and attracted students from all countries. This circumstance, helped by his authority and perhaps aided by the Teutonic fashion coming with the Prince Consort, led to the adoption of Rosenbusch's views in Britain and America; granites were igneous rocks and granitisation did not occur—these were views that filled the French with astonishment, to say the least of it. Franco-Prussian wars have been fought on many fronts.

The general problem that divided the French and Germans still remains unresolved though the particular emphasis may have changed. A division of opinion still separates those geologists who consider that granites are always igneous, consolidating from an intrusive melt which has displaced a certain volume of country-rock, and those who consider that granites are formed by the replacement of country-rocks through granitisation. It has come to be displacement versus replacement, magmatic granites versus non-magmatic. The magmatic interpretation necessitates a granitic magma and we may now examine how it is proposed such a magma might arise.

Magma is completely fluid rock-material; on consolidation magma gives rise to the igneous rocks—these are statements acceptable to all. But when we consider how granitic magma might be made we encounter a diversity of opinion even among the magmatists. Some favour the classic view of Durocher and Bunsen that there are two independent primordial magmas, one providing the granitic rocks, the other the basaltic. The actual occurrence of igneous rocks in the crust gives support to this view —the commonest rocks are granite and basalt, other types being relatively rare. Further, it is reasonable to derive these two contrasted magmas from the two outer earth-shells postulated by earthquake-investigation. The granitic rocks, confined to the continental masses, come from the discontinuous sial layer, the basaltic, occurring in continental and oceanic areas alike, come from the continuous world-wide simatic layer. Two shells, two magmas seems sound.

Geophysical considerations suggest that the sial and sima are solid, so that the granitic magma has to be produced by melting, pure or otherwise. From the many proposals to bring about pure melting we may select two—the one depressing the sial into the hot sima, the other raising the hot sima into the sial. In the fold-belts of the crust great piles of sediments are involved that were deposited in geosynclinal depressions—these depressions localise the folding and hence further depression and thickening of the cover. According to Anderson geophysical evidence is not opposed to the suggestion that the cover may become thick enough to cause the sial to melt and the resulting granitic magma to rise up into the fold-roots. It is an observed fact that the greatest granitic bodies are situated in the fold-belts, are elongated along them and came into place soon after the paroxysm that caused them. On the other hand, hot basaltic magma, produced by the melting of the sima, rising through the overlying sial might melt this and provide a small amount of granitic magma. Certainly, there is a puzzling association of granitic and basaltic rocks in many igneous complexes that might be explained by this mechanism.

Pure melting has many difficulties; partial melting or partial solution is considered to have fewer. Many geologists (especially Eskola of Helsinki) who are familiar with the oldest portions of the crust have suggested that in the deep zones the more fusible constituents such as quartz and felspar may melt in sufficient quantities to provide bodies of granitic magma when the sponge is squeezed during fold-movements. This partial fusion may be assisted by solution brought about by the activity of energetic fluids; but here we meet a topic that is best considered later in this account.

During the last twenty years or so the classic two-magma view has become replaced among the magmatists by a one-magma view—and the magma in view is not granitic. The gigantic bodies of basaltic rock, dominantly lava-flows, found in all parts of the crust show a very close similarity in composition. From this circumstance it is concluded that vast quantities of basaltic magma have been at all times available by melting of the sima. Basaltic magma is thus a magma primary in its own right. On the orthodox modern view granitic magma is simply the residue resulting from the separation of crystals from the primary basaltic magma. The first crystals to form in such a magma would be the heavy iron-magnesium silicates, olivine and pyroxene; their crystallisation leaves the residual melt enriched in silica and alkalies, oxides not required in great abundance for olivine or pyroxene but the characteristic oxides of the granitic rocks. If therefore at some suitable stage the early crystals could be removed from the melt, a granitic magma would become available. This separation could result from sinking of the heavy crystals or by the squeezing-out of the fluid residuum from a mesh of them, either through pressures during folding or the weight of the mesh of crystals. By tapping the crystallising magma at appropriate times there could be drawn off partial magmas of diverse composition that are held to provide the variety actually seen in the igneous rocks. We must here confine ourselves to the question of the production of the extreme term, the granitic magma.

Most geologists would readily admit that granitic magma could arise in any required quantity by direct melting of the granitic sial. To some, on the other hand, it seems a difficult matter to produce a magma of the requisite volume and composition by crystal-fractionation of the primary basaltic magma. Their criticisms of this process concern the quality and quantity of the product and the mechanism by which it is separated from the crystal-mesh. They contend that the crystal-fractionation would result in residual liquids rich in iron and point to the celebrated Skaergaard intrusion in Greenland investigated by Wager and Deer as giving an excellent demonstration of this in the field. Supposing, all the same, that a granitic residuum is produced, then its volume must be less than one-tenth of that of the basaltic magma from which it has come—fractions nearer the basaltic parent bulking more greatly. But we have already noted that granitic rocks are exceedingly abundant in the crust; to produce them by this means would require a stupendous volume of primary basaltic magma. Lastly, the critics feel that the expulsion of the small quantity of granitic residual magma from the crystal-mesh would be an operation of such a kind and of such a magnitude that it should be capable of geological observation. There is, however, no field-evidence pointing to the action of the necessary stresses on the basaltic masses. These are formidable objections to the derivation of granitic magma from a basaltic parent by crystal-fractionation; if granitic magma exists it must be produced in some other way.

If granitic magma exists—that may seem a senseless qualification in the face of Hutton's decisive demonstration of intrusive veins in Glen Tilt. But size does enter into this problem and especially with regard to the mechanism of the emplacement of magma, whatever its composition may be. The emplacement of a narrow vein or of a small body or even of a large sheet may be readily accounted for by structural adjustments in the crust; the emplacement of tens of thousands of cubic miles of magma in a fold-belt is an operation of another order. It is this room-problem that lies at the heart of this subject of granite. With it we have to deal with those fascinating bodies, the granitic batholiths—we have to discover how granites occur in the crust.

As their name suggests, batholiths are rocks of the depths—they are revealed to us only after prolonged erosion has removed their roofs of country-rocks. Mining operations disclose that they enlarge themselves downwards, and no floor to them has yet been discovered. They are situated in the fold-belts of the crust and are elongated along them. They are overwhelmingly made of rocks of the granitic kindred. Finally, and most vitally for any discussion of the granite problem, they are of gigantic size—a circumstance that controls the whole debate. For instance, the British Columbia batholith is some 1500 miles long by 100 miles wide, a batholith in Patagonia is 800 miles long and another in Montana has an area of 16,000 square miles. Multiply these surface dimensions by a third dimension even only of a few miles and we have truly colossal volumes of granitic rock to consider. The room-problem becomes a very big problem indeed.

Let us agree for a moment with the magmatists that granitic magma is available in the crust. The emplacement of small volumes of this magma might be explained as a result of structural readjustments in the crustal rocks. Room might be made for the magma by doming of its roof, or by collapse of a segment of the crust along straight or circular fractures; the emplacement of the granite masses of North Arran, Cruachan and the Mourne Mountains, for example, has been explained along these lines. But these masses are special, small in size and not truly batholithic in character. It is true that gigantic bodies of basic rocks such as those of Sudbury (Ontario) and the Bushveld in South Africa were intruded as sheets but the granite batholiths are not of sheet form, they have no floors and they appear to replace, not to displace, the tightly-folded country-rocks. In the upper parts of batholiths, blocks of the roof are often seen and it has been suggested accordingly that the magma has risen by wedging off portions of its roof by an operation similar to what the miner calls stoping. These stoped blocks are considered to have sunk out of view through the magma. It is questionable, however, whether blocks of ordinary country-rock would sink. Moreover, if they did sink they should soon become a part of the magma and should so contaminate later upsurgings that their former presence should have been detected. It can, in fact, be demonstrated in many granite batholiths that the structural pattern of the surrounding country-rock can still be recognised in the arrangement of the blocks enclosed in the granite; they have not changed position.

It appears to many geologists, therefore, that the emplacement of great volumes of granitic magma cannot be accounted for by structural or other mechanical adjustments. No mechanism has so far been proposed by the magmatists to solve the room-problem. We can, if we are made that way, ignore the problem, or we can quite legitimately maintain that on geological grounds vast bodies of granitic magma must have intruded the crust, though we cannot suggest how they did it. But the non-magmatists believe that the problem can be solved by proposing that in fact no gigantic volumes of granitic magma are involved in making the batholiths. These great bodies are the products of granitisation, the country-rocks being made into granite on the spot by piecemeal replacement; granites are only incidentally igneous. We have now to consider whether the granitisation proposal has any reputable body of evidence behind it.

As we have already noted, the French have maintained for more than a century that granitisation and felspathisation were demonstrable at their granite contacts. For the most part, however, they considered that a granitic magma was involved—its intrusion was preceded by a cortège of emanations that transformed the wall-rocks into granite and so made possible their assimilation by the advancing magma. Such a process is only in part granitisation. A somewhat similar position was taken up by Sederholm, the Finnish master, as the result of his early studies of the Fennoskandian basement. There granitic magma was considered to mix in varying degrees of intimacy with the country-rocks to produce migmatites, mixed rocks. Later, Sederholm came to place more emphasis on granitic juices or ichors as the active agents in migmatisation, but still granitic magma was somewhere below as the source of these. Under the influence either of ichors or of magma, the country-rocks could be melted or dissolved to produce a new-born magma that Sederholm called anatectic. But thorough-going granitisation in the modern sense does not appear to have been countenanced either by the classic French school or by Sederholm.

But modern Fennoskandian geologists, working in some cases over Sederholm's own ground, are among the foremost advocates of granitisation. They have come to the conclusion that many of the Finnish granites—bulking very large and red on the geological maps—are the result of soaking of country-rock and its transformation by metasomatism into granite. Granitisation has now become the most important process in migmatisation. It is brought about by the activities of emanations—to use a nice non-committal term—coming up from below and moving through the rocks, leaving them changed into granite behind them. Wegmann in particular has stressed this proposal of fronts of different qualities moving at different speeds with different results through the framework of the diverse country-rocks. The process is helped by an intergranular film or pore-liquid and it is considered capable of transforming vast volumes of country-rocks, none of which at any time need be in a magmatic condition. Granites may be, on this interpretation, completely non-magmatic. It is as well, then, to adhere to the definition already quoted from the *New English Dictionary*.

Granitisation requires little or no magma and would provide a complete solution of the fundamental room-problem. It is essential, therefore, to endeavour to estimate the validity of the field-evidence on which it is based, for in a geological discussion like this the field-evidence is decisive.

The evidence comes from all over the world and is provided by geologists of almost every land. Dozens of sections have been mapped and described that indicate without reasonable doubt a gradual passage from country-rock to granite through an intermediate zone of migmatisation and felspathisation. Dipping our hand into the bag, as it were, we bring out among recent work that of Goldschmidt in Norway, Anderson in California and Nevada, Wegmann in Greenland, Barth in New York, Fernando in the Shetlands, Cheng in North Sutherland, Read in Central Sutherland and Aberdeenshire, Doris Reynolds in Ireland, MacGregor in Galloway, Wilson in Yugoslavia—and we could add dozens more. It may be objected—and it often is—that such transitions are best interpreted as contacts between magmatic granites and unaltered country-rocks with a relatively unimportant felspathised belt between. It is here that one remarkable feature of these contacts comes into prominence; large and identical felspars occur in country-rock and granite alike—there is no possible doubt about this. We look into it more closely.

The large felspars of granite are considered by the magmatists to be formed by crystallisation from the granitic magma; some of these felspars are peculiar and complex and if this is their manner of formation then their crystallisation-history has been a complicated one. But exactly similar felspars are found in the country-rock, this being in many cases of sedimentary origin ; these felspars have undoubtedly grown in the solid country-rock. If identical complex felspars can be formed in two such different environments, then the application of physical chemistry to rock-formation becomes meaningless. It is clear that there can be only one environment, the one that can be proved, that of the solid country-rock.

Now return to the exposures showing transitions from country-rock to granite ; on the country-rock side, the granitisation-process begins by the growth of large felspars and other less conspicuous components of granite, and proceeds with increasing intensity till, passing through the migmatite and felspathised zone, the granite itself is reached. This was all said a century ago by Lyell and the French. It can be demonstrated in any city opulent enough to use large polished slabs of granite in its buildings.

Some granite masses are admittedly eruptive ; they break the continuity of the rocks, their appearance is sudden and we meet them without warning—to paraphrase a statement of de Lapparent's on eruptivity. Sharp and discordant contacts could arise in granitisation by, for example, a halt in the movement of the migmatite front or through the circumstances of temperature pressure or concentration governing the reactions. But there appears to be no reason why the granitisation-products should not become able to flow or to move ; the fashionable term for this process is mobilisation—a word which should keep its ordinary English meaning of making movable. Mobilisation may be brought about by loss of cohesion between the grains of the rock through increase in the amount of intergranular fluid, either indigenous or introduced by soaking, and is assisted by increased temperature or pressure and the arrival of fresh energetic emanations. The result may be a mixture of relic grains in a fluid base—a mixture that has been given the name of migma. When migma consolidates it produces some of the migmatites. If the process of making migma goes so far as to liquefy all the relic grains, then clearly a magma has been formed and this, on solidification, will naturally give an igneous rock. At any stage of the migma-making, the sponge may be squeezed by folding pressures in the crust leading either to eruptivity of the migma or to the expulsion of the liquid portion to collect as another sample of magma. The foregoing illustrates what the writer considers to be the correct use of migma and magma ; from this the true significance of the adjective igneous can be assessed.

There is on this view a unity in the granites, whether granitisation-products or migmatites or magmatites. This unity was implicit in the views of Lyell and the French school; one aspect of it is expressed in the statement of Termier, for example, that where we find a granite surrounded by zones of migmatisation and metamorphism we are dealing with a granite made in place and that where a granite is sharply defined from its country-rock it has arrived ready made from lower levels. This genetic connection between schists, gneisses and granites lies at the heart, the writer believes, of regional metamorphism and may now be briefly considered.

When the great terranes of the regionally metamorphosed rocks are traversed it is found that differences of metamorphic grade are revealed by the presence of different minerals in harmony with the different environments. For example, a series of metamorphic zones marked by the occurrence in order of the index-minerals chlorite, biotite, garnet, kyanite and sillimanite, has been mapped in the South-East Highlands of Scotland. Rocks of the highest-grade zone, that of sillimanite, are associated with granites in a central area of migmatisation around which the lower-grade zones are arranged as an aureole. This pattern can be reproduced from all the great metamorphic areas of the world. It is evident thereby that granitisation, migmatisation and regional metamorphism are genetically related. This relation becomes interpreted when the doctrine of fronts is considered. The process of granitisation taking place in the kernel leads to the expulsion of waves of metasomatism which, passing through the surrounding country-rocks under changing chemical and physical controls, promote the formation of the series of metamorphic zones about it. Regional metamorphism, therefore, has little to do with stress or folding or deformation—it is largely an affair of permeation by metasomatising solutions. It is only by this kind of operation that the delicate original textures of the sediments, for example, can be preserved when the rocks are thoroughly recrystallised.

The schists and the gneisses and the central granites thus make up a natural group of rocks, though the three components are separated in most present-day schemes of classification. When we look into this natural group we find it to be none other than Lyell's *Primary Rocks*, formed according to him by plutonic action in the depths of the crust. To quote from the master: "to this plutonic action the fusion of granite itself in the bowels of the earth as well as the development of the metamorphic texture in sedimentary strata may be attributed." Any modern rock-classification that divides up a natural association of rocks may have its uses to some but may be discarded with few regrets by seekers after a genetic classification. The writer has therefore returned to Lyell and proposed that there is a *plutonic* class of rocks, comprising the granitic and the metamorphic, formed in connection with crustal compression in the fold-belts where the sialic layer may become activated.

Having taken the first step down the slippery slope of genetic rock-classification, the writer deemed it necessary to finish the job. The French have always more or less explicitly separated the granitic-plutonic rocks from the basaltic-volcanic, and the same separation is apparent in the early tenets of Lyell. This distinction has been lost sight of in Britain and America where the German igneous blanket covered all. Recently Kennedy has re-established the fundamental difference between the Plutonic class (as defined above) and the Volcanic association. The *Volcanic* class is dominantly basaltic in composition, truly magmatic, mainly effusive and associated with crustal tension. Its basaltic magma is derived in large draughts directly from the simatic layer.

The third class of rocks is the sediments ; these are dominantly marine and the writer has been impetuous enough to propose that they be called *Neptunic.*

The new rock classification—or the refurbished old classification of Lyell—is then Neptunic, Volcanic, Plutonic. Into each of these divisions go naturally large numbers of the common rocks of the crust, the sediments into the Neptunic, the basalts, dolerites and gabbros into the Volcanic, the metamorphic rocks and granites into the Plutonic. There remain admittedly important rocks whose position is not yet clearly established, but this is a task to which the writer feels we may confidently address ourselves now that we are free from the igneous fetters. Rocks have ceased to be merely nicely-trimmed specimens in museum-cabinets ; they have become once again parts of the crust recording in themselves fundamentally different styles of earth-history. This reorientation becomes possible so soon as we consider seriously the room-problem that constitutes this subject of granite.

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RECENT PROGRESS IN INDUSTRIAL MYCOLOGY

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Industrial mycology is concerned with the activities of the organisms commonly known as "moulds." The term is a useful, but unscientific, means of denoting the micro-fungi which are found contaminating industrial raw materials and manufactured products. The moulds do not constitute a natural group in the mycologist's classification but include species belonging to three out of the four classes of fungi, Phycomycetes, Ascomycetes and Fungi Imperfecti. Very many of them grow on a wide range of substrata and, owing to their ability to flourish at temperatures varying from freezing-point to about 40° C., are found in all parts of the world.

The activities of moulds may be classified as harmful and beneficial. On the one hand, they cause an enormous amount of damage to almost every type of material which is of organic origin, the task of the industrial mycologist in this field being to develop means of protection which will prevent the damage as far as possible. On the other hand, a number of moulds, when grown under suitable conditions, can synthesise, from comparatively simple and readily available carbon compounds, useful substances which the organic chemist cannot manufacture economically. During the war years advances have been made in both branches of industrial mycology, in our knowledge of the best means of combating the harmful activities of moulds, and in useful applications of mould metabolism. In addition, no review would be complete without some mention of a number of recent publications, on the taxonomy of fungi, which are of great importance to all students of moulds.

The Prevention of Mould Growth

Our better understanding of methods of control are due, in large measure, to the war in the tropical regions of the Far East. The majority of moulds grow most readily when the temperature is moderately high, 75–85° F. (24–30° C.), and when the relative humidity of the air is very high. Such conditions obtain only occasionally in this country, but in tropical jungle the relative humidity may be close to 100 per cent. for considerable periods, with little variation between night and day. It is not surprising, therefore, that, during the war, the problem of maintaining military stores and equipment in good condition was a very serious one. In reality the problem was by no means a new one, but merely came into prominence during the war, owing to the enormous amounts of materials involved, and the necessity to economise shipping to the utmost. Before the war much of the loss, in tropical countries, caused by moulds was taken for granted and, moreover, there was no co-ordinating body, like the Ministry of Supply, to initiate research which would benefit many different industries.

Packing.—To a large extent the methods of protection of stores during the war were based on well-known principles. In theory the simplest method of control of mould growth is to limit the relative humidity of the air in contact with the goods to a figure not exceeding 70 per cent. For goods which are to be shipped to, and stored in, a region of high atmospheric humidity, this involves packing previously dried material in hermetically sealed containers. Questions of cost and, more important during the war, shortage of metals, limited the range of equipment which could be safeguarded in this way. For photographic materials and certain medical supplies there was no alternative, but for machinery, fabrics, leather goods, optical instruments, and electronic equipment other methods had to be found. Packing materials had, of course, to be in themselves highly resistant to attack by moulds. In addition they were selected not only to give adequate mechanical protection but to retard access of moisture-laden air. For delicate machinery and instruments it was found that really well-made wooden cases provide a good first-line barrier, since water vapour diffuses only very slowly through properly constructed joints. Waterproof crêpe paper, either as a lining to the cases or as wrappings for the contents, still further retards ingress of moisture, often to such an extent that a drying agent such as silica gel, inside the package, can deal with all the moisture which penetrates over a comparatively long period. For soft goods, such as clothing and bedding, existing methods of high-pressure baling were adopted during the war with little modification, the only real improvements being the use of mould-proofed papers and of more highly waterproofed crêpe paper and hessian for the final coverings.

Mould-proof Materials.—As a result of thousands of tests our knowledge of the innate mould resistance of a large variety of materials has greatly increased. Much of the work was carried out with a view to rendering wireless sets, and other electronic equipment, mould-proof under field conditions, it being found that the only really satisfactory method of proofing equipment of this type is to use in their construction only the most highly resistant materials available. Materials with good insulating properties, and which at the same time are not appreciably attacked by moulds even under the worst conditions, are vulcanised rubber and a number of plastics, particularly polyvinyl chloride (P.V.C.), polythene, and most of the high temperature phenolic plastics. With the latter, care has to be taken that the fillers, incorporated to increase mechanical strength, have adequate mould resistance. If unsuitable fillers are used mould may grow inwards from cut edges and imperfections in the surface and may even cause complete disintegration.

The high resistance of vulcanised rubber has been made use of in another connection. Bonded cork is largely used for gaskets, but the ordinary varieties, bonded with glue or casein, are rapidly broken down to a crumbly mass when attacked by moulds. Cork itself is not mould-proof but is not appreciably decomposed, the failure of the bonded material being due to breakdown of the adhesive. Rubber-bonded gaskets develop a thin superficial growth of fungus but retain their strength and flexibility for a long time.

The resistance of pure cellulose is not a new discovery, but it took a long time, even when the problem of supplying mould-proof equipment was urgent, to get the knowledge properly utilised. Many moulds do not attack cellulose at all, and those which are able to digest it do so only when certain other nutrients are present as impurities. In practice it is found that cotton, boiled for at least an hour with 1 per cent. soda ash and then well washed, has a very high resistance to attack by fungi. The process does not produce pure cellulose but does remove most of the inorganic matter, soluble carbohydrate, and fat, leaving only traces of material which can be utilised by moulds. If cotton is first treated in this way the problem of making it completely mould-proof is very much simplified.

Antiseptics.—During recent years there has been an intensive search for new antiseptics, to meet the many and varied demands for mould-proof materials. The most successful of the introductions in the decade before the war were undoubtedly salicylanilide (Shirlan), developed by The British Cotton Industry Research Association primarily for use in fabrics, and the organic mercurials, used as seed dressings for the control of parasitic fungi.

The organic mercurials exhibit amazing potency in laboratory tests, but attempts to extend their use to the protection of a wide range of materials have met with very limited success. Various claims have been put forward regarding the value of the phenyl mercuric compounds as antiseptics for textiles, but, unfortunately, they are decomposed when processed fabrics or yarns are dried over heated metal cylinders, and hence have very limited application in this field. They have, however, been used successfully in paints and varnishes.

Salicylanilide has been tested far more severely than it was before the war and has proved its value. It has one useful characteristic which is not shared by a number of other common antiseptics. Some substances are effective in suppressing mould growth at moderately high humidities, but very rapidly decrease in efficiency as the relative humidity approaches 100 per cent. Shirlan, although developed originally for the protection of cotton goods under conditions less severe than those of tropical jungle, has been found to be fully effective at 100 per cent. R.H. The quantity used has, of course, to be greater than that required for less stringent conditions. The actual amount required is 0·07–0·1 per cent. of the weight of the fabric or other material, whereas the original recommendation of the B.C.I.R.A. was 0·04 per cent. An interesting result of the many tests carried out during the war is that this substance is effective only in hygroscopic materials such as cellulose. Its antiseptic value is very low when it is incorporated into non-aqueous media such as oils, paints, and varnishes.

During the war the demand for Shirlan exceeded the supply and substitutes had to be sought. The best of these, found to be quite equal to Shirlan in antiseptic potency, is mercaptobenzthiazole, a substance commonly used as an accelerator in the vulcanisation of rubber. It has long been known that a mixture of two or more antiseptics is more efficient, weight for weight, than any one of them used alone. It is not surprising, therefore, that the best results on certain types of military equipment were obtained with a mixture of salicylanilide and mercaptobenzthiazole.

For the protection of fabrics which have to withstand exposure to the weather nothing really new has been evolved. Neither Shirlan nor mercaptobenzthiazole is of use in this connection since both are rapidly leached out by rain. The old process of dyeing with mineral khaki (chromic oxide plus ferric oxide), provided that soluble salts, and in particular compounds of hexavalent chromium, are removed after impregnation, gives an excellent degree of resistance. The real antiseptic is chromic oxide, and it has been found that this can be used alone, when it gives only a pale greenish shade on which other colours can be dyed. For very heavy materials, such as tarpaulins, and for cordage, copper naphthenate has been widely used.

Volatile Antiseptics.—Volatility is one of the least desirable properties of an antiseptic to be used for the protection of textiles, paper, wood, leather, etc., since exposure to air would result in gradual evaporation. During the war, however, there arose several problems for the solution of which volatile antiseptics appeared to be promising. The first experiments were carried out with wireless sets, for, although methods were fairly soon suggested for making new sets mould-proof, there were an enormous number of sets already in use which needed protection, and the presence of a quantity of volatile antiseptic inside any set which is reasonably airtight should, in theory, prevent mould growth. Later, experiments were extended with a view to preventing growth inside optical instruments, such as binoculars, telescopes and range-finders.

The method of test used for the preliminary experiments was crude but gave interesting and valuable results. More elaborate experiments, designed to determine the limiting concentrations of various antiseptics in moisture-saturated air which would suppress mould growth, were started just before the end of the war and have not yet been completed. The method used was to place within a large glass vessel, nearly but not quite airtight, a dish containing a small quantity of the substance to be tested, an open Petri dish containing culture medium, several pieces of material which were known to be readily attacked by moulds, and a large dish of water to maintain a saturated atmosphere. The contents of the vessel were sprayed with a water suspension of mould spores before incubation. Tests were carried out at various temperatures, 24°, 30°, and 37° C., so as to get a rough idea of the volatility of the substance. Any substance which completely inhibited growth in these experiments was further tested for its action on insulating materials and on the various metals used in the construction of wireless sets and optical instruments.

The alkyl organic mercurials, such as ethyl mercuric chloride and ethoxyethyl mercuric chloride, completely suppress all mould growth under the conditions of test. At the same time they volatilise only slowly, suggesting that fairly small quantities could be used to give reasonably lasting protection to instruments which were not completely airtight. Unfortunately, these substances attack many metals in presence of moist air and, in addition, are unpleasant to handle.

Camphor and terpineol are both very effective in suppressing growth, but are too volatile to permit of their use in any apparatus which is not hermetically sealed.

"Cresatin" (metacresol acetate) was found by American workers to be an excellent volatile antiseptic. It has a sufficiently low rate of volatilisation, which can be further controlled by incorporating the substance, in various proportions, in ethyl cellulose. However, it tends to hydrolyse to some extent in moist air and, in consequence, attacks a number of metals.

Thymol is the most promising of all the materials tested. It is highly toxic to moulds, it volatilises only slowly, it has an agreeable smell and is pleasant to handle, and, being quite neutral, it does not attack metals.

Unfortunately, all the effective substances are absorbed, to varying degrees, by other organic materials. In wireless sets they tend to swell rubber and to soften paints and varnished sleevings. In optical instruments they are absorbed by, and soften, canada balsam, and hence cannot be used in certain types of instruments containing balsam-cemented components which are freely exposed to the vapour. Nevertheless, it is probable that, when we know more about the minimum effective concentrations of some of these substances, it will be possible to use them in such a way as to avoid the difficulties, and, in the meantime, the knowledge gained may have use in other connections.

Uses of Moulds

Moulds have been used for a considerable number of years in some industries, as, for example, in the manufacture of citric acid and gallic acid. Other commercial processes of great antiquity—bread-making, cheese manufacture, and the preparation of alcoholic liquors—are based on the activities of moulds or their near relations the yeasts. There have been no noteworthy advances, during recent years, in developing the utilisation of moulds along these lines. Instead mould biochemistry has taken a new turn in the search for antibiotics, that is, products of mould metabolism which will inhibit the growth of other organisms such as pathogenic bacteria and fungi.

The story of penicillin has often been told and will not be elaborated here. However, it is interesting to note that the publication of Fleming's original discovery, of the antibacterial properties of culture filtrates from *Penicillium notatum*, found an unresponsive world, and ten years elapsed before there was any serious attempt to apply the knowledge. Even then it is at least doubtful whether we should now be able to receive penicillin treatment whenever necessary if it had not been for the war. It is unlikely that the enormous sums of money, which have been expended in the development of methods of large-scale production, would have been forthcoming if the war had not altered, for the time being, our scale of values.

The difficulties encountered in the early attempts to manufacture penicillin, and the fact that this substance has no action on a number of pathogenic bacteria, naturally stimulated the search for other mould products with antibacterial properties, in particular for a product which can inhibit the causal organism of tuberculosis. Enormous numbers of species of moulds have been tested in various laboratories, both in this country and abroad, but, although quite a number of new antibiotics have been isolated, very few of them are at all promising from the point of view of practical application. Although some of the substances inhibit pathogens which are insensitive to penicillin, they are too toxic for general administration. The most promising of the new antibacterials is undoubtedly Streptomycin, and this is obtained, not from a mould, but from an Actinomycete, *Actinomyces griseus*. This substance inhibits *Mycobacterium tuberculosis* both *in vitro* and *in vivo*, and has given encouraging results in a number of clinical trials, but, in spite of all the experience gained in the manufacture of penicillin, it is at present difficult to produce and very costly. Recently Hollande, in France, has described an antibiotic obtained from one of the higher fungi, *Clitocybe candida*, which inhibits *M. tuberculosis* in laboratory animals, and the results of further tests are awaited with interest. The difficulty here is likely to be the collection of sufficient raw material, unless it proves possible to grow the fungus on artificial culture media.

Another of the new antibiotics is interesting from two points of view. Patulin, which is produced in varying yields by a number of species of *Penicillium* and *Aspergillus*, first came into prominence in 1943 as a possible cure for the common cold. Subsequent large-scale trials failed to corroborate the original findings and, at the present time, interest in the subject has completely subsided. Nevertheless, it is difficult to explain away the results of the first experiments and the matter might repay reinvestigation. Patulin, however, shows marked antibiotic activity in quite another direction. Van Luijk (1938) first showed that when *Penicillium expansum* is grown on synthetic culture media, the filtered culture fluid has very strong powers of inhibiting the growth of species of *Pythium*. Species of the latter genus are serious plant pathogens, attacking a variety of cultivated plants and especially causing damping-off disease of seedlings. Van Luijk found that his culture filtrates not only inhibited the growth of a number of species of *Pythium in vitro*, but, when added to soil which was heavily infected with *Pythium*, permitted seedlings to grow normally, whereas identical seedlings, planted in the same soil without the antibiotic, were rapidly wilted. Later experiments in this country (Anslow *et al.*, 1943) showed that the anti-fungal activity of culture filtrates from *P. expansum* can be explained on the basis of their content of patulin, since the pure substance completely inhibits the growth of species of *Pythium* at a concentration of 1 : 400,000. Pure patulin is fairly easy to prepare in quantity using *Penicillium patulum*, which gives a better yield than *P. expansum*, and there is a promising field for the investigation of its possibilities for the protection of plants on a commercial scale.

In 1932 an American worker, Weindling, published the first of a series of papers in which he showed that a common mould, *Trichoderma lignorum* (= *T. viride*), produces a substance which is very active in suppressing the growth of some parasitic fungi. The strain of *Trichoderma* which was used in early experiments was thought at first to be a species of *Gliocladium* and, in consequence, the crystalline substance which was isolated from culture filtrates was named " Gliotoxin." *T. viride* was found to become readily established in over-wet soil, in which the parasitic *Pythia* also flourish, and some success in the control of damping-off was achieved by this means. Incidentally, gliotoxin is active also against pathogenic bacteria, but is much too toxic to human beings to be of use in medicine. More recently Brian and McGowan (1945) have found that some strains of *T. viride* produce a second substance, which they named " Viridin " and which, although it has no effect on bacteria, is even more active than gliotoxin against parasitic fungi. Its great disadvantage is that it is very unstable.

The work just described has opened up a promising new field of research. It is probable that, just as a considerable number of species of moulds have been found to produce antibacterial substances, it will be found that many can synthesise anti-fungal agents. However, the discovery of a new antibacterial has to be followed up by an extended series of tests to determine its toxicity, its action on leucocytes, and its stability in the body, before there can be any question of using it in attempts to cure human diseases. Similarly, any new anti-fungal agents will have to be tested for their action on the plants to be protected. It is highly probable that, just as many of the antibacterials are toxic to animals, so some of the anti-fungal agents will prove to be toxic to plants.

TAXONOMY OF MOULDS

The importance of taxonomy in mycology can hardly be overstressed. In any work on fungi, whether it is concerned with plant diseases, with large-scale fermentations, or with purely academical studies in cytology, physiology, or biochemistry, it is necessary to have correct identifications of the fungi themselves. In the past a very large amount of effort has been wasted through failure to realise this. For example, hundreds of papers have been written on the physiology and biochemistry of *Penicillium glaucum*, and in each, all we know about the organism used is that it was one of some 150 species of *Penicillium*. We do know, however, that the nutritional requirements and synthetical abilities of the different species show very great variety. In consequence, most of the published work on *Penicillium glaucum* has little value at the present day.

There has been a long period during which many botanists (and mycology is usually considered to be a branch of botany) have regarded plant physiology and cytology as of paramount importance, and have considered that time spent in taxonomic studies is time wasted. At the present day there is a reawakening to a sense of the importance of systematic studies, but there is much leeway to make up, particularly in this country. The importance of taxonomy in mycology has been realised sooner in America and some of the European countries, with the result that most of the recent work on the subject has been published abroad.

Of great importance to many mycologists has been the issue in America, in 1944, of a new edition of Saccardo's *Sylloge Fungorum*, reproduced from the original by photolithography. This work, begun by the Italian mycologist P. A. Saccardo in 1882, aims at publishing descriptions, in Latin, of all known fungi. There are, up to date, 25 volumes, and it is expected that further volumes will be issued at intervals. Complete sets of the original edition are rare and, when obtainable, extremely costly, yet it is almost impossible to carry out a serious systematic study of any group of fungi without the aid of "Saccardo." The new edition, issued at less than one-tenth the cost of a set of the originals, has put this important work on the shelves of many workers who, previously, could consult it only at intervals, and then by courtesy of one of the few libraries which were fortunate enough to possess complete sets.

Another noteworthy and recent American publication is *A Manual of the Aspergilli*, by Thom and Raper. The two related genera *Aspergillus* and *Penicillium* are easily the most important in industrial mycology, and it is highly desirable that there should be adequate monographs to enable workers in industry to identify the species they encounter. The monograph on " The *Aspergilli*," published by Thom and Church in 1926, has been of the greatest value, but it has long been out of print and, furthermore, a fair amount of new knowledge concerning the *Aspergilli* has accumulated during the intervening years. The new book, which is a workers' manual and not a monograph in the true sense, has come opportunely at a time when interest in moulds is spreading rapidly. Not only does it describe all known species in such a way as to make identifications straightforward, if not easy, but it gives much information on the occurrence and importance of the various species in industry.

For many years now there has been in progress in Holland a detailed study of the yeasts and yeast-like fungi. A very large number of these organisms have been maintained in culture at Delft, forming part of the International culture collection known as the Centraalbureau voor Schimmelcultures. This collection has formed the basis of an investigation which has resulted in the publication of a monograph likely to remain for many years the standard work on the subject. The first part, by N. M. Stelling-Dekker, was issued in 1931, and dealt with the true sporing yeasts. The second part, describing the non-sporing yeasts, has appeared in two volumes, the first, by J. Lodder, in 1934, and the second, by H. A. Diddens and J. Lodder, in 1942. The final stages of the investigation were carried to completion and actually published under German occupation, in spite of the great difficulties of laboratory work, when materials for culture media were more than scarce and sterilisation was a serious problem.

In England no comparable monographs have been published, but several papers have appeared, chiefly from the staff of the Imperial Mycological Institute at Kew, which have simplified the taxonomy of a number of genera of moulds, in particular of many of the dark-coloured moulds which are important to the industrial mycologist as destroyers of cellulosic materials.

Meanwhile the British Mycological Society has become very much concerned about the lack of trained taxonomists. A special committee, appointed to enquire into possible means of remedying the lack, issued its report, " The Need for encouraging the Study of Systematic Mycology in England and Wales," at the end of

1944, and a second committee has just issued its report on "The Teaching of Mycology." Even if the suggestions in these two reports are adopted it will be several years before the position is materially improved, but at least the first step has been taken and there is hope that, with the present widespread and growing interest in fungi, something definite will be done in the near future to restore the prestige which Britain once had in the mycological world.

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COLOUR

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The scientific study of colour involves three distinct types of phenomena: (*a*) physical phenomena, including the emission, absorption and reflection of light; (*b*) physiological phenomena, including the histology of the retina and optic paths, the photochemistry of the visual substances and the electro-physiology of nerve currents; and (*c*) psychological phenomena, in which the qualities of the sensation have to be described and correlated with the physical stimulus. Inevitably, the conception of colour derived by the physicist is likely to differ from that of the physiologist, while the psychologist must be expected to have an outlook which differs radically from both the physical and the physiological points of view.

It is in consequence by no means easy to present an account of colour which is acceptable to all three branches of science; the problem of a consistent and agreed terminology is one of the more obvious difficulties which must arise. The situation is in fact made even more complex by the existence of many industries concerned with colour, for example, dyeing, painting, pottery, glass, printing and so on, some of which have their origin in crafts dating back to the earliest historic periods. These industries, too, have their own outlook, terminology and conception of colour which, from the scientific point of view, it would be advantageous to harmonise without at the same time eliminating the more picturesque qualities of the individual crafts. To most people, however, colour is probably of greatest interest because of its decorative and æsthetic value, and unless we can understand the approach of the artist to colour, we cannot claim to have a full comprehension of the subject. We should not, perhaps, demand that the artist should understand the physics and physiology of colour, nor can we expect that knowledge of what the artist does, or how he does it, will enable us to emulate him. On the other hand, it should be possible to demonstrate quite conclusively that there is no conflict of principle between the artist and the scientist, but that, on the contrary, the two points of view are complementary to one another.

THE LIGHT STIMULUS

The radiations which are capable of stimulating the eye are electro-magnetic radiations having wavelengths between the approximate limits of 0·40μ and 0·75μ. If sufficient energy were available, radiations of shorter or longer wavelengths than these values would become visible, but for most colorimetric purposes these are the effective limits. The term "light" should properly be restricted to those radiations which can stimulate the eye, and will be used here with that connotation.

Light is emitted through the excitation of the atoms and molecules of matter by some suitable agency, for example, by heat, by electric discharge through gases, by fluorescence, by chemical activation, etc. Where heat is used, as in the tungsten filament lamp, light is radiated over an extended range of frequencies, so that analysis of the light in a spectroscope reveals a continuous spectrum. In a gas discharge lamp, on the other hand, the light is normally radiated at a few, fairly widely separated, frequencies characteristic of the particular gas; analysis in a spectroscope then reveals a discontinuous or line spectrum.

The physical quality of a beam of light can be defined by its spectral composition, that is, by the relative energies of the different radiations through the visible spectrum. This description could be given in absolute energy units, although in nearly all colour problems we are concerned merely with relative values.

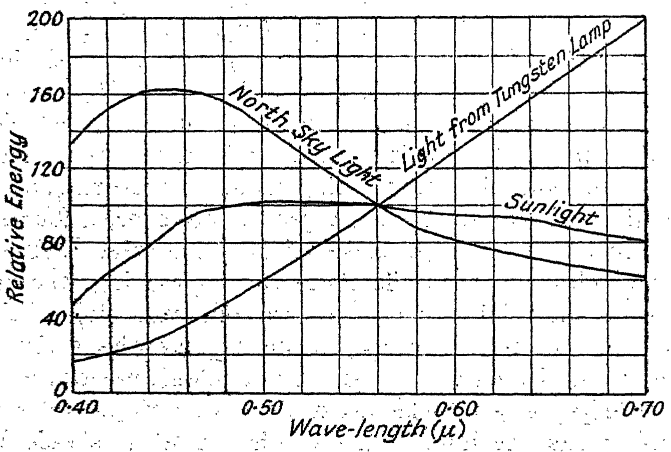

FIG. 1.—Curves showing the spectral composition of the "white" light radiated by a gas-filled tungsten lamp, the sun and the north sky.

Where the total amount of light is involved, it is usually more convenient to deal with it as a separate photometric problem. The spectral composition of typical "white" illuminants is illustrated in Fig. 1.

When light falls on a diffusing surface, *e.g.* a sheet of paper, a piece of felt, etc., the rays of light penetrate to a greater or less extent within the particles or fibres of which the surface is composed, but a considerable fraction of the incident light eventually emerges from the surface again, after suffering a number of partial reflections at the particle or fibre boundary surfaces. According to the size, shape and arrangement of the particles or fibres, their refractive index, the smoothness of the top surface and so on, so the surface will have a matt or glossy reflection. The relative magnitudes of the specular and diffuse components of the reflected light can be determined quantitatively by recording the polar reflection curves of the surface for various directions of the incident light.

The transparency of the particles or fibres will determine the whiteness of the surface. In the case of a surface made up of very transparent crystals, such as snow or magnesium oxide smoke, the surface is extremely white. If the particles are relatively transparent to some wavelengths, but absorb other wavelengths fairly heavily, the reflected light, and therefore the surface, will appear coloured. Again, the physical quality of the reflected light can be defined by its spectral composition, but since the composition will be a function not only of the absorption within the medium but also of the quality of the illuminant, it is usual to define the

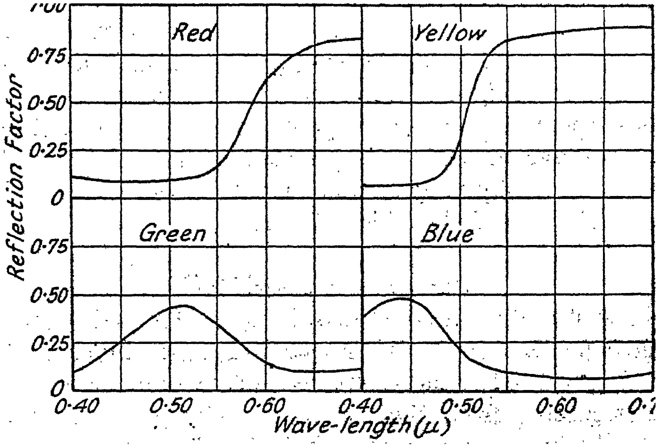

FIG. 2.—Typical spectral reflection curves for red, yellow, green and blue surfaces.

spectral characteristics of the surface in terms of a curve showing the percentage of the incident energy which the surface would reflect at each wavelength through the spectrum. Typical reflection curves are shown in Fig. 2. For any given illuminant, such as those represented in Fig. 1, the spectral composition of the light reflected from the surface can then be derived as the product of the energy curve of Fig. 1 and the reflection curve of Fig. 2.

The type of absorption within any medium is determined by the chemical constitution of the medium, but it has not yet proved generally possible to deduce theoretically the shape of the absorption curve from a knowledge of the atomic or molecular structure of the substance. When, however, the transmission or absorption curve of a solution is known for one concentration, the transmission curve for any other concentration can be calculated by the application of Beer's law. In the same way, the transmission curve for a mixture of solutions can be calculated from the curves of the component solutions, although the extension of the same process to the case of dyes or pigments applied to a surface is more complicated, since the presence of the surface may modify the type of absorption, and the random manner in which the light penetrates to various depths within the surface involves an indeterminate combination of additive and subtractive light mixture.

If sufficient measurements were taken, it would be possible to specify the exact physical pattern of the light entering the eye of an observer when looking at any given scene. The pattern in nearly every case is incredibly complicated, but it is important to recognise that the differences between, say, black, white and coloured surfaces, between opaque, transparent and translucent materials, or between glossy, matt or greasy finishes, originate in physical differences of light absorption, transmission and reflection. At the same time, lightness, colour, greasiness, translucence, etc., as we know them, are subjective perceptions which would not exist without an observer with an eye and a brain to record them, although we normally project them mentally outwards and regard them as attributes of the object itself.

THE VISUAL RESPONSE

The light stimulus radiated into the eye is focused by the cornea and crystalline lens on to the retina lining the inner wall of the eyeball. The retinal tissue is divided into a number of layers, the layer most remote from the cornea containing the light-sensitive elements, the rods and cones. Within these receptors, photochemical substances are secreted which decompose in the light and are re-formed in the dark. The products of the decomposition set up a train of nerve impulses which are transmitted across the retina via the neurones and synaptic processes which comprise the other retinal layers, until they reach the nerve fibres leading off to the optic nerve and thence to the brain.

The rods differ from the cones in their anatomical structure (although in the central part of the retina the cones become distinctly rod-like in shape), in their distribution over the retina, in their photo-chemical secretion and in their visual functions. The rods operate primarily at low intensities and are not generally believed to play any important part in colour perception; this is, however, still a matter of some dispute. The cones certainly operate at high intensities and play a very vital part in colour perception. The cones predominate in the central regions of the retina and at the central point itself, the fovea, there is an area subtending 1·5° to 2° which is entirely devoid of rods, at least in most eyes. This is the area where vision is most acute and colour discrimination is most sensitive.

The only photo-chemical substance which can be readily isolated from the retina and which has been studied extensively is visual purple. This is contained in the rods, and its spectral absorption (maximum absorption at wavelength $0{\cdot}50\mu$) corresponds very closely to the spectral sensitivity of the dark-adapted retina when recorded for a retinal area where rods predominate. No comparable information is available in regard to cone substances, presumably because their concentration is so small that they are very difficult to isolate. Since, however, colour perception necessarily involves some process of retinal reaction which must vary according to the wavelength of the stimulus, we are forced to conclude that within the cones there must be more than one type of photo-chemical substance secreted. If one substance were more sensitive to the long-wave radiations, while a second substance were maximally sensitive to the shorter wavelengths, then as the wavelength of the stimulus was changed, so the relative decompositions of the two substances would alter. We should then have a process capable of discriminating one wavelength from another.

No reliable photo-chemical evidence for such a state of affairs has so far been forthcoming, but Granit [1] has demonstrated by electro-physiological methods that the spectral sensitivity of one cone may differ very markedly from its neighbour. He has developed a micro-electrode technique by means of which he has recorded the energy required at each wavelength in the spectrum to produce a threshold nerve response from a single cone. The inverse of this energy plotted against wavelength then gives the spectral sensitivity of the cone. He has also extended the method by subjecting the retina to selective light adaptation before recording the nerve currents and has thus been able to isolate one type of cone more readily from another. Fig. 3 shows the average sensitivity curves obtained for three types of receptor in the retina of the cat.

The nerve currents transmitted to the brain consist of a sequence of nerve impulses which differ in magnitude from one fibre to another, but which have not so far been shown to differ in quality according to the wavelength of the light stimulus. On the assumption that all the nerve impulses are of the same kind, it follows that the response from the different types of cone can only be identified in the brain if there is some geographical correspondence between the distribution of the cones in the retina and the sensory processes in the brain.

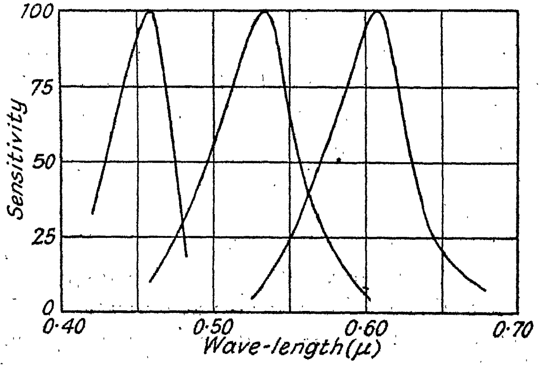

FIG. 3.—Spectral sensitivity curves for three types of receptor unit isolated in the retina of the cat (GRANIT).

THE VISUAL SENSATION

The end-product of the light stimulus and the visual response is the visual sensation. Sensations are the result of some unknown type of activity in the higher centres and since it is impossible to provide any sort of mental yardstick to measure them, the only knowledge we can obtain about our sensations is that derived from mental introspection. Further, any attempt to describe a sensation in words can only be made by references to objects and stimuli which give rise to the sensation concerned. Thus, if we wish to explain what we mean by "redness," we can only do so by saying that redness is the visual quality which, say, blood and a geranium and the radiation of wavelength $0{\cdot}65\mu$ have in common. Even then we have no means of knowing for certain that the sensation being experienced by observer A when looking at a pool of blood is the same as that experienced by observer B. Indeed, if A has normal colour vision while B has defective colour vision, the sensations will almost certainly be different, although there is no obvious way of discovering how they differ.

Bearing in mind this meaning which must be given to the terms describing our sensations, we can say that in normal colour vision, any colour sensation can be described in terms of the six primary sensations—red, yellow, green, blue, black and white. Any other colour has something of the quality of two or more of these primary colours; thus, most observers would agree that the sensation of brown has something of the qualities of redness, yellowness and blackness in its make-up.

The natural starting-point in any attempt to relate the physical quality of the light stimulus with the colour quality of the sensation is the spectrum, since the light radiations are here in their most elementary form. Under normal observing conditions we find that radiations from $0{\cdot}63\mu$ upwards are red, from $0{\cdot}59\mu$ to $0{\cdot}63\mu$ are orange, $0{\cdot}57\mu$ to $0{\cdot}59\mu$ yellow, $0{\cdot}55\mu$ to $0{\cdot}57\mu$ yellow-green, $0{\cdot}51\mu$ to $0{\cdot}55\mu$ green, $0{\cdot}48\mu$ to $0{\cdot}51\mu$ blue-green, $0{\cdot}45\mu$ to $0{\cdot}48\mu$ blue, and $0{\cdot}40\mu$ to $0{\cdot}45\mu$ violet. Since the transition from one hue to the next is gradual and somewhat ill-defined, these wavelength boundaries are to some extent a matter of opinion and should not therefore be regarded as exact limits.

When we consider a heterogeneous mixture of radiations such as would be reflected from the surfaces represented in Fig. 2, we should expect that where one part of the spectrum was more strongly represented than other regions, the hue would correspond to that of the predominating radiation. As a first approximation, such a conclusion would be reasonable, but it is in fact only a very rough approximation. Before we can obtain a more exact answer, we have to study the effect of mixing radiations from different parts of the spectrum, and to investigate the basic laws of colour mixture.

We cannot, however, assume that when we send a given light stimulus into the eye, we shall always arouse the same sensation in the brain. The linkage between the stimulus and the sensation is a physiological and not a physical linkage, and as such is subject to variations of sensitivity according to the conditions of the observation. In particular, the light sensitivity of the retina may vary over an enormous range according as the eye is light- or dark-adapted, corresponding to a low or high photo-chemical concentration in the rods and cones. Stimulation of one retinal area will also affect the sensitivity of the neighbouring areas, so that the yellowness of a primrose, for example, is enhanced by contrast with the surrounding green leaves. Moreover, the stimulus under investigation will also produce changes in retinal sensitivity during an actual observation, until a balance is reached between the rate of photo-chemical decomposition produced by the stimulus and the rate of regeneration determined by the chemistry of the retina. The retinal area being stimulated has then become locally adapted to the light, but the balance will immediately be disturbed when that area receives a different stimulus. In consequence, when we are looking at any normal scene and our eyes are roving across it from one area to another, the retina is in a constantly varying level of sensitivity.

It is perhaps surprising that we are not more aware of this instability, but the power of the eye to adjust its sensitivity to the prevailing level of illumination does in fact tend to produce a constancy in the appearance of non-luminous objects which is a most important property of the visual process. Thus, if we were in a room in which, by some suitable means, the illumination level were increased by a factor of 10, but if, as the eye became adapted to this higher level, its sensitivity were reduced to one-tenth of its previous value, then the appearance of the objects in the room would be unchanged.

The colour constancy of an object is also assisted if we recognise or know what the object is. The final quality of our visual sensations is not, in fact, a simple function of the activity within the visual centre of the brain, but is determined by the integrated pattern of activity within the whole of the cortex.

Even from this very brief outline of the situation, it becomes immediately evident that measurement of the physical stimulus cannot serve as a measurement of the sensation. Sensations can be equated, as in photometry, where the intensity of two lamps may be determined by making a brightness match between the two halves of a photometer field, or as in colorimetry, where the two halves of the field may be matched for both colour and brightness. In each case, however, the quantity recorded in the observation is the magnitude of the physical stimulus required to produce the visual equation; no measurement of the sensation is, or could be, made.

COLOUR MIXTURE PHENOMENA

The results of mixing light stimuli depend to an important extent on the size of the field and the area of the retina used in the experiment [2, 3]. As most colour-matching observations, however, have been made using the foveal area and a size of field of the order of 2° angular subtense, and as these observing conditions are undoubtedly the most important in their practical application, we will limit this discussion to experiments of this type. Nevertheless, we may expect results of very considerable theoretical interest to be forthcoming in the future from experiments under other conditions, particularly with very small fields.

Suppose we have a colorimeter in which the two halves of a 2° field, such as that illustrated in Fig. 4, are illuminated respectively by a test colour C to be matched, and a mixture of red, green and blue radiations, which we will term the matching stimuli and to which we will give the symbols R, G and B. Experiment then shows that in general it will be possible to obtain a match between the two halves of the field, both for colour and brightness, for one particular combination of R, G and B.

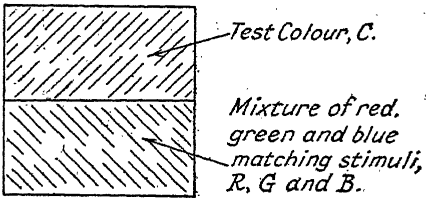
Test Colour, C.
Mixture of red, green and blue matching stimuli, R, G and B.

FIG. 4.—The matching field of a colorimeter.

The fact that three matching stimuli are necessary and sufficient to obtain a match has been central to most theories of colour vision, and particularly to the trichromatic or three-response theory. The assumption is made that there are three types of receptor in the retina, one type having a spectral sensitivity curve with its maximum towards the red end of the spectrum, the second with its maximum in the green and the third in the blue. The colour quality of any light stimulus will then be determined by the extent to which it stimulates each of the three processes.

Various attempts have been made to determine the sensitivity curves for the three processes postulated on this hypothesis, for example, by analysing subjective phenomena such as colour mixture data, adaptation, discrimination, colour matching in defective colour vision and so on. A recent set of curves derived by Pitt [4] on the basis of colour mixture and colour blindness data, and some adaptation measurements by Walters [5], is shown in Fig. 5. If these curves, or some other curves similar to them, are correct, then we should expect to find photo-chemical substances in the cones with absorption curves corresponding to Fig. 5. The absence of detailed and reliable evidence for such substances may be reasonably attributed to the great practical difficulties of isolating them ; it would certainly appear from Granit's experiments illustrated in Fig. 3 that the foundations of the theory are fundamentally sound.

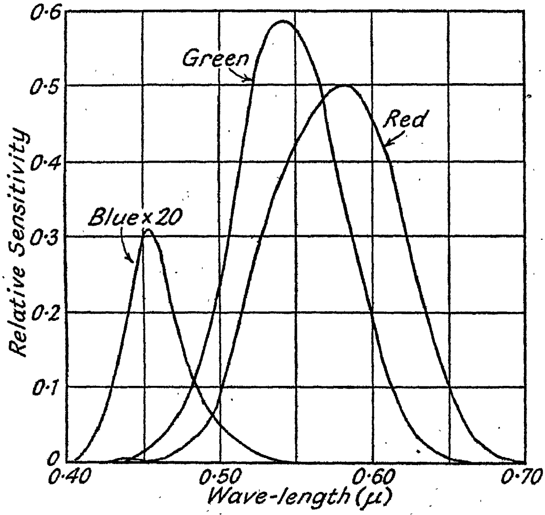

FIG. 5.—Hypothetical spectral sensitivity curves of the red, green and blue processes of the three-response theory, as deduced by Pitt from colour mixture, colour blindness and adaptation data.

Returning to the colour match illustrated in Fig. 4, the visual equation between C and the R, G, B mixture can be expressed algebraically by the equation

$$C = u.R + v.G + w.B,$$

where u, v and w represent the amounts of R, G and B in the match when expressed in some convenient units (*e.g.* energy units, luminosity units, etc.). A further experimental fact of great importance is that these colour equations are additive. Thus, if we had two radiations C_1 and C_2 whose colour matches were represented by the equations

$$C_1 = u_1.R + v_1.G + w_1.B$$

and

$$C_2 = u_2.R + v_2.G + w_2.B,$$

then if C_1 and C_2 were mixed additively by some suitable optical device (*e.g.* a rotating disc, an integrating sphere, etc.), it is found that the new colour C_3 will be matched by an R, G, B mixture represented by the equation

$$C_3 = (u_1 + u_2).R + (v_1 + v_2).G + (w_1 + w_2).B.$$

The importance of this additive principle lies in the fact that, if the colour equations for each of the radiations through the spectrum are known, the colour equation for any heterogeneous radiation can be calculated directly from a knowledge of its spectral composition.

Colour Measurement on the Trichromatic System

It will be noticed that the equation for C given above is in terms of R, G and B. Now R, G and B are light stimuli and the relation is therefore a stimulus relation which in effect merely states that the stimuli represented by the two sides of the equation arouse identical colour sensations. From the point of view of visual theory, it would be of interest if R, G and B corresponded to the actual responses postulated by the three-response theory, and u, v and w the activities along those three response paths. From the technical point of view, however, the stimulus equation itself is a perfectly satisfactory specification of the colour C, provided the stimuli R, G and B have been given an exact physical definition, *e.g.* by their wavelengths, if they happen to be monochromatic radiations.

If a system of colour specification is to be of general use among different laboratories and in different countries, some agreement on the choice of the red, green and blue stimuli in the colour equation is necessary. This does not mean that the matching stimuli incorporated in all types of trichromatic colorimeter should be identical, but it is necessary that the measurements recorded in terms of the matching stimuli R, G and B of a colorimeter should be expressible in terms of some standard reference stimuli, say X, Y and Z.

Fortunately, the additivity of colour equations and, as a corollary, the application of the ordinary arithmetical processes of addition, subtraction, multiplication and division to colorimetric quantities, implies that if the colour equations for R, G and B are known in terms of X, Y and Z, then the colour equation for C in terms of R, G and B can readily be converted into an equation in terms of X, Y and Z. The adoption of standard stimuli X, Y and Z thus in no way limits the choice of red, green and blue stimuli in a colorimeter.

In 1931 the Commission Internationale de l'Éclairage established an international system of colour measurement on this principle and the method has been widely applied in science and industry [6, 7]. Two stages were involved in the establishment of the system, (*a*) the colour-matching characteristics of an average observer had to be adopted to define the colour vision of a hypothetical standard eye, and (*b*) the qualities and units of the three reference stimuli X, Y and Z had to be defined.

Stage (*a*) was accomplished by tabulating the amounts of the three monochromatic radiations $0{\cdot}700\mu$, $0{\cdot}5461\mu$ and $0{\cdot}4358\mu$ required by the standard observer to match the colours through the spectrum. The tabulated data were the mean of experimental measurements made at the National Physical Laboratory and at the Imperial College. Stage (*b*) consisted in defining X, Y and Z by their trichromatic equations in terms of the monochromatic radiations above, and in transforming the colour equations for the spectral colours so as to refer to X, Y and Z.

The particular selection which was made for X, Y and Z was not made on the basis of any theory of colour vision, but for practical convenience in colorimetric and photometric calculations. In the equation given for the colour C earlier, namely

$$C = u.\mathrm{R} + v.\mathrm{G} + w.\mathrm{B},$$

u, v and w were assumed to be positive quantities corresponding to the intensities of R, G and B employed in the colorimeter. Regarded merely as an algebraical equation, we might define a hypothetical colour by three coefficients u, v and w which might include negative quantities. Such a stimulus might not have any physical existence, but mathematically it would not differ from any other physical stimulus. This could be compared to the definition of an alloy in terms of the admixture of three elementary metals. There is no difficulty in defining an alloy with a negative amount of one of the elements and in relating it algebraically to all-positive alloys, even though no such combination could be produced chemically.

X, Y and Z are, in fact, all of this hypothetical character. They should properly be regarded as algebraical abstractions which are useful because they simplify colour calculations, but in so far as a mental picture is of assistance, they may be regarded as super-saturated red, green and blue radiations respectively.

One convenient method of representing the amounts of X, Y and Z required to match the spectral colours is in the form of the equal-energy distribution curves in Fig. 6. The ordinates at any wavelength λ give the distribution coefficients $\bar{x}_\lambda$, $\bar{y}_\lambda$ and $\bar{z}_\lambda$, where these coefficients define the amounts of X, Y and Z which would have to be mixed to give a colour and brightness match of λ for a constant energy of λ through the spectrum. The units of $\bar{x}$, $\bar{y}$ and $\bar{z}$ are here based on equality of X, Y and Z required to match an equal-energy white illuminant.

If we sum all the $\bar{x}_\lambda$, $\bar{y}_\lambda$ and $\bar{z}_\lambda$ values through the spectrum, we shall obtain the total amounts of X, Y and Z required to match the radiation represented by the curves in Fig. 6. Since this is the equal-energy stimulus and since the units of $\bar{x}$, $\bar{y}$ and $\bar{z}$ are adjusted for equality in the match on this stimulus, we should expect and find the three sums, that is the areas under the three curves, to be equal. The distribution curves can further be used to derive the trichromatic specification for any heterogeneous radiation provided the energy distribution is known. Thus, if some radiation S is defined by the energy e_λ which is radiated at each wavelength λ through the spectrum, the amounts of X, Y and Z required to match S will be given by the tristimulus coefficients x', y' and z', where

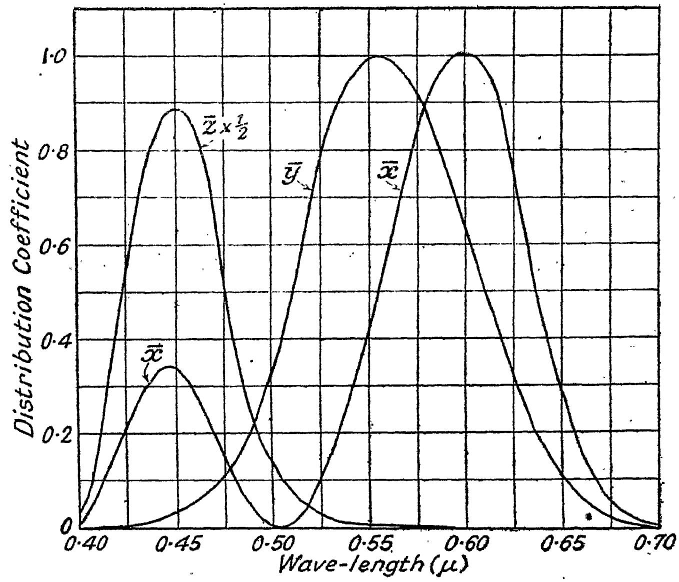

FIG. 6.—The distribution coefficients $\bar{x}$, $\bar{y}$ and $\bar{z}$ for the equal-energy spectrum, showing the amounts of the stimuli X, Y and Z required to match the spectral radiations.

$$x' = \Sigma e_\lambda . \bar{x}_\lambda$$
$$y' = \Sigma e_\lambda . \bar{y}_\lambda$$
$$z' = \Sigma e_\lambda . \bar{z}_\lambda$$

This is equivalent to multiplying each of the distribution curves of Fig. 6 by the energy curve of the stimulus S, and integrating the areas under the new curves. x', y' and z' will then be given by the three areas and the tristimulus equation for S will be of the form

$$S = x'.X + y'.Y + z'.Z$$

It is for many purposes convenient to separate the colour quality of a stimulus from its photometric magnitude. The quality depends on the relative amounts of X, Y and Z required in a match, while the intensity depends on the absolute amounts. One way of defining the quality of a stimulus independently of its quantity, is to divide the tristimulus coefficients by their sum $(x' + y' + z')$ to give the unit trichromatic equation

$$S = x.X + y.Y + z.Z,$$

where the trichromatic co-ordinates x, y and z are given by

$$x = \frac{x'}{x' + y' + z'},$$

$$y = \frac{y'}{x' + y' + z'},$$

$$z = \frac{z'}{x' + y' + z'},$$

and necessarily $x + y + z = 1$.

The unit equation for S is now a function of two variables instead of three, since if any two are known, the third can be derived. It is therefore possible to represent the colour quality of any stimulus on a plane diagram such as the chromaticity diagram of Fig. 7, in which the horizontal axis represents the x co-ordinate, the vertical axis the y co-ordinate.

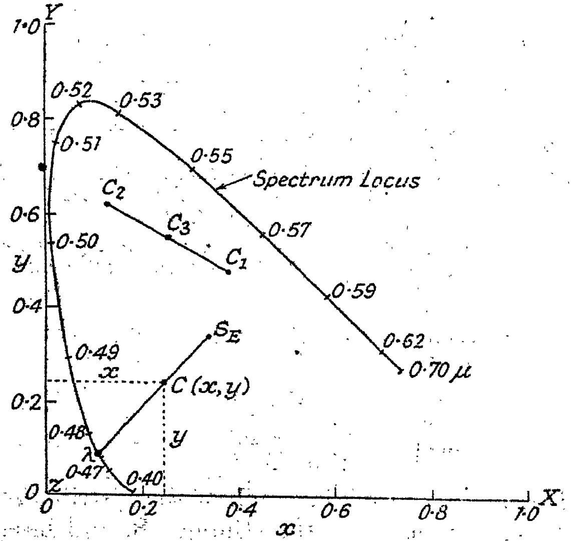

FIG. 7.—The chromaticity diagram in terms of the reference stimuli X, Y and Z.

This diagram is a stimulus diagram which is, fundamentally, a representation of colour mixture data. The positions of the spectral colours in the chart are shown by the spectrum locus; if the coefficients of any wavelength λ on this locus are x_λ and y_λ, then it means that λ will be matched by x_λ of X, y_λ of Y and $(1 - x_\lambda - y_\lambda)$ of Z. The equal-energy white stimulus S_E has the co-ordinates (0·333, 0·333) and any other colour C has the co-ordinates (x, y). It can be shown that in this diagram the result of mixing two stimuli is equivalent to finding the centre of gravity of two weights, so that the result of mixing equal quantities of C_1 and C_2 is to produce a new colour C_3 which will be represented by a point lying mid-way along the straight line joining C_1 and C_2. An alternative way of expressing C would therefore be to draw a straight line from S_E through C to intersect the spectrum locus at λ. We could then define C by its admixture of S_E and the monochromatic radiation λ.

The trichromatic system and the chromaticity chart thus provide a unique and unambiguous method of specifying the colour quality of a stimulus. Any coloured sample can be measured and located in the chromaticity chart and valuable information in regard to its relation to other stimuli can be derived. The system may be used to record the changes in colour of a dyed material as a result of fading, for specifying the colours of signal glasses, for analysing the quality of reproduction in a system of colour photography and so on. The specification is not intended as a guide to the artistic design of a colour scheme, but once an artist has chosen the colours which he wishes to include in a scheme, the technician can then specify the chosen colours on the trichromatic system and subsequently check and control the production of the colours, so that the artist's wishes can be executed as accurately as the manufacturing processes permit.

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THE GEOCHEMISTRY OF URANIUM

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I. Introduction

Uranium in the earth's crust is usually found in association with radium and other radioactive elements. After the discovery of radium by Mme and M. Curie in 1898, radium and uranium ore was first mined at Joachimsthal (Jáchymov) in Czechoslovakia, but in 1912 radium-uranium bearing vanadiferous sandstone began to be mined in the States of Utah and Colorado. The discovery of still richer ores in Katanga (Belgian Congo), however, made the mining of the vanadiferous sandstone unprofitable and, from 1922 onwards, Katanga became the chief producer of radium. In 1930 radium ores were discovered on the eastern shore of the Great Bear Lake in Arctic Canada. During the first five years of mining (1933–37) 54 grams of radium were obtained from this region and thus Canada in its turn became the chief provider of radium. Smaller amounts of radium ore are also mined in Cornwall, Australia, Portugal, Russia and other countries.

The most important radium-uranium ores are associated with mineral veins of hydrothermal origin (Arctic Canada, Katanga, Joachimsthal, Cornwall), while vanadiferous sandstone (Utah and Colorado), vanadiferous limestone (Fergana, U.S.S.R.) and pegmatites (Ontario, Karelia) are classed as less profitable deposits. The total amount of radium produced up to date has not been published. During the first thirty years of radium mining (1898–1928) approximately 500 grams of radium were actually produced and possible reserve estimated at 1000 grams. Taking 1 gram of radium mined as equivalent to 3 metric tons of uranium obtained as a by-product of refining, the corresponding amounts of uranium would be 1500 and 3000 tons respectively, but the present estimated reserve (not published) may be much higher.

According to the available evidence it seems that the radioactive elements are concentrated in the outermost shell of the earth. At a reasonable guess one may assume that this shell is

100 km. thick and that it consists of three zones: granitic (acid igneous rocks), basaltic (basic igneous rocks) and peridotitic (ultrabasic igneous rocks). The relatively thin mantle of sedimentary rocks and the ocean waters may be ignored.

The latest estimates given for the average content of uranium in the earth's crust are as follows:

TABLE I

AVERAGE CONTENT OF URANIUM IN THE EARTH'S CRUST

(in grams per metric ton (10^6 gm.))

(1 gm./m. ton = 10^{-6} gm./gm. = 10^{-4} per cent.)

	Authors.		
	Holmes, 1937 [1].	Keevil, 1938 [2].	Evans & Goodman, 1941 [3].
Acid igneous rocks	6·0	2·8	3·0 ± 0·3
Basic igneous rocks	1·8	0·95	0·96 ± 0·11
Ultrabasic igneous rocks	1·5	—	—
Sedimentary rocks	—	—	1·2 ± 0·1

Taking the lowest of these estimates one may assume that the average content of uranium in the outer shell of the earth is 1 gram per ton. The volume of a shell of 100 km. in thickness is about 4·6 per cent. of the total volume of the earth and its weight about 2·6 per cent. of its total weight. So even if a small amount of uranium is present in the interior of the earth, the total amount of uranium in the earth must be estimated in a fraction of 1 per cent. It must be remembered, however, that the estimated amounts of certain well-known elements in the earth's crust are even smaller. Expressed in grams per ton these amounts are as follows: boron 3, antimony 1, mercury 0·5, iodine 0·3, silver 0·1, platinum 0·005, gold 0·005.

Of all the igneous rocks granite contains most uranium, but even so this concentration is very small as compared with the concentration of uranium in the late magmatic products of granitic magma, such as in certain types of pegmatites and mineral veins. This concentration is probably due to the fact that uranium does not form stable compounds with the prevalent rock-forming elements such as silicon and aluminium, and under magmatic conditions only combines with the rare earths, thorium, niobium, tantalum and other similar elements, which usually form part of the fluid or volatile fraction of the magma, are thus easily concentrated in the upper region of the magmatic reservoir and are subsequently intruded in the form of pegmatite dykes and hydrothermal veins.

The geochemical cycle of uranium has been little studied and it can be only tentatively followed up from the initial magmatic stage through all the stages of endogenetic and exogenetic processes taking place in the lithosphere, hydrosphere and biosphere. The migration of uranium, when arrested at a certain stage, leads to its concentration; when accelerated, to its dispersion. The genetic history of uranium can be suggested by a careful study of the paragenesis of minerals and association of elements.

2. The Association of Chemical Elements

There are about 100 minerals in which uranium is present in large enough quantities for us to assume that it forms an integral part of the molecule. These uranium-bearing minerals reveal one striking feature, namely, that they all contain oxygen and thus that uranium is never present among native elements, sulphides, arsenides, tellurides, etc. According to their genetic association the uranium-bearing minerals can be divided into three main groups:

I. Orthomagmatic and pegmatitic: Phosphates, Titanites, Niobates, Tantalates, Tantalo-niobates, Titano-niobates, Zirconates, Oxides.

II. Hydrothermal and supergenic: Oxides, Hydroxides, Hydro-silicates, Hydro-arsenates, Hydro-phosphates, Hydro-carbonates.

III. Biogenic: Hydro-vanadates.

On the whole, this classification falls in with the genetic order ranging from high-temperature anhydrous minerals to low-temperature hydrous minerals, but the two schemes cannot be exactly matched, simply because every magmatic or epimagmatic process passes through a wide range of temperature changes, and thus, for example, supergene minerals may be found in pegmatite dykes as well as in hydrothermal veins. In spite of this difficulty all these minerals can be grouped into two assemblages: high temperature and low temperature.

The high-temperature assemblage is characterised by the following elements: thorium, lead, rare earths, tantalum, niobium, titanium, and to a smaller extent by silicon, phosphorus and carbon. The most important elements belonging to this assemblage are heavy elements which occupy the lower part of the periodic table (Fig. 1). Nearly all the minerals are dark in colour, heavy in weight, hard and relatively stable. They occur in pegmatite dykes and as accessory minerals in granites.

The most important mineral of this assemblage is uraninite. A considerable amount of uranium, however, is found in niobates, tantalates, titanates, tantalo-niobates and titano-niobates in the form of minerals such as euxenite, polycrase, columbite, ellsworthite, samarskite, hatchettolite, betafite, mendeleevite, etc. To a minor degree uranium is also associated with thorites, silicates and phosphates, such as thorite, thorianite, zircon, naegite, monazite and xenotime. It is usually assumed that these minerals are made of salts composed of cations such as U, Th, TR (Terræ Raræ—rare earths), Zr, etc., and simple or complex anions of the type $[PO_4]^{3-}$, $[TiO_4]^{2-}$, $[NbO_3]^{1-}$, $[TaO_3]^{1-}$, $[SiO_4]^{2-}$ and others, forming compounds of the following types: $TR^{3+}[PO_4]^{3-}$ xenotime, $Th^{2+}[SiO_4]^{2-}$ thorite, $TR_2^{3-}[TiO_4]_3^{2-}.TR^{3-}[NbO_3]_3^{1-}$ euxenite.

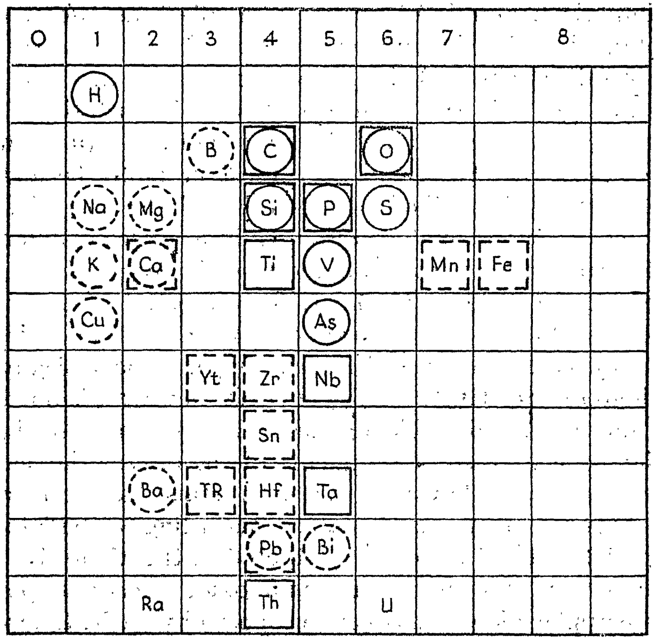

FIG. 1.—Association of chemical elements with uranium.

Solid lines—dominant anions; *dashed lines*—dominant cations; *squares*—high-temperature assemblage *circles*—low-temperature assemblage.

The low-temperature assemblage is characterised by the following elements: arsenic, vanadium, sulphur, silicon, phosphorus, carbon and the hydroxyl group. These elements are relatively light in weight and occupy the upper part of the periodic table (Fig. 1). The majority of minerals are light in colour (varying from yellow to green), light in weight and rather unstable under atmospheric conditions. Pitchblende is the characteristic mineral of this assemblage. The remaining minerals are extremely varied in composition and are represented by the following groups:

Para-uranates (*e.g.* curite, $2PbO.5UO_3.4H_2O$);
Hydroxides (*e.g.* beckerelite, $4UO_3.7H_2O$);
Hydro-silicates (*e.g.* sklodowskite, $MgO.2UO_3.2SiO_2.7H_2O$);
Hydro-carbonates (*e.g.* sharpite, $6UO_3.5CO_2.8H_2O$);
Hydro-sulphates (*e.g.* zippeite, $2UO_3.SO_3.5H_2O$);
Hydro-phosphates (*e.g.* torbernite, $CuO.2UO_3.P_2O_5.8H_2O$);
Hydro-arsenates (*e.g.* uranospinite, $CaO.2UO_3.As_2O_5.8H_2O$);
and
Hydro-vanadates (*e.g.* carnotite, $K_2O.2UO_3.V_2O_5.3H_2O$).

From the economic standpoint the most important minerals are uraninite and pitchblende. Both of them are composed of oxides of uranium (UO_2 and UO_3) and of a smaller amount of oxide of lead (PbO). Uraninite occurs as well-formed octahedra and it contains a certain amount of ThO_2 and oxides of the rare earths. Pitchblende occurs in the form of colloform incrustations.

According to Goldschmidt & Thomassen [4], UO_2 (uranous oxide) is isomorphous with ThO_2 and CeO_2 most probably because of the closeness of the ionic radii of these metals: U, 0·97 Å; Th, 1·02 Å; Ce, 1·01 Å. Uranous oxide crystallises in a lattice of fluorite type and each uranium ion is co-ordinated to eight oxygen ions. In this case the ratio of radii of U and O ions is equal to 0·79, a figure which represents a close approach to the theoretical ratio of 0·73 for a closely packed lattice of fluorite type. Theoretically such a lattice should be relatively stable, but this, however, is not the case. Both uraninite and pitchblende under X-rays show a highly disrupted lattice composed of fragments of the original lattice. Such a phenomenon, also shown by zircon crystals, has been called by W. C. Brögger a "metamict" state. According to the latest study the metamict state of zircons is attributed to the disruptive activity of uranium. In the case of uranium oxide such a hypothesis is even more applicable and has been proposed by H. V. Ellsworth [5]. The mechanism of this transformation, called "autoxidation" by Ellsworth, is based on the assumption that the radioactive transformation of uranium into its final product, lead, releases one oxygen atom which, in its turn, oxidises uranous oxide (UO_2) into uranic oxide (UO_3), *i.e.* $2UO_2 = PbO + UO_3$. If, however, autoxidation were the sole cause of the oxidation of uranium, then the amount of PbO in the mineral should be proportional to the amount of UO_3. The actual plotting of PbO against the relative proportion of UO_2 and UO_3 (plus the equivalent amount of ThO_2) for 52 analyses of uraninite (Fig. 2) shows quite clearly that the relation is not so simple, the dots not being arranged along a definite line or belt but widely scattered. Considering that the minerals analysed are of various ages and of varying degrees of freshness, such a scattering of dots is not surprising. Averaging the analyses into three groups according to the amount of PbO we get the following figures:

PbO.	UO_3 Ratio.
0·00– 4·99	29·70
5·00– 9·99	45·60
10·00–14·99	50·90

In the diagram (Fig. 2) these average values are shown by means of crosses. A curve drawn through these averages shows the progressive increase of UO_3 with the increase of PbO, and this curve is approximately parallel to the theoretical line corresponding to the equilibrium expressed by the equation: $2UO_2 = PbO + UO_3$. This suggests that autoxidation may be a real factor in the transformation of uranous into uranic oxide. The distribution of the dots in the diagram does not suggest the existence of any definite proportion between the oxides and therefore there is no ground for assuming the hypothetical oxide U_3O_8 as being present, as frequently reported in analyses of uraninite. The analyses also show that neither pure UO_2 nor pure UO_3 do exist in nature and that on the whole uraninite is extremely variable in composition and

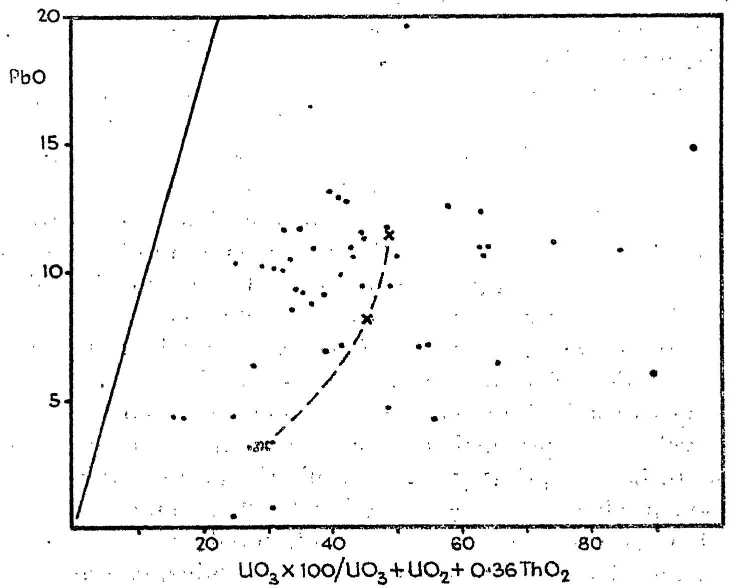

FIG. 2.—Diagram showing the relation between PbO and the relative amounts of UO_2 and UO_3 in uraninite (52 analyses).

The line drawn from the left bottom corner corresponds to the equilibrium condition expressed by the equation $2UO_2 \longrightarrow PbO + UO_3$. The crosses connected by a dashed line indicate the average composition for given intervals of PbO.

can only be represented by a mixture of the two oxides in an indefinite proportion.

The principal ions entering into the composition of uraninite are probably as follows: uranous cation U^{4+}, uranyl cation $[UO_2]^{2+}$ and uranate anion $[UO_4]^{2-}$. Uraninite is probably characterised by a dynamic equilibrium between uranous and uranyl cations, representing the end members of the transformation of U^{4+} into U^{6+}. Such a transformation from a state of low to a state of high valency is bound to decrease the size of the ionic radius of uranium and such a decrease will obviously affect the stability of the lattice. Assuming that in the uranous oxide each uranium ion is surrounded by eight oxygen ions and on transformation into uranic oxide the co-ordination number changes to four (tetrahedral grouping), the theoretical ratio of ionic radii for the tetrahedral grouping is 0·225 and, taking the radius of oxygen (O^{2-}) to be 1·32 Å, one would expect that the ionic radius of the hexavalent uranium atom to be of the order of 0·30 Å. Such a transformation of the crystalline lattice of uranium oxide in a solid state would entail the complete breakdown of the crystalline structure and thus would explain its structural anisotropy or the, so-called, metamict state.

This hypothesis may also be capable of explaining the association of chemical elements in the two assemblages. The minerals belonging to the high-temperature assemblage are characterised by a relatively high proportion of uranous oxide, which one may assume to be of a primary origin, while the uranic oxide is a later oxidation product. On the other hand, all the minerals belonging to the low-temperature assemblage contain only uranic oxide. One may assume that the formation of the latter minerals was governed by the ease of isomorphous replacement of the component ions, which in its turn was determined by the relative sizes of the ionic radii. The ionic radii of the prevalent elements of the two assemblages are as follows:

High-temperature assemblage: U^{4+} 0·97, Th^{4+} 1·02, Nb^{4+} 0·69, Pb^{4+} 0·84, Ti^{4+} 0·68.

Low-temperature assemblage: U^{6+} 0·30 (?), V^{5+} 0·59, Si^{4+} 0·41, P^{5+} 0·34, S^{6+} 0·29, C^{4+} 0·15.

This shows that the elements of the high-temperature assemblage have comparatively large, while the elements of the low-temperature assemblage have comparatively small ionic radii. In such a way one may trace the influence of crystallochemical factors on the association of elements and on the paragenesis of minerals in the earth's crust.

3. The Paragenesis of Minerals and the Genetic History of Uranium Deposits

Under high temperature conditions uranium apparently has little or no affinity towards silicon and aluminium and thus does not enter into the composition of the principal rock-forming minerals such as feldspars, pyroxenes or olivines. In granitic rocks uranium is concentrated only in certain accessory minerals such as zircon, monazite, allanite, columbite, gadolinite, polycrase, etc. These minerals are also abundant in certain types of pegmatites. According to Fersman [6] the granitic pegmatites can be subdivided into ten principal types, each type being defined by the range of temperature during which the main fraction of its typomorphic minerals crystallised out. These pegmatite types are shown in Fig. 3, which gives also a diagrammatic representation of the geochemical stages of uranium. The uranium minerals are confined chiefly to the rare-earths type of pegmatite, a type characterised by the presence of niobates, tantalates, tantalo-niobates, oxides, etc. In all these minerals uranium is invariably associated with thorium and the rare earths. The exact paragenetic relations of these minerals are unknown, but there are indications that the crystallisation proceeded in the order of an increasing uranium content and that the uranium-bearing niobates, tantalates and other similar minerals were the first to crystallise out. These were probably followed by the uranium-rich minerals which crystallised out in the following order:

thorite → uranothorite → thorianite → uranothorianite →
uraninite → pitchblende → supergene uranium minerals.

The high-temperature assemblage also contains a rather rare but highly significant group of minerals, namely, the carbon compounds. Already in 1874 A. Helland described numerous occurrences of a strange pitch-like material resembling anthracite or hard asphalt in the pegmatite dykes of Sweden and Norway. In the famous pegmatite dyke of Ytterby near Stockholm this substance was found enclosed in gadolinite and elsewhere it was found replacing uraninite. In Canada a similar carbonaceous substance, occurring in pegmatite dykes, was discovered in 1894 by J. Obolsky and since described in detail by H. V. Ellsworth [5]. Because on analysis it was found to be composed mainly of Th, U, C, H and O, Ellsworth gave it the name of thucholite. Similar material was also found in the pegmatite dykes of Karelia and named carburan, while a variety containing cerium discovered in the Kola peninsula in the U.S.S.R. was named carbocer. This shows that these carbonaceous substances are of a widespread occurrence and are found only in a particular type of pegmatite, namely, in the rare earths pegmatite containing uranium minerals. Thucholite is invariably associated with uraninite, which it frequently replaces even when the last-named mineral is completely enclosed in feldspar or quartz. According to Ellsworth "the thucholite occurs in exactly the same way as the uraninite, embedded in feldspar, quartz, or mica, or in a mixture of all three, and is certainly just as much a primary mineral as are any of those just mentioned" [5, p. 175]. Further, Ellsworth writes as follows: "The occurrence of carbon as a magmatic mineral is established, and it looks as if there has been absorbed in thucholite a sample preserved through some 1000 million years, of the final gases of a Precambrian granite magma, with the exception of helium which is not appreciably retained by carbon. It becomes evident also that uraninite is formed under strongly reducing conditions in the presence of carbon, carbon monoxide, and hydrogen and, therefore, its essentially UO_2 initial composition is almost a certainty. The foundation for the autoxidation process in uraninite, as suggested by the writer, thus becomes secure. Further, one may speculate as to whether the noteworthy differences between uraninite, which occurs in crystals, and the always amorphous pitchblende, may not be due to difference in the reducing effect of the magmatic vapours or solutions at the same time they were formed. Under highly reducing conditions such as prevailed when thucholite was formed, uranium would appear as the lowest oxide (UO_2) which has a strong tendency to crystallise, so much so that artificial crystals can be easily prepared. Isomorphous oxides as ThO_2 (which is definitely known to be isomorphous with UO_2) and rare-earth oxides (about which less is known at present) would be likely, if present, to crystallise with the UO_2 as uraninite. On the other hand, under less completely reducing conditions and probably at lower temperatures, representing a later stage of the magmatic vapours, uranium would probably precipitate as U_3O_8 with which ThO_2 and rare earths are not isomorphous, and which has little if any tendency to crystallise. Thus there is a rather plausible explanation for the remarkable fact that uraninite always contains thorium and rare earth besides relatively large amounts of UO_2, often greatly exceeding the ratio of UO_2 in U_3O_8, and always crystallised, whereas pitchblende contains only traces, if any, of thorium and rare earths and has a UO_2 content not greater in amount than is required for U_3O_8 and is never crystallised. The fact that carbon occurs in various forms in nickel-iron meteorites, as diamond in the blue-ground pipes, etc., is sufficient evidence that it can be a primary constituent of deep-seated rocks" [5, pp. 183–4].

Besides being present in thucholite and other carbonaceous substances, magmatic carbon is also present in certain varieties of phosphates. Magmatic apatite containing Y_2O_3 was described as long ago as 1898, but recently cerium-silicate apatites, yttrium-silicate apatites and carbonate apatites have been described, and a suggestion has been made that there exists a continuous series of carbonate apatites ranging from $Ca_3P_2O_8$ to $CaCO_3$. In addition to carbonate apatites some pegmatites contain ancylite (cerium-strontium carbonate) and parisite (calcium-cerium carbonate) and many pegmatites also contain calcite. The origin of this calcite is still disputed, as there exists a probability that some of it at least may have been derived from a magma contaminated with calcareous sediments, but on the other hand some calcite may be of truly magmatic origin.

Magmatic volatiles, as proved by the analysis of gases evolved from active volcanoes, are composed of numerous carbon compounds, including CO_2, CO and CH_4. The gases obtained from ordinary igneous rocks, such as granite, are comparable in composition to volcanic gases. According to De Cori [7] monazite sand, on heating to 1100° C., evolved a gas of the following composition: CO_2 24·3, CO 9·1, H_2 30·3, He 34·5, hydrocarbons 1·7 per cent. The presence of hydrocarbons in monazite is rather significant, as this mineral is frequently found in high-temperature pegmatites. In any case the fact that uraninite is often found in association with thucholite, carbonate apatite and calcite suggests that there is a definite association between uranium and carbon, an association, as we shall see later, also found among other concentrations of uranium.

Another interesting association in pegmatites is between uraninite and smoky quartz. A number of workers, including Doelter, have shown that on heating, smoky quartz loses its colour, which is restored on its exposure to radioactive emanations, and this suggests that the colour of the smoky quartz may be entirely due to radioactive emanations.

A generalised and simplified scheme of paragenesis of uranium minerals in pegmatites (Fig. 3) shows that these minerals are best developed in the rare-earths type of pegmatite and are also present in smaller quantities in the tourmaline-muscovite and lithium-tin types. The uranium-bearing pegmatites of Canada, Norway, Sweden and Karelia belong to the rare-earths type, those of India and of Villeneuve in Canada to the tourmaline-muscovite type, and those of Woogina, Australia, to the lithium-tin type.

Although there are some cases when a pegmatite is seen to pass into a hydrothermal vein, the majority of hydrothermal veins have no visible connection with pegmatite dykes. In the hydrothermal veins uranium oxide is nearly always present in the form of pitchblende and it is associated with minerals belonging to various

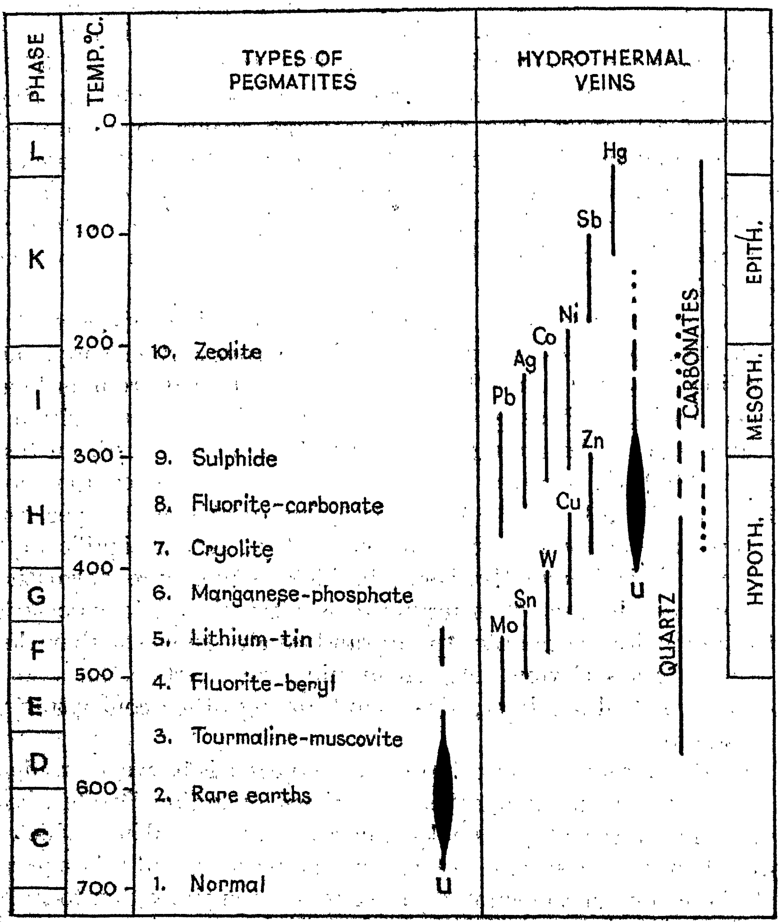

FIG. 3.—Geochemical diagram showing the position of uranium in the thermal succession of pegmatites and hydrothermal veins.

copper, zinc, lead, cobalt and nickel sulphides. A host of hydrous uranium minerals are also present. It is rather remarkable that uranium is absent in the high-temperature type of hydrothermal veins containing molybdenum, tin and tungsten, especially because uranium is abundant in the corresponding pegmatite phase. From all the recorded data in numerous localities such as Jáchymov (Joachimsthal), Katanga and the Great Bear Lake in Canada, it seems that the appearance of pitchblende is connected with the appearance of carbonates in the gangue. In all these localities pitchblende is found in a gangue composed of a mixture of quartz and carbonates and it is absent in the lower horizons of the veins which are composed entirely of quartz.

The mineral association in mineral veins (pitchblende, sulphides of Cu, Co, Ni, Pb, Zn, Ag, native silver, etc.) is totally different from the mineral association of pegmatite dykes. Not only the actual minerals are different, but also uranium does not enter into the composition of the associated sulphides in contrast to the uranium-bearing niobates and tantalates in the pegmatites. The association of sulphur with uranium is only known in the case of hydrous uranium sulphates.

The secondary uranium minerals, such as gummite, torbernite, etc., are very numerous in hydrothermal veins, and many of them are formed at the expense of pitchblende. These minerals belong to the epithermal and supergenic phases of the hydrothermal process.

Thus magmatic concentrations of uranium are present in two types of rocks: pegmatites and hydrothermal veins. The third concentration of uranium is found among sedimentary rocks and, according to all available evidence, is due to biochemical agencies. As compared with the earth's crust as a whole, the sea water shows a greatly reduced concentration of uranium, namely, 0·002 gm./ton [8]. This low concentration, however, is balanced by the increased concentration of uranium in marine organisms and marine sediments. The concentration of uranium in deep sea oozes is apparently very high and terrigenous sediments are also comparatively rich in uranium. Recent figures as given by Unkovskaya [9] show that petroleum on the average contains 100 gm./ton and oil waters 10 gm./ton of uranium. Some of the solid bitumens are exceptionally rich in vanadium and sulphur [10] and this suggests that they may also be rich in uranium, because uranium is very often associated with vanadium in biochemical deposits. The best-known uranium deposits of sedimentary origin are found associated with vanadium in a sandstone of Jurassic age in the states of Utah and Colorado. In his early studies of these deposits, Hillebrand suggested that the concentration of uranium and vanadium is genetically connected with bitumens, and later Hess described carnotite associated with asphalt in veins traversing limestone in Colorado. In spite of this evidence Hess and other writers on this subject have tried to explain the origin of carnotite as being due to circulating solutions depositing carnotite among the decaying vegetable matter, which is rather abundant in the sandstone of this region. There is no doubt that the vegetable remains acted as collectors of carnotite, but it is highly probable that uranium and vanadium salts were originally present in petroleum and oil waters. The abundance of asphaltic material in the carnotite-bearing sandstone is testified by Doerner, who writes as follows : "Because of the large amount of organic material present, these ores are among the most difficult to treat by either mechanical or chemical methods. Much of the organic substance resembles a tough asphalt and is so closely associated with the valuable minerals that both mechanical and chemical disintegration are obstructed" [11, p. 187]. It is well known that petroleum, asphalt and pyrobitumen are very abundant in this region and in all probability the vanadiferous sandstone represents a former petroleum reservoir bed which in course of time formed a condensation trap for bitumen enriched in vanadium and uranium.

A different type of sedimentary segregation of vanadium and uranium is found in Tyuya-Muyun in Fergana. According to Chirvinsky [12] and Fersman [13] the uranium-vanadium ore is found in a calcite gangue infilling karst pipes and cavities in limestone. The principal mineral of this deposit is tyuyamunite ($CaO.2UO_3.V_2O_5.4H_2O$), a mineral allied to carnotite. According to Fersman this deposit was formed through leaching of uranium and vanadium salts from the neighbouring bituminous shales. The figures given by Fersman show that the bituminous shales of the region on an average contain 400 gm./ton of uranium and 850 gm./ton of vanadium, while in the ore body the corresponding values are 600 gm./ton of uranium and 3000 gm./ton of vanadium. This shows that the amount of uranium in the shale is exceptionally high. Such a result is not at all surprising, for in their study of radioactivity of sediments associated with petroleum, Beers & Goodman [14] have shown that the radioactivity of the black shales is more than ten times higher than the radioactivity of ordinary sedimentary rocks and that the content of uranium in these rocks is roughly proportional to the percentage of carbon which they contain. It has also been shown by Russell [15] that of all sedimentary rocks marine bituminous shales have the highest radioactivity, while, on the other hand, coal has very little radioactivity. The low radium content of coal had already been noted by F. W. Smith [16]. The average of five analyses of coal given by this author is $0{\cdot}38 \times 10^{-12}$ gm. of radium per gram of coal, while according to Evans & Goodman [3] the average content of radium in sedimentary rocks is $0{\cdot}57 \times 10^{-12}$ gm./gm. This suggests that radioactive elements are more concentrated in the marine than in the terrestrial deposits of organic origin.

4. The Geochemical Cycle of Uranium

The data relating to the distribution of uranium in rocks and geospheres is scanty and approximate. From the figures given for the average content of uranium in the acid, basic and ultrabasic igneous rocks it is evident that uranium, like radium, is concentrated in the outermost granitic geosphere and that this concentration, as compared with the average content of uranium in the earth as a whole, is of a comparatively high number, but even so this concentration is very small in comparison with the concentrations of uranium in pegmatites, hydrothermal veins and in certain types of sedimentary deposits.

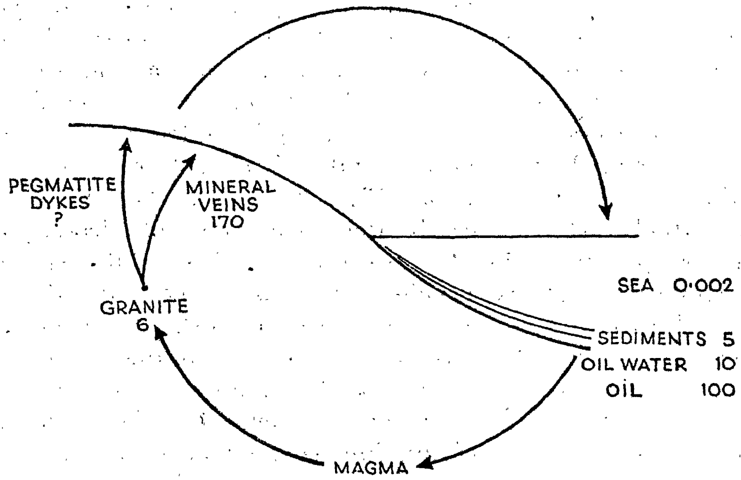

Fig. 4.—Geochemical cycle of uranium. The figures indicate the average content of uranium in gm./m. ton.

The geochemical cycle of uranium embraces only the upper part of the lithosphere, the hydrosphere and the biosphere. A complete geochemical cycle of any element consists of three principal stages: (1) magmatic, (2) sedimentary, and (3) metamorphic (Fig. 4). Assuming that magma is formed through a complete or partial remelt of the crustal rocks, one can visualise the geochemical cycle as a continuous differential movement of matter—ascending during the magmatic stage, and descending during the metamorphic stage. This movement of matter is accompanied by a complex differentiation, in virtue of which elements are concentrated or dispersed. In the case of uranium a certain amount of information is available for a rough reconstruction of the magmatic and sedimentary stages, but hardly any for the metamorphic stage.

During the epimagmatic phase of the magmatic stage occurred two concentrations of uranium—in the pegmatite dykes and in the hydrothermal veins. No estimate of the uranium concentration in pegmatite dykes has ever been made, but a computation from the analyses of vein material of Jáchymov [17] gives an approximate figure of 170 gm./ton of uranium for the whole body of the vein. Considering the high economic value of these veins, at first glance this figure seems to be extremely low, but it is important to realise that the uranium minerals occur in bands or nests which are easily discernible because of their dark colour and are thus easily picked by hand.

The concentration of uranium in pegmatite dykes and hydrothermal veins is probably determined by the nature of the elements which are likely to form stable compounds with uranium, the relative amounts of these elements and the temperature-pressure factors. These two concentrations embrace two quite different assemblages of elements and they are also separated from each other by a considerable temperature interval (Figs. 3 and 5). This interval of approximately 200° C. must be due to different physico-chemical conditions prevailing in the respective mother solutions. It is also significant that the uranium minerals in the pegmatites are associated with thucholite, while in the hydrothermal veins they are associated with the carbonates and that in the sedimentary deposits uranium is associated with bituminous substances.

The sedimentary stage of the geochemical cycle of uranium began with the removal of uranium compounds from the crustal rocks through the process of weathering and their dispersion in the waters of the oceans. This was followed by the concentration of uranium by marine organisms. The highest concentration of uranium is found in the deep sea oozes, especially those of organic origin and in the terrigenous deposits uranium is also associated with organic matter, as it is shown by the bituminous shales and the asphaltiferous sandstones. Petroleum and oil waters are also relatively enriched in uranium. Thus all available evidence suggests that uranium, together with vanadium, is concentrated by living matter of marine origin. The reason for such a concentration is unknown, but it may be that uranium and vanadium, like many other "trace elements," have a definite physiological function, either as catalytic elements or as integral components of organic molecules, especially those forming the respiratory or photosynthetic pigments.

This brief account of the geochemistry of uranium, based on a rather scanty and imperfect data, suggests a possibility of a wider field of research carried out by a combined team of petrologists, mineralogists and biochemists. The distribution of uranium in rocks and minerals, the paragenesis of uranium minerals, the concentration of uranium by plants and animals, the function of uranium in living organisms, all these problems are awaiting solution and detailed investigation. The pioneering attempts by Vernadsky [18] and by Fersman [19] of providing a synoptic view of the geochemistry of uranium must be followed up by a planned research aimed at filling gaps in our knowledge as well as constructing an all-embracing scheme of the geochemical cycle of uranium.

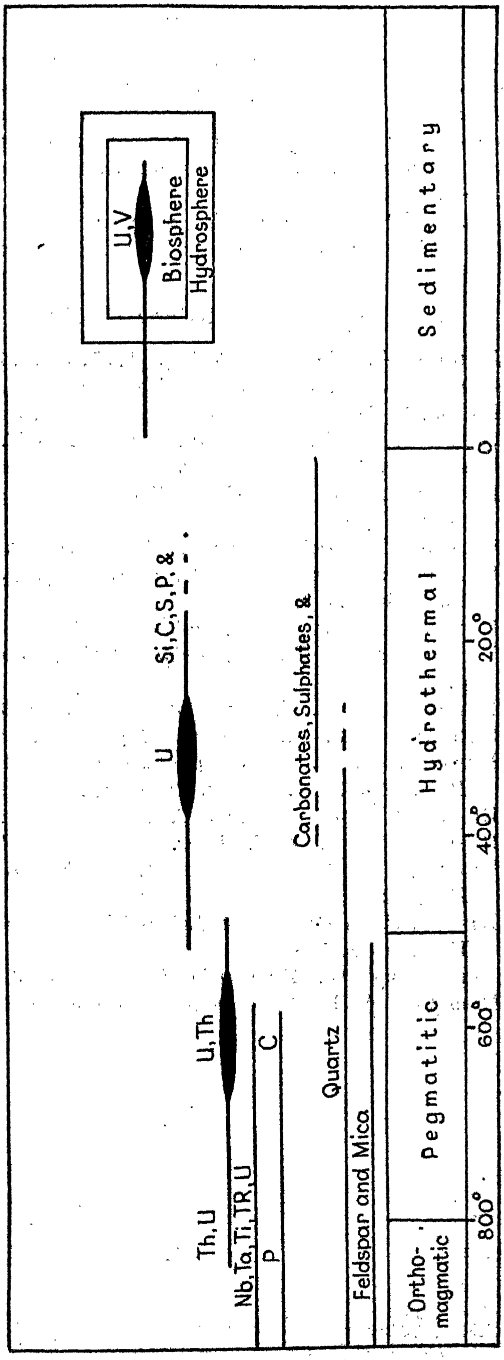

FIG. 5.—Part of the geochemical cycle of uranium showing the paragenetic succession and the association of elements.

GLOSSARY

Colloform. Minerals of colloidal origin often appearing as crusts, globules, etc.
Epimagmatic. Minerals or rocks of late magmatic or postmagmatic origin.
Epithermal. Low temperature hydrothermal deposits.
Hydrothermal. Mineral deposits formed from hydrous solutions.
Karst. Solution effects on limestone leading to the formation of caverns, pipes, etc.
Supergenic. Processes occurring near or on the surface of the earth.
Typomorphic. Minerals characteristic of a given zone, phase, or rock.

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GAPS IN THE SCIENTIFIC EVIDENCE FOR THE PROSECUTION

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IT must not be assumed lightly that scientific evidence brought *for* the Crown is designed to fit the needs of a prosecution intent on conviction. Nothing is, or should be, further from the truth. It is in the prosecution of truth and justice, rather than a criminal, that the Crown offers evidence, and scientific evidence should be as impartial as scientific observation. The Court should hear what is plainly the pursuit of truth, the cold objective proof of facts and the drawing of proper conclusions from them. English law shows a traditional fairness to the accused that may at times exasperate, but is nevertheless, on reflection, plainly just and impartial.

The chain of scientific evidence has, with advances in the scope and technique of laboratory investigation, become a tie that the defence find increasing difficulty in questioning before the lay jury, for less and less is being left to mere opinion. Science is providing more and more cold scientific proof of association between the criminal and the scene of a crime, with a dead victim, a rifled safe, stolen property, forged documents, counterfeit money and the like. The application of the pure sciences to forensic problems involves the skilled assistance of so many experts that no scientist is outside its scope. Physicist, chemist, biologist, pathologist, bacteriologist, analyst, metallurgist, photographer, engraver and the like are all within the reach of the police in their investigation of crime, and it might be expected that such resources would provide scientific assistance which could hardly fail. This is not yet so, for serious gaps still exist and it is well that they should receive attention.

The subject is too wide to discuss from all aspects. Each expert recognises both weaknesses and real gaps in his own chain of evidence, and it would be presumptuous for me to discuss difficulties in specialities outside my own experience. I propose to outline some of the gaps in the evidence for the prosecution on charges of murder by violence. Many of them are likely to be filled by advances in fields outside medicine which were not designed to such an end, for it is the habit of scientific advance to grow and become complete by accretion from all sides; therein lies the advantage of pooled scientific knowledge.

The principal questions to be decided by the pathologist assisting the police in the investigation of a murder are (*a*) the identity of the victim, (*b*) the time of death, and (*c*) the cause of death. Except when poisoning is responsible, the last is rarely a matter of any difficulty, for painstaking care in examination of the injuries and simple deduction are usually sufficient to enable a reconstruction of the course of events to be made with accuracy. Let us examine some of the gaps in the answering of the two apparently simple questions—Who is the victim? When did death take place?

1. Identity of the Victim

When clothing or papers are found on the body identity is seldom in doubt, for persons who can recognise these are soon forthcoming. But when the body is stripped or mutilated the problem becomes a medical one demanding careful study and compilation of scientific data before identification can be effected.

Recognition of the features has proved unsafe on many occasions. In the Luton "Sack Murder" no less than 39 identity visits were paid to the victim in the presence of police officers who demanded close scrutiny, and 9 persons quite genuinely identified the body as that of four different individuals—none of whom proved to be the victim.

Scientific data acquire the safety of disinterested observation. The principal data required are sex, stature, age, colouring, and certain special data such as blood grouping and finger prints. Dentition and details of tattoos, deformities and disease may perhaps be added. In rare circumstances the precipitin test for human protein (injected into rabbits for preparation of test agglutinating sera) may be necessary, because the fragments of tissue are too small for any anatomical identification to be attempted.

Sex provides no difficulty so long as some parts of the pelvic skeleton remain, for the lower spine, sacrum, iliac bones and femurs are quite distinctively shaped in the two sexes. Even the sternum and humerus have provided some tabled measurements by which "mathematical sexing" has been achieved. Of course, the majority of cases cause no such exercise, for soft genital tissues remain for immediate recognition: indeed, one of the last organs to decompose is the tough uterus.

Stature is also a matter of little difficulty, for if the whole body is not available (even in pieces) the height can be estimated to within an inch from the lengths of individual long bones. Karl Pearson and Rollet have both compiled tables for stature estimation based on comparison with standard measurements of humerus, femur, radius, ulna or tibia, and in practice these—more particularly the Pearson's formulæ—have proved remarkably accurate.

Age is unfortunately a problem of more difficulty. For some ten days after birth the organisation of clot in fœtal vessels no longer required, like the ductus venosus, and the drying, sloughing and healing of the umbilical cord, provide a fair index of day-to-day age. But between ten days and six months, when the first lower central incisor teeth of the "milk" dentition erupt, there is a serious gap. Sidney Smith admits that during this period one has to "rely on increase in length and weight," and that "from a medico-legal point of view for fixing the age they are of little value." Nothing may be known of the prematurity or otherwise of birth, of the exact birth weight, or of the progress made in development —a group of most variable factors.

> A child's body found in a portmanteau deposited in Ashford railway station weighed 8 lb. 2 oz. and measured 23 in. in length. This would—if the birth were of average weight and at full term —represent good development up to an age of about 2 months. Police enquiry resulted in the arrest of a woman in Birmingham who admitted killing the child when it was just short of 6 months of age. The discrepancies were explained by her adding that it was born nearly two months prematurely and had progressed poorly with frequent ailments. Even the developing teeth, as yet unerupted, were tardy.

The ductus arteriosus, a developmental shunt between R. ventricular blood—arterial in the fœtus—and the main aorta, closes functionally as soon as the lungs commence to function at birth, but remains open structurally for some weeks or months, occasionally even years: its closure is far too erratic for age estimation. Barcroft and Barclay have recently studied the change in this shunt circulation at birth by means of thorotrast injection and X-ray cinematography, a novel scientific method. The survival of the fœtal form of hæmoglobin remains obscure and provides no assistance.

Age estimation from 6 months to about 25 years is made the easier by the progressive eruption of, first, deciduous and, later, the permanent teeth. After the seventeenth or eighteenth year, when only the third "wisdom" molar remains unerupted, its development may be studied by X-ray of the jaw: calcification of crown and root proceeds steadily—even if eruption fails to follow. The development of the ossification centres of bone formation in the ends of the long bones and their fusion with the shafts provide covering evidence for age estimation during this period. From then until the beginning of the fifth decade (40th–50th year) there is a second gap in which opinion as to age is once again not far short of a guess. The development of adult lines of stress in bone (shown best by X-ray photographs of thin slices as in the Ruxton case), the fusion of the plates of the cranial vault and ossification of cartilages like the voice box and ribs, together with the development of senile conditions, provide rough guides in the later years of life.

Colour in the victim may rest with hair, longest surviving of the eyes, skin and hair which provide this part of the identification data. Hairs are also commonly found adhering to weapons with which crimes have been committed or clinging to the clothing of suspects. The colour is not difficult to judge even when dyeing or bleaching has affected it, but the identity is quite another matter, for hundreds of individuals in any community may have identical hair. Identification of similar hairs can, at the best, only go to increase the number of data linking a suspect with a crime or a weapon with a victim: hairs of victim and assailant may be indistinguishable from each other.

> A W A A F was found murdered in a ditch in Suffolk, and police investigations led to the finding of three hairs identical with the victim's head hair clinging to the trousers of a man already suspect. They afforded a strong direct link between the two which helped to support the circumstantial evidence. Unfortunately sample hairs, taken for exclusion purposes from the suspect's wife, proved to be quite indistinguishable from the dead girl's, and this valuable evidence had to be discarded.

The identification of blood is becoming increasingly complex as the ABO group specific substances become supported by the finding of M and N, P and finally Rhesus factors, but although these genotypes provide grounds for individuality it must remain clear that so long as these are group substances with wide group distribution their value in individual identification must remain limited. Of course, where one of the parties is of the rare groups B (10–15 per cent. of the white population) or AB (3 per cent.) the chances of distinguishing victim's from assailant's are correspondingly increased. Error sometimes creeps into blood grouping owing to the weak qualities of a subsidiary A2 agglutinogen and A2B may be mistaken for B, or A2 for O. Gaps of this kind in the scientific evidence are serious.

Somewhat similar agglutinable substances are also excreted in the secretions of some 50 per cent. of persons, notably in the saliva, and may be detected on discarded objects like cigarette ends. Their value is subject to the same limitations as that of bloodstains. It is ironical that, apart from menstrual blood, science is still unable to distinguish male from female blood, though the development of endocrine studies is already bringing this nearer.

The finger print is, of course, made reliable by its astronomical detail. Galton estimated the chance of two complete ball-prints from different fingers being identical to be less than one in 64 thousand million—some thirty times the world's population—and he has been surpassed in astronomical estimation by Cherrill, Scotland Yard's finger-print expert, who judges it to be something like one in a septillion—a figure with no less than forty-two zeros. A single print may contain far more precise detail than all the other identity data of a body, and even the palm exposed in the "V" of a glove worn by a safebreaker has provided ample data to convict. Even if sufficient data were not present in the forking, contra-forking, looping and ending of ridges, impressions taken in yellow wax, greek resin, spermaceti and tallow coloured by cobalt oxide will provide unlimited further detail in the pore markings. Locard estimates 8–10 pores to open on (and therefore interrupt) every millimetre of ridge. White lead, graphite, chalk, mercury, kaolin, antimony—and silver nitrate, iodine fumes or palladiun chloride as developers—have all added to the delicacy of police method in this art. Even the finger print of detached skin in drowned subjects has been recorded by reversal and by transmitted light both in this country and by Russell in the United States. The finger print on clothing has more recently been demonstrated with the use of 10 per cent. silver nitrate and acetic acid, followed by sunlight exposure as well as by ultra-violet photography.

Demonstration of the finger print on the neck in strangling—and what an advance in scientific detection this would provide—is still unattained. There are still big gaps through which the criminal may escape.

Dentition data and disease or deformity, combined with the basic data set out above, have provided interesting material for study in two recent murders:

> In the classical Dobkin case the remains of a woman were found eighteen months after dismemberment, burning and burial

in lime under a church cellar floor. Identity rested partly with the general data as to sex, age, height and colour, but it was clinched by the presence of natural disease (a fibroid tumour of the womb for which the suspected victim had sought treatment), by superimposed photography of a portrait on the X-ray positive of the skull, and by exact correspondence of certain dental data in the upper jaw. The shape of the jaw, the number and situation of residual teeth, their fillings and the presence of roots left after extraction all corresponded in exact detail with the records of a dental surgeon who had attended a woman whose photograph he recognised as that of the supposed victim.

In the Luton Sack Murder a naked body was found in a shallow stream on the outskirts of the town tied in four potato sacks. It was a woman of 30–35, five months pregnant, dark hair and brown eyes, edentulous, and with an appendix scar in the abdomen. Her finger prints were taken, and a photograph of the profile of the face, X-rays of the jaw, together with a plaster cast of the edentulous upper jaw, was circulated to local dental surgeons without success. Her identity remained unknown in spite of wide circulation of her identity data and photograph and the most extensive enquiries, until a dyers' tag found one day in some discarded clothing caused a visit to a certain address. The woman of this house had disappeared at about the time of the murder and could not be traced. Her general physical data and the dental conditions corresponded with those of the victim. She had had no teeth and the cast and X-ray photograph of the jaw taken at the time the body was discovered were found to correspond exactly with those taken by a dental surgeon who had attended the missing woman. Blood stains of the same group as the dead woman were found splashed on a door jamb in the house and a L. thumb finger print which corresponded with that of the victim was found on a pickle jar which had by then lain for at least three months on a shelf in a cellar. Identification was proved to the satisfaction of the Assize court jury.

2. How Long Dead ?

It has been remarked that the only accurate and scientific answer to the question "How long dead ?" is "Five foot ten—the same as in life"! The greater the length of time which has passed before examination is made, the less reliable is the estimation of the lapse of time likely to be. The only reliable guide is the steady fall in temperature which takes place during the first

24 hours following death. The development of rigor mortis and of the post-mortem livid stains is altogether too uncertain, and by the time decomposition sets in no estimate which may be made can have an error of less than a day—or a correspondingly increased period as the time passes. To achieve anything like the accuracy the police require in fixing the time of death, the body must be found whilst still warmer than the surrounding atmosphere, that is, usually within 20 to 24 hours.

Under average conditions some 2½° Fahrenheit is lost each hour for the first 6 hours and another 1½–2° during the second 6 hours, the loss of heat slowing in rate as the temperature of the body approaches that of its surroundings: thus roughly:—

$$\frac{\text{Normal temperature }(98{\cdot}4) - \text{rectal temperature}}{1{\cdot}5} = \text{hours dead.}$$

Heat loss in the river, lake or sea is about twice as fast as on land. But many factors interfere with the simplicity of this estimation. The time of year, degree of exposure, ventilation, amount of clothing, state of nourishment of the body might alone be sufficient to give a defending counsel anxious to upset the basis of calculation all the material he desired.

> In a suicide pact couple dead of coal-gas poisoning, in whom the circumstances showed death to have taken place at virtually (if not legally) the same time, a difference of 3° F. was present in the rectal temperature 6 hours after death. The warmer body was clothed in nine layers of largely woollen materials, the cooler in three of a lighter texture.

Cerebral hæmorrhage—even the intense congestion which follows from constriction of the neck in strangling—may cause a rise of temperature at death which is incalculable at autopsy: I have observed a temperature of 101° 2 hours after death in a case of violent asphyxial death. As it is commonly in such asphyxial deaths that the police wish to establish the exact time of death this is particularly unfortunate and often offers an opportunity for accused to widen the time for an alibi. A rise of 4 to 6° is not uncommon, but cannot be safely assumed.

The conditions under which maggots raise the temperature of the body—often in parts to levels of 75–78° F.—develop so long after death that they do not interfere with estimations made from the fall from normal within the first 18–20 hours. They have, however, introduced another specialist into the field, the entomologist. The lapse of time after death in the Ruxton case was estimated largely on the life phases of calliphorine flies. Meguin had already applied entomological observations in the endeavour to fix the time of death but came to the conclusion that they had only local value.

Fleas provide a tail to this short cortège, for it has been observed in bodies found drowned they are certain to be dead after some 24 hours, require an hour for revival after some 12 hours' immersion, and some 4–5 hours after 18-20 hours' immersion. Body lice drown in 12 hours beyond revival. The occasions when these things are of value are limited.

When all of these factors have such inadequacies the estimation of the exact time of death seems a hopeless task. The later stages of decomposition both on land and in water proceed too slowly to provide any grounds for close estimations. Bodies which have become mummified (as infants without their contents of bacteria may) remain in a leathery state for months, even years, gradually desiccating. Even adipocere, that landmark in the decomposition of the moist body, is a wayward mark, for as with most chemical, bacteriological and enzyme actions, temperature alone may greatly hasten the pace. Hydrogenation of body fat is accelerated by warmth.

> In the case of a murdered girl in which the body was discovered buried in a shallow sandy grave on Godalming heath, adipocere was found well established in as little as 3½ weeks: the process is rarely seen in this country under 4 to 5 months, and had clearly been hastened by the shallow burial and heavy warm rains with intervening sunshine which had preceded discovery of the body.

Such infinite variability is incalculable to any close limit. Experience merely teaches one to be humble—and grateful when, on the basis of ordinary estimation and bearing in mind as many known factors as seems reasonable, some fairly close estimate is reached. It is never too late to measure the temperature in the hope that, though "the body is cold," it may still register a few degrees above that of the surroundings. In a recent murder at Hastings 7° difference still remained after some 18 hours in the open with heavy snow on the ground and the time of death was fixed to within about an hour.

Such gaps as these in the scientific approach to important primary questions in criminal investigation exhibit weaknesses which must be repaired. Toxicology, forensic ballistics, spectrography hold more and it is by interchange of view and converse that such gaps will become narrowed and eventually closed.

A "DIRECTIVE" PHILOSOPHY OF PAINTINGS[1]

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In the last issue but one of Science Progress to be published—that which appeared shortly after the outbreak of hostilities in 1939 [1]—the present writer tried to present a view of creative art, applied particularly to paintings, based on the *Gestalt* concept, and the associated law of *Prägnanz.* Physically, the standpoint had much in common with potential theory, in its use of maximum-minimum principles, and the analysis led to the adoption of a minimal free-energy as indicative of satisfaction or repose in a work of art. In addition, a species of invariant qualitatively akin to value was introduced. This axiological aspect was not developed further at the time, but is clearly connected with the notion of "requiredness" characteristic of some psychological thinkers. In the sequel, this train of thought will be further investigated. Some six years of intermittent attention to the subject, and the encouragement of certain scholars during that time, taken together, must form the *raison d'être* for the present contribution. Incidentally, a wider implication can perhaps be discerned, namely a connection with Aristotelian philosophy and with a biological system possessing certain teleological features. If the latter trend is somewhat suspect, one can but recollect similar hints in present-day work in theoretical physics.

Further, there is an historical element, mentioned in the earlier paper, but left over at the time for future consideration. This needs clarification now. An effort at a somewhat sharper focus will be made in terms of a species of neo-scholasticism. In passing, this harmonises with much of the view implicit in the theory of art amenable to Husserl's Phenomenology, which itself bears witness in some respects to the *philosophia perennis.*

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[1] The author would express his deep sense of gratitude to Professor Sir Edmund Whittaker, F.R.S., for guiding his thought in many ways over a period of some twenty-five years. To Dr. E. S. Russell, F.L.S., he is indebted for a recent stimulating discussion on the biological aspect, and to Mr. H. Ruhemann for help and advice on the painting by Botticelli referred to in the text.

At this stage it may well be asked, why all this fuss and bother? The answer is that we have come to the end of the time when an essentially Cartesian outlook will suffice as a *Weltanschauung* for each and every activity of mankind. Inadequate even for the modern physics, it is patently incapable of coping with situations unsuitable for analysis by differential equations. If a strict necessitarianism cannot always be maintained in the physical field, it is indeed unlikely to satisfy us in the large uncharted spaces of psychological causality. And indeed the latter is perhaps the more interesting. Here may be the place to notice a tendency in the history of thought to take over bodily some well-tried and sharpened tool already existing, and to set to work with it on a new, or at least upon a little-known, fabric, not always with the happiest of results. Wave-mechanics is a case in point: not that the method failed, far from it, but because in some minds it seemed to induce an impression that the "why" as well as the "how" could be obtained by essentially classical means. Somewhat similar has been the result of applying physico-chemical processes, especially thermodynamics, to biological phenomena. Clearly most valuable, but unable to account for the behaviour of the organism and the characteristics of life. What stands out is that then "ultra" situations, like art, are factual as much as the rest: they cannot be ignored because they are methodologically inconvenient, still less because they are not strictly, or sometimes not even approximately, rational. To investigate them is all the more exciting.

We are faced then with certain facts of man's productive genius, namely great paintings. In beholding them, it is clear how much it cost in creative strife to bring them to birth. What interests us now is not so much *how* as *why*.[1] Technical research has yielded valuable data relating to ways and means employed by the great masters, it has absorbed a fair amount of natural science in the process, but *per se* it is ill-fitted to deal with the reasons for the existence of supreme works of art. Majestic as they are, they are probably but the goal, or end-state, of certain (Aristotelian) causes. At this angle, particularly, it is proposed to study them.

This urge to see the *philosophia perennis* as a kind of intellectual lifeline, stretching back into the ages and forward into the future, raises the question why it seems to hold out something which eager hands may grasp in the present context. The reason is probably to be found in the solidarity of knowledge which has always been its most outstanding characteristic. It is a system capable (unfortunately by no means universally effective however) of dealing collectively with sense data, and thus with arts and sciences alike. In a word, within it may possibly be found that couple or bond we seek in an effort to work out a " directive " philosophy of paintings.

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[1] *i.e.*, the immediate issue is much more qualitative than quantitative, the reason for the existence of pictures more than how they are constructed.

" Directive " suggests at once a sequence of events beginning with potentiality and ending in entelechy, which is precisely what the gradual emergence of the design of a great picture involves. We shall try later on to follow the process in an actual example, a painting by Botticelli. In so doing much of the viewpoint tentatively put forward in the earlier article will be retained ; the difference, however, is the greater weight now accorded to the artist in terms of " directiveness." Physically conceived, this means that maker and object constitute a closed system, rather like the type envisaged in theoretical thermodynamics as developed axiomatically by Carathéodory on the basis of Pfaff's equation. Philosophically conceived, we are attributing a certain purposefulness to the artist's effort, and this in its turn implies a sense of values. If the pressure to create is unconscious, or nearly so, the whole reaction only differs in degrees from many another in the organic world. Thus far by way of introduction. For the rest, " faint heart ne'er won fair lady " is of the essence of the venture to enfold great art within a broadly scholastic frame. There must be no impression of force, as Husserl knew from his experience with the phenomenological horizon, and his overwhelming desire for rigour. One has only to recollect the pregnant condition of art in the days of St. Thomas Aquinas to see how naturally it may serve, or even subserve, the principles of scholasticism, and through it a wide expanse of human need. This is not of course to suggest a lack of reciprocity : for, as the sequel is intended to show, Aristotelian views on causation appear to illuminate, in part at least, the intentionality of art.

" DIRECTIVENESS " IN CREATIVE ART

We consider first, in its application to the artist, the schematic sequence suggested by Murray [2], and illustrated by Russell [3], namely,

$$(BS) \rightarrow (SS) \rightarrow (A) \rightarrow (ES),$$

in which (BS) stands for the " beginning-state," (SS) for the " stimulus situation," (A) for the " action," and (ES) for the " end-state." The whole concept is typically Aristotelian, as indeed its biological origin might almost independently suggest. What matters now is that it provides a clue to creative activity without any necessity for mechanical or materialistic details, such as modes of operation. Its structure is purely functional, and, thus interpreted, seems attractive for our present quest. Whether it corresponds to any actual physico-chemical process or not is for the moment neither here nor there.

An artist assuredly possesses, at a given epoch, a state of unrest or potentiality (BS); he is no less certainly involved in a stimulus situation (SS); activity (A) follows, and an end-state (ES)—for us an object of supreme art is, theoretically, reached. This transition from (BS) to (ES) is mirrored, so to say, in the completed work: it is not difficult in general to distinguish in a great painting the tangible results of this development. The application of X-rays and other physical means to the study of classical pictures has enabled us to follow through—in favourable cases—such attempts as the artist has made to reach satisfaction, often by devious paths, giving a strong hint of a species of effort akin to the biologists' morphoplasticity. In fact, as the artist's purpose expands, so does his design. This is not to assert that the two necessarily go hand in hand at every phase. In most instances it looks as if the whole scheme for a great painting was worked out in detail well in advance, and that when the actual work came to be done, everything was settled and fitted into place. What is involved is something earlier, as may occasionally be inferred from cartoons or preliminary drawings. In these the unfolding process may be more clearly marked, the onset of stimulus and the corresponding action.

Readily revealed in supreme art is the basic difference between organic and inorganic reactions. The former reach their end-state (ES) in spite of all obstacles, overcoming or circumventing them as occasion requires (that complete success does not *invariably* occur can scarcely be taken to invalidate the main argument). The latter approaches its appointed end, or, if impeded sufficiently, further action inevitably ceases. It is perhaps well to observe that all this does not in the least preclude a detailed study of the underlying physico-chemistry: it is probably essential for a final understanding of the whole matter, just as a functional comprehension is imperative at the human level. It appears more likely that some insight into the reasons for creative art can be obtained as an integrative principle rather than upon an analytic one. And this is what the Murray-Russell expression helps us to do. Thus while (BS) → (SS) → (A) → (ES) is directly applicable to the formation of norms and to the description of behaviour, there is no reason why we should not use it tentatively if in fact that behaviour is purposeful. The focus of interest is now upon the terms themselves rather than upon the totality of the reaction, except for one aspect to be discussed later.

Nevertheless, the integrative principles already alluded to suggest something akin to a *Gestalt*, more especially since the goal attained is obviously much more than the " end-summation " of its parts. Stimulus and activity produce together something which without organisation is meaningless. Manifestly, a complete knowledge of the physics and chemistry of the ingredients (wood, canvas, pigments and so forth) of a painting fails to provide any information whatever concerning either its appearance or its meaning. Not only then, on this view, is the end-state (ES) a *Gestalt* at a high psychological level, but so is the natural product of the stimulus and activity, *i.e.* the painting itself. To see this, we have only to remove some part, even a seemingly trivial part, of the design, to bring ruin upon the whole. In other words, the end-state (ES), as objectivized by the finished work, is extremely sensitive, and its state of equilibrium easily upset. If satisfaction is achieved, it is only by the most delicate acts of balance. Poise is, in fact, not only geometric, it is mnemic as well.

To return to the question of the totality of the formula (BS) → (SS) → (A) → (ES), it seems to contain within itself two characteristics familiar to physical chemists, namely the principle of Le Chatelier and the law of mass action. The former asserts in effect that a process once initiated tends to meet with opposition which acts in such a way as to slow down further progress. The latter is concerned with the products of a reaction which, when formed, and unless removed from the scene of operations, may function so as to drive the process in the reverse direction, thus inhibiting complete transformation. If these widespread phenomena are well understood in the field of chemical energetics, they are assuredly significant also in the present context. Not unlike Le Chatelier's principle, many works of art bear witness to a certain evaporation of desire as their fashioning continued, and, on a more extended scale, even schools have inclined to meet obstacles, both social and psychological, which, taken altogether, have combined to slow down their effect, and ultimately perhaps to bring it to a standstill. The law of mass action works in such a way as occasionally to poison the original purity of purpose, and so to bring activity to a premature end, considerably short of the state theoretically attainable. In the language of science, the yield is disappointingly small. Some of these questions have received detailed attention by Pickford [4].

It is just possible to go a step further, and to recognise the analogue of catalysis. Some artists, or groups of artists, have been exceptionally socially-conditional, for example the Barbizon School, where their *milieu* had provided an essential stimulus to achievement. But they sorely needed an interpreter for the uninitiated, and found one in Thoré, in much the same way as the Pre-Raphaelites relied upon Ruskin. In all this, there is unmistakable evidence of directiveness whether consciously appreciated or not. Furthermore, the various hindrances which history shows as sometimes thwarting and sometimes fortifying leading artists have at least stressed the complexity of the whole reaction from beginning to end. It is well to notice the order of events : a stimulus situation (SS) is a pre-requisite to action (A) as in the purely biological field ; indeed (A) may even be endued with such a heavy inertia as never to get under way until (SS) becomes exceptionally powerful. Otherwise expressed, the system may have been originally metastable, or even labile. More common, however, is the reverse effect in which a small threshold stimulus exerts initial response insufficient in the event to reach a valuable goal. Odd as it may seem, the traditionally rigid demands upon medieval art from ecclesiastic quarters often generated enough potential to fire the spark.

It is now possible to see how closely the morphogenesis of a picture approaches the five conditions enumerated by Russell as characteristic of directiveness. Somewhat abbreviated they are—

(1) Action ceases when the goal or end-state is reached.
(2) Action usually persists until the goal is attained.
(3) Alternative methods of reaching a goal may be used, and furthermore, defective methods may be made good by an increased use of alternatives.
(4) Different paths, starting from different beginnings may lead to the same goal : in fact the end-state is more constant than the method of getting there.
(5) Goal-directed activity is limited by conditions, but is not determined by them.

Epistemologically, this scheme is thoroughly Aristotelian ; there is no sign of a mechanism (in this it resembles much of classical thermodynamics) ; the implications are teleological in that we are bidden to look towards ends rather than means, and finally a hint at least emerges as to why great paintings arise rather than how they do. They come about because the conatus or drive which is initiated in the artist's mind forges ahead to its appointed goal, scattering difficulties left and right as it gathers momentum. Thus, around the graceful neck of this general theorem hangs the corollary obtained before by the present writer (Rawlins, *loc. cit.*) from distinctly different premises, that the free-energy of a great painting represents a minimum, associated with some specific degree of organisation. Exactly what degree cannot as yet be determined numerically: it is the "best" as the law of *Prägnanz* demands. But one point needs emphasis, and that is that the best is not necessarily the simplest organisation. Discrimination here goes straight to the core of æsthetics: those entitled to form a judgment regarding the beautiful will reach a decision with a unanimity surprising to anyone unaccustomed to such studies. If "requiredness" so decides, simplicity will be found to give place to complexity.

What is more, art is not alone in this, or at least the fine arts are not. Dirac [5] has pointed out how mathematics may be subject to the same criterion, and with the same confidence of recognition.[1] It is tempting to declare—as a result of this—without any apology (what is tacitly admitted already) that mathematicians are artists, born not made, and subject at times to insight differing but little from "revelation."

In any case, maybe a result of the kind of thought outlined above will be a closer understanding between the arts and sciences in the realm of creative instinct. There seems a tendency occasionally to overstress the differences between these types of human endeavour: they are probably at their most acute when seen in the company of applied science and technology: as might be expected, they diminish as the creative element predominates over the merely inventive.

The Historical Background

A good case could no doubt be made out for establishing—or at the very least for indicating—the historical approach to directiveness in painting before attempting even so tentative a methodology as that discussed in the foregoing section. To have acted thus, however, would have demanded more of the reader's patience than could properly be expected, for until comparatively lately there was no reason to believe that a creative movement of mankind such as we are contemplating now could seriously be regarded as amenable to a reaction such as (BS) → (SS) → (A) → (ES), let alone the five postulates concerning beginnings and goals. Thus, should the present position be judged interesting (if not completely tenable as it stands), we are naturally driven to ask to what initial circumstances so strange a view of pictures could possibly be ascribed. We try in fact to span the centuries in retrospect in the hopes of discovering the rock whence our theme is hewn. This endeavour will not, of course, be a brief history of painting ; on the contrary, it will be a brief history of thought, of that kind of thought which has evoked action of a superlative character. Its essence has always been akin to revelation rather than to reason, to synthesis rather than to analysis, to insight rather than to experience, to integration rather than to differentiation. What does all this mean ? Perhaps that the Universe is not, so to speak, a closed system, forbidding entirely the impact of events from outside—in other words, that strict determinacy seems unlikely. Centuries ago, Epicurus felt this—and allowed for it—in dealing with the consequences of the atomic theory of Democritus.

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[1] Sir Frederick Pollock, himself a senior wrangler in 1806, wrote of Samuel Vince, who was Plumian Professor from 1796 to 1822, that he (Vince) "was a bungler, and I think utterly insensible of mathematical beauty." Pollock was an old man when he penned this remark, which seems to suggest complete confidence to discern clumsiness and lack of aesthetic appreciation.

For Aristotle, *ἀιτία* or cause meant something altogether more expansive than our connotation of the word. Thus, he introduced (1) the *material* cause, (2) the *formal* cause, (3) the *efficient* cause, (4) the *final* cause. In our present context, these would be construed as follows—(1) would be the ingredients of a painting, *e.g.* wood, canvas, pigments, oil, resin, and so forth, (2) would be the finished picture, (3) would be the artist, and (4) would be the " message " or intention. This last is evidently teleological. The value of these concepts for our theme is readily seen to be in bringing the agent and his work into a more precise relationship than any other system is capable of doing. At the same time an axiological turn is given to the discussion by the presence of the *final* cause, since clearly the artist's purpose may have any degree of value from the most trivial to the most profound. Indeed, so vital is this factor in the assessment of great art, outstripping by far the relevance of mere technique within wide limits, that, conjointly with æsthetic accomplishment, it may be said to establish philosophically the worth of an artist's labours. In terms of the previous section, the end-state (ES) is uniquely determined by the Aristotelian *final* cause, rather than by the *formal* cause. It would need too long a digression into æsthetics to establish a more rigorous boundary between these two ; besides, our purpose at the moment is more in the connotative region than in the formal. Birkhoff [6] has demonstrated how it is possible to arrive at quantitative data in the latter field, but admits that such a result is unlikely for the former.

Nevertheless, the history of art, consciously or unconsciously, has shown great interest in, and concern for, the first three causes, even if their characteristic inter-relationships have not always been too well discerned. Thus, the *material* cause has long been studied for its rôle in fixing, or at least in indicating, both date and provenance. The corresponding truth-density involved is not of the highest, but enough is often obtained to provide a species of lemma upon which a proposition relating to the origin of a work or series of works can reasonably be founded.

The bond between *material* cause and *formal* cause is largely that of technique, whereas stylistics are now seen to be concerned more with the latter cause itself. This distinction seems to be of some moment, and perhaps for this reason. For many a year experts have busied themselves with trying to understand *how* a great artist (*e.g.* Rembrandt) achieved his results, whereas to-day some of the interest has shifted to asking *why* he employed certain means to secure certain ends [7, 8]. Epistemologically, building-up is conceived more as a *Gestalt, i.e.* as an integrative process, and less as a juxtaposition of materials, however skilfully arranged. In some such way as this, the scientist is becoming more sympathetic to " style," and the pure art critic less inclined perhaps to overlook the deeper levels of psychological activity inherent in the subconscious use of appropriate materials. It is worth noting how relatively little of value has come out of a detailed knowledge of the chemical constituents of a painting, whereas the impact of physical methods, linking up with the visual properties of paint-films, media and surfaces has been decidedly more encouraging for making common cause with the art historian.

Born out of strife, a really great picture appears as an event in the experience of mankind; yet there have probably been few epochs when travail of another sort was not being spent in an endeavour to understand supreme art, and to interpret it, as a harmonious background might do its lesser part in the general setting.

From such a position, it is a natural step to contemplate the *efficient* cause, *i.e.* the artist himself. Almost instantaneously one thinks of Leonardo da Vinci as the embodiment of the Stagirite's third cause. To the scientist, his passion for experiment is well known; what is less recognised, however, is his veneration for Archimedes and his outlook [9]. This may point to a sense of rebellion against the verbal futilities of the schoolmen in degeneracy, coupled with a desire to submit everything to the test of practice. When one realizes what difficulties Leonardo overcame in his efforts to study the works of Archimedes, it is obvious that a mental end-state (ES) was being sought out *en route* as it were to yet more distant goals. In fact, a very definite teleological principle is seen to be at work in the master's mind, as he laboured and suffered.

In recent decades the relationship of art to philosophy has been examined anew in connection with Husserl's Phenomenology [10]. There are several important aspects, but two may be selected as indicative of directiveness, even though they were not probed specifically with that intent. They are *purity* and *greatness*. A short discussion of each of them may fittingly conclude this section dealing with the historical background of our principle.

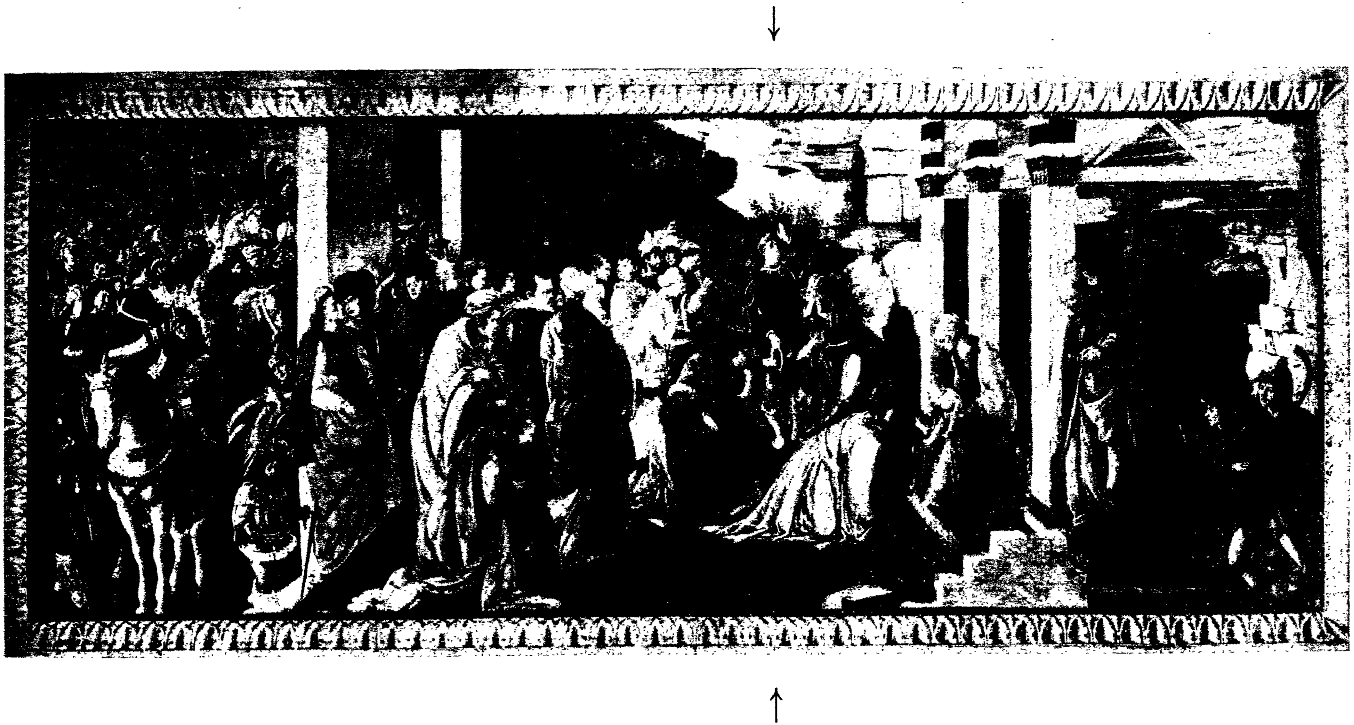
Purity.—Accuracy of focus—both in the choice of idiom and in the factual implementation of the theme that symbolises the contours of feeling—affords some estimate of the degree of purity attained in a painting. Herein the Aristotelian transition from potency to actuality is seen at work. Looked at from outside, the process is not unlike the phenomenological one which discards what has been called " the straightforward evidences of naïve ontologies," and tries to trace them backwards, finally to anchor them securely in the intentional context.

Greatness.—Liberation of the depth of being occurs in a great picture. The question is how deeply and how honestly it is expressed in the actual work. In a way analogous to purity, greatness is registered—and perhaps its degree assessed—by how successful it is in giving to the mind somewhat to have and to hold " of things unknown." Once again, a teleological tendency is present, this time in increasing what might be called the resolving power of the mind. Otherwise expressed, the artist's goal is the refinement of thought, vague and fuzzy originally, into a balanced whole, disciplined and purposeful. The aim is resolution rather than magnification.

So much for a rapid glance at the course of events which, for better or for worse, forms the *Hinterland* of a directive approach to paintings. It will be observed that the modern and somewhat unorthodox principles in view by no means imply that they are intended particularly for contemporary art: on the contrary, it seems to the writer that such concepts have been " occluded " for centuries in such a system as the *philosophia perennis*, and that they are most appropriately illustrated by the great masters of classical painting.

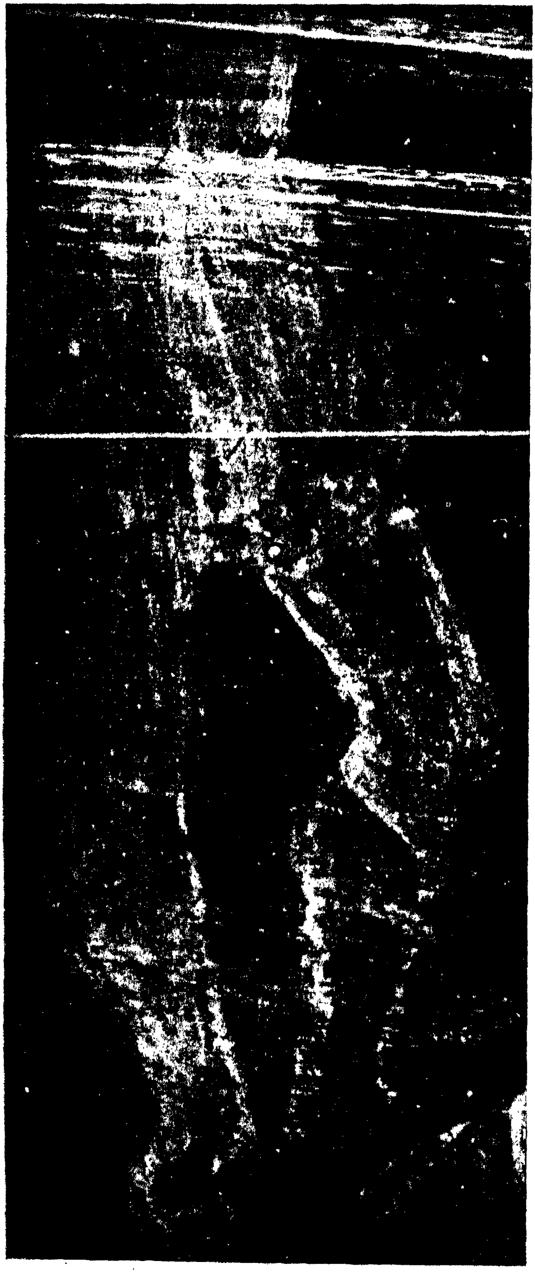
For ourselves, the value of these studies lies in just that property

PLATE I



“The Adoration of the Magi,” by Botticelli.

(Courtesy of the Trustees of the National Gallery.)



X-radiograph (14 kv., 15 ma.). Actual size, showing part of the figure indicated by arrows in Plate I. Observe the tentative outlines of his left thigh and foot —also (dark) incisions on right leg.

(*Courtesy of the Trustees of the National Gallery.*)

of scholasticism which has always been its outstanding merit, namely its comprehensiveness, that is, its ability to include such diverse accomplishments as philosophy, natural science, the fine arts and the rest within its abundant charity. In a world immersed in technology and troubled by its powers, the suction of art into one great corpus of knowledge may yet be advantageous.

An Example of " Directiveness " : " The Adoration of the Magi," by Botticelli

This is an early work, and examination shows a number of features illustrative of the functional approach already fully discussed. Plate I is a photograph of the whole picture, the dimensions of which are 20 inches by 55 inches. There are a number of incised lines which are found not only at or near the edges of architectural details, but also appear in hands, faces, garments and so forth. Naturally the purpose might be to accentuate contour, where they seem to have been scratched into semi-dry paint, but it is more likely that at least in some instances where as many as three such lines are present they represent successive attempts to improve the design as it progressed. In other words, the whole was not conceived and acted upon with mechanical rigidity, but evolved —in finer points at least—as the artist felt inclined. Incidentally, there is a sword or sceptre (incised) which never materialised in paint. Of course, much of this may be little else than carelessness, but it is interesting to reflect that effects of this kind are consistent with our theorem according to which if ends are not attained by one method, another is introduced until the agent is (in theory) finally satisfied. Again, with the sceptre, it may have been adjudged irrelevant, and therefore left unfinished. In all these modifications, according to our interpretation, there is a decrease of free-energy as the painting approximates ever more closely to the state intended.

From here is an easy step to consider the manner of the preliminary drawing-in. The thinned (black) paint lines do not follow the incisions, but give the impression of being more in the nature of tentative affairs, in continuous effort to improve upon the design. Sometimes there are several such strokes, analogous to more than one incision. The contrast between these gropings and the characteristically hard outlines of the finished picture suggest some kind of hesitancy, or trying one way after another to obtain the desired result (Plate II).

It has been pointed out by Ruhemann [11] that this may constitute a real difference between the true painter and the artisan, or copyist, in that the former will work out his purpose dynamically, even though everything was "all set" in his preliminary sketch or cartoon.

Broadly, our example demonstrates the Russell theory in action. A definite end-state or goal exists in the artist's consciousness, and he reaches it by any means available to him once he has set out upon his journey. If one path is blocked, he tries another, and not only so but he seems to realise that modification in one particular implies—perhaps even demands—consequential changes in another (a *Gestalt* property), so that the inevitable condition of minimal free-energy should at length be reached. The whole process is seen to be analogous to the adjustment of currents in the arms of the Wheatstone bridge: whatever is done is subject to the ultimate energy distribution in the network, an example of minimal "requiredness."

There seems little doubt, to judge from a number of pictures investigated, that these alterations, varying from minor outlines to large shifts (including *pentimenti*), occur very frequently amongst the works of the great masters. They represent a type of vacillation of great psychological interest, clearly connected with a certain humility of brain and hand, in contradistinction to the cocksureness of the imitator. Although many of these effects can be seen with the naked eye, scientific (especially physical) methods can often lift them above any suspicion of mere subjectivity.

We have travelled some way along a none-too-easy road, maybe not without stumbling, and certainly not without labour. A moment's pause now at the end to see what, if anything, we have accomplished. Starting out from a certain parallelism between potential theory and the implications of the *Gestalt* concept (and recollecting that all maximum-minimum principles are to some extent teleological) we have applied the symbolism of (organic) directiveness to the creation of paintings, as indeed such symbolism is itself most naturally orientated towards biological ends, purposeful or automatic. And so, a retrospect over the centuries brought the result, almost obvious at the beginning, that we should find the stirrings of such an outlook in the giant intellect of Aristotle, who gave to mankind the first *Methodik* of causality and the urge to use it for the interpretation of function. This indeed is what we want, a system patient of mathematical reasoning yet independent of mechanics. For try how we will, art refuses, utterly and always, to be fettered by a concatenation of materialism. Finally, an example of tentative morphoplasticity is furnished by a consideration of Botticelli's "Adoration of the Magi."

Through some such strife may come glimpses of that First Cause Whose way is in the sea, Whose path is in the great waters, and Whose footsteps are not known [12].

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THE VENETIAN LAGOON

AN HISTORICAL AND BIOLOGICAL SKETCH

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ORIGINS

THE amphibious conditions of life which determined the origin and history of the great Venetian Republic are a matter of common knowledge. But the physical and biological factors upon which these conditions are based are not so generally realised, though much has been written on the subject, chiefly in Italian, during the past four centuries.

The low-lying north-eastern Adriatic coast between Ravenna and Trieste is so jigsawed with lagoons, sand-bars, estuaries and islands that a coastline is an arbitrary abstraction. But, strange to say, this at first sight wild and "natural" coastal configuration is the result of centuries of struggle between man and the forces of nature. There is a state of precarious equilibrium maintained by constant vigilance and effort directed primarily, not as in Holland to the reclamation and preservation of land, but to the protection of the sea against encroachment by the land, or rather the coastal lagoons and channels from extinction by silting. The purpose of this struggle is and has always been twofold—the maintenance of navigation channels and the preservation of the lagoon fisheries.

The lower Po Plain (Padana) and the Venetian Coastal Plain (Veneto) have been formed by extension into the sea of alluvial deposits eroded from the Alps and northern Apennines, a process which had of course been in operation long before man became a factor in the situation. The configuration of the coast was then determined by a balance between alluvial deposition by rivers and the action of winds and tides. The prevailing winds being from the north-east and thus parallel with the general coastline, outflowing sediments were accumulated as sandbars which ultimately formed elongated islands (*lidi*), leaving shallow lagoons between them and the mainland. Such lagoons are ultimately due for extinction by further silting unless this can be artificially prevented.

The natural result is the blocking of the river mouths which must then break through to reach the sea at some other point. Over many thousands of years these forces have operated to extend the plain into the sea, and striking changes have been recorded in historic times. Ravenna, now some 6 km. from the sea, was a seaport in Roman times ; the Po delta has advanced into the sea and its mouths, now six in number, are constantly moving, the main stream having been diverted from the Po di Tolle to the Po di Pila since 1900.

Such then was the origin of the Venice Lagoon, all but separated from the sea by two elongated islands (the Lido and Pellestrina) which were formed by the prevailing currents from the sediment deposited by the rivers Piave, Sile and Brenta. It has survived during historic times only because the prosperity and indeed the very existence of the Venetians required that it should, and means were found to preserve it.

PRESERVATION

Both the near-by swampy mainland and the islands of the Lagoon were inhabited before Roman times and the Romans started the work of controlling the rivers by the construction of canals. But with the collapse of the Roman Empire and with the barbarian invasions from the north, these works fell into decay and were not effectively revived until some centuries later when the Venetian Republic had been firmly established. It was in fact the periodic threat of invasion which helped to make and consolidate Venice as a great power. The population of the islands was increased by refugees from the mainland towns ravaged by the Huns in the fifth and by the Lombards in the sixth century. In the ninth century the almost successful invasion by the Franks stimulated the unification of the Republic and the concentration of the administration on the islands of the Rialto. But the emergence of the City and State of Venice, with all the cultural and economic consequences for the world during the coming centuries, was only partly assured by political unification and the removal of the threat of invasion. Its power and prosperity must of necessity be founded upon the sea and overseas trade. But the Veneto coast is naturally most unfavourable for navigation, and the port of Venice was under constant threat of extinction from the silting of the Lagoon by the three rivers. The struggle against these rivers by the construction of diversionary canals was begun in the thirteenth century and has continued to the present day (Fig. 1). The Brenta and Piave transport immense quantities of sediment from the Alps during the

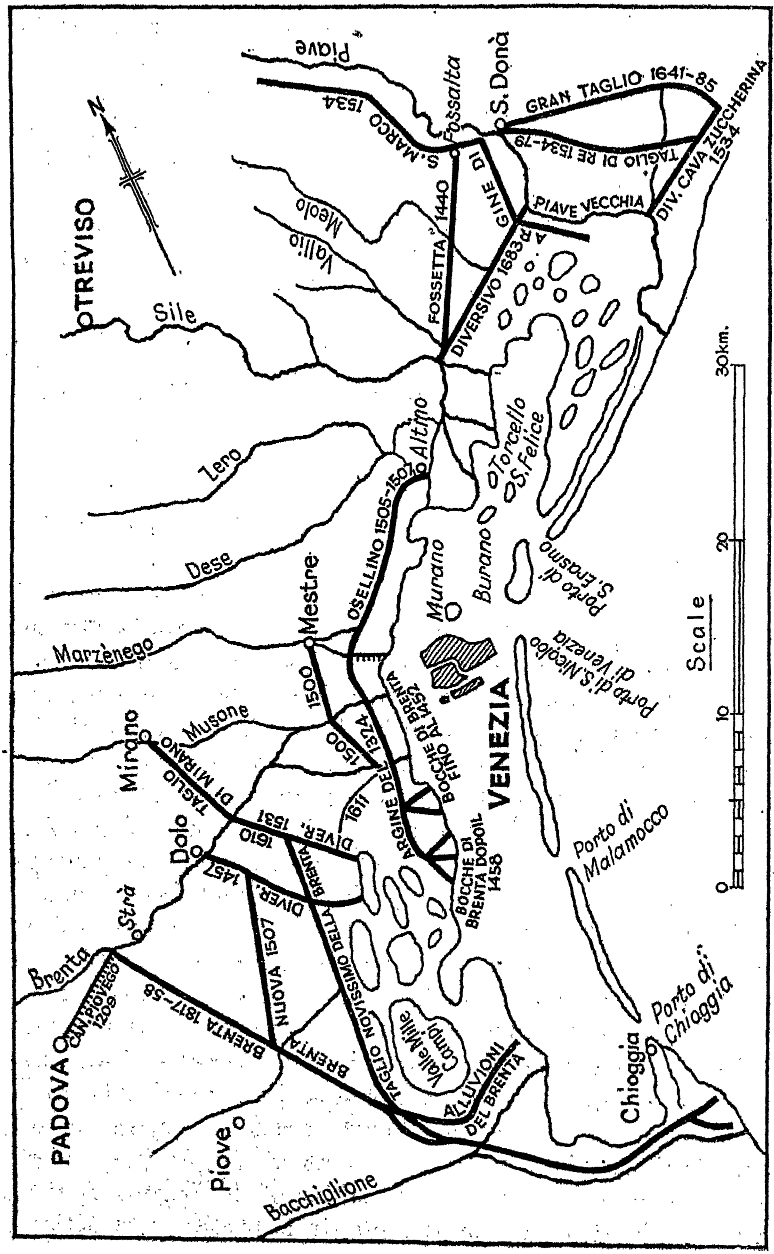

FIG. 1.—Map showing the canals constructed from the fourteenth century onwards to divert away from the lagoon the rivers Piave, Sile and Brenta.

(*Reproduced from "Bonifica e Vallicoltura," Rome, 1937, by kind permission of the Magistrato alle Acque, Venice.*)

spring floods, and the Sile rises from copious deep cold springs above Treviso and is thus in constant flow. By the progressive building of a canal system the three rivers, which originally discharged into the Lagoon, are now diverted into the sea, the Brenta to the south and the Sile and Piave to the north (Fig. 1). The mouth of the Brenta, which until the mid-fifteenth century opened into the Lagoon at Fusina close to Venice, was progressively moved farther south and was finally expelled from the Lagoon at the end of the last century by a canal to Brondolo south of Chioggia, in all a distance of about 30 km. There now remains only the Dese flowing into the Lagoon opposite the island of Torcello, but the amount of sediment involved is insignificant. As well as the main diversionary canals there is an intricate system of inland waterways connected to the Lagoon by locks. Most of the vegetables and fruit grown in this area are transported to the Venice market by boat. There are also connections between this inland water system and channels which skirt the edge of the Lagoon to give a supply of fresh water for operating the fishing *valli* (see below).

The diversion of the rivers was only one of the necessities for the preservation of the Lagoon in a navigable condition. Dredged channels had to be maintained to connect the entrances (*porti*) with Venice and the other islands, and the *porti* themselves were in danger of blockage from sediment washed down the coast from the new mouth of the Piave. The *porti* were finally protected by moles, and liners can now reach the port of Venice through the Porto San Nicolo. It was also necessary to stabilise the external islands (*lidi*) which were constantly eroded by winds and currents and whose stability was essential for the protection of the Lagoon. They were originally strengthened by wooden stakes but, after catastrophic damage by storms in the eighteenth century, these were replaced by dykes (*murazzi*), reinforced with stone blocks transported from across the Adriatic, by which about 20 km. of the external coastline is now protected.

Yet another threat came from the Lagoon fisheries which soon became, as they are still, an important local industry. The peculiar method of fishing evolved on this coast, which is described later, involves the enclosure of large areas of the surface of the Lagoon (*Valli da Pesca*). The unrestricted enclosure of *valli* began to hinder the free flowing of the tides and thus to threaten the navigation channels with silting. As early as the fourteenth century the government of Venice faced this problem and passed laws regulating the enclosure of *valli* and the opening of sluices and obtained powers for expropriating and destroying *valli* which were directly threatening the shipping ways. This conflict of interests continued until recent times and the present police regulations limiting the abuses of the *vallicolturi* for the benefit of shipping were drawn up under the Austrian régime in 1841.

Hydrology

All affairs relating to the hydrology of the lagoons and inland waterways of the Veneto coast are now under the supervision of a special government department in Venice, the Magistrato alle Acque, under whose auspices a survey of the hydrography, fisheries and hygienic problems was published in 1937 (Bonifica e Vallicoltura).

The area of the Venetian Lagoon is approximately 1000 sq. km. It is very shallow—up to 20 m. in the main dredged channels, tailing off to 1–3 m. in the smaller branch channels, outside of which most of the water, especially in the *valli*, is less than 1 m. deep, and extensive mud flats (*barene*) are exposed at low tide. The tidal amplitude on this coast is 60–90 cm., which owing to the shallowness of the water has a considerable effect on the external regions of the Lagoon (*Laguna viva*) but has little influence on the landward periphery (*Laguna morta*).

The physico-chemical conditions have been studied by Picotti (1935, 1936, 1939). In general the salinity decreases towards the landward periphery owing to fresh water from the land, but except very locally (*e.g.* at the mouth of the Dese) there is never more than a 50 per cent. dilution. The biologically important characteristic of the Lagoon as compared with the sea outside is the wide fluctuation of both salinity and temperature. These features are shown in the accompanying table. In midsummer the salinity of the Lagoon approaches that of the sea, which is of importance in connection with the migration of fish (see below). In some of the *Valli da Pesca* the salinity in midsummer may exceed that of the sea and in some of the extreme peripheral shallows the water approaches saturation.

TABLE

(Approx. mean salinity and temperature figures derived from Picotti (1935 and 1936).)

	N. Adriatic.	Inner Lagoon.
Salinity :		
Mean ann. max. .	38 per thous. ($^0/_{00}$)	32 per thous. ($^0/_{00}$)
Mean ann. min. .	37 ,, ,,	24 ,, ,,
Range	1 ,, ,,	8 ,, ,,
Temperature :		
Mean ann. max. .	22° C.	25° C.
Mean ann. min. .	10° C.	5° C.
Range	12° C.	20° C.

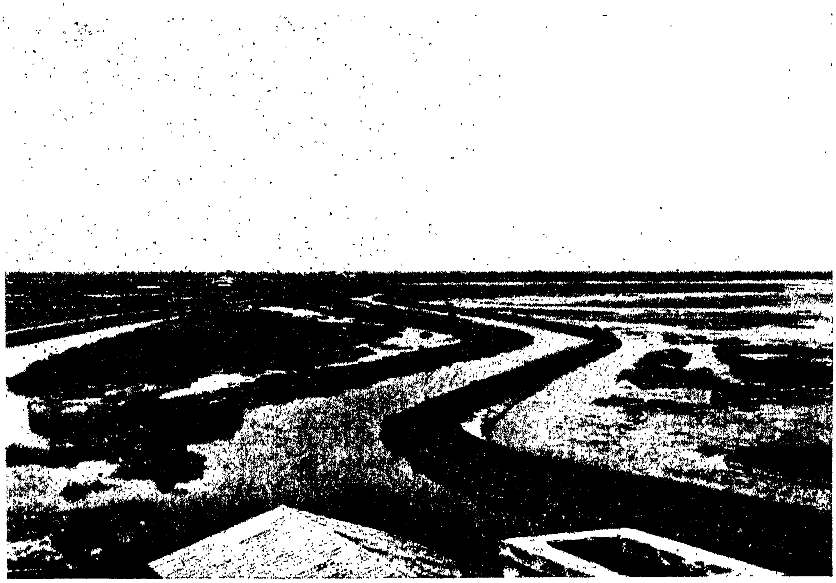

Fig. 3.—Main channel from the open lagoon skirting a *valle* (on right) to connect with another *valle* further inland. Note low-water level in *valle* (August) in preparation for the September fishing operations. View from the roof of a *Casone di Valle*. Sailing barge in middle distance.

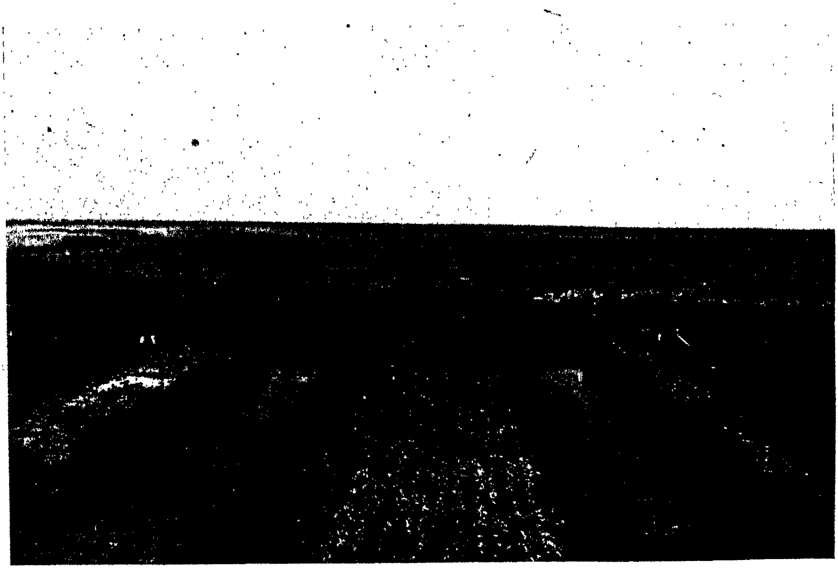

Fig. 4.—*Paschiere*. Deep channels protected by *Tamarix gallica* in which the fish spend the winter in relatively warm water. Maize grown by irrigation between the *Tamarix* hedges.

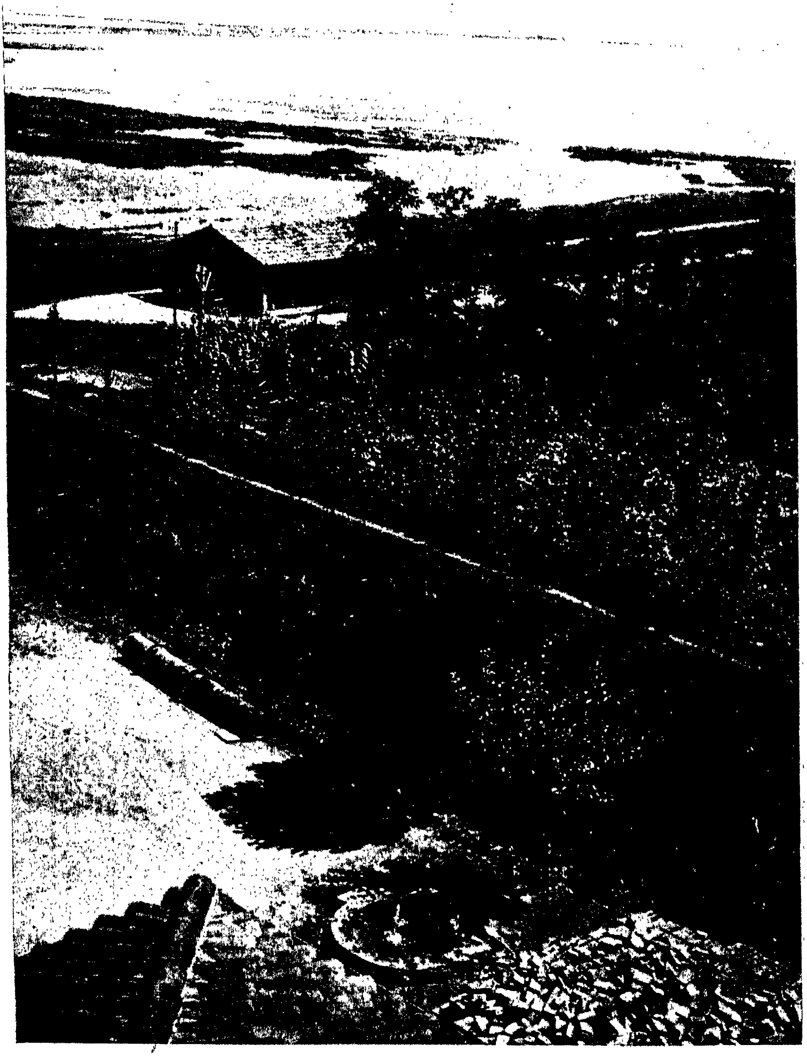

FIG. 5.—View of *valle* from housetop.

From horizon backwards:

(*a*) Main lagoon.
(*b*) Outer dyke (*argine*) of *valle* with main sluice and traps between the two houses.
(*c*) The *valle* with water level low (August).
(*d*) Mudflats and exposed *Salicornia* beds.
(*e*) Vegetation surrounding the *peschiere*.
(*f*) Salt water channel.
(*g*) Artesian well (fresh water).

The problem of fresh water for the inhabitants of the promontories and islands has been solved by drawing from both surface and underground sources. The channels supplying the *valli* with fresh water, mentioned above, also serve to irrigate crops on the mainland and on the spits which project into the Lagoon on which the custodians and employees of the *valli* live. Some of the larger inhabited islands, including Venice itself, are supplied with water piped from the mainland. But much of the fresh water for agricultural and domestic purposes both in and around the Lagoon is obtained from artesian borings. The fresh is thus brought up from below the salt water of the Lagoon, and maize and vegetables are grown in a foot or two of soil above a salty substratum (Figs. 4 and 5). This artesian water is peculiar in containing a high concentration of methane derived from the older underlying alluvial beds which, like those at present being deposited on the floor of the Lagoon, are rich in organic matter. In the Padua-Vicenza region the methane is actually extracted on a commercial scale for heating and for use as a motor fuel. Towards the close of the campaign last year on the Veneto coast the Germans were exploiting to the utmost this local source of motor fuel.

To few of the countless foreign visitors to Venice is the Lagoon more than a rather grim setting to the jewel which is Venice, and the approach to the causeway through the industrial area of Mestre is likely to discourage further investigation of a coast which is in fact both interesting and beautiful. The colours which in midsummer fringe some of the remoter regions of the lagoon—the red-browns and greens of the *Salicornia* beds, the vast purple sheets of sea lavender (*Statice limonium*), backed by a great expanse of plain relieved by the campanile of an island church and the red sail of a barge half hidden by a bank of reeds, and behind it all the Dolomites raising their grotesque limestone peaks above the distant haze—such is the landscape of the Veneto coast.

FOUR CENTURIES OF NATURAL HISTORY

From Padua to Venice is barely 25 miles. We are thus on the threshhold of the land which witnessed the renaissance of European science and medicine. Some great names are therefore to be expected in the long list of naturalists who have studied the Lagoon. An historical account has recently been published by Minio (1938). Owing no doubt to its inaccessibility, naturalists were slower to investigate the Lagoon than the mainland, and the study of the plant life was begun almost two centuries before serious attention was given to the animals. Among the botanists who laid the foundations of the recorded flora during the sixteenth to early eighteenth centuries are Michiel (1510–76) who composed the earliest extant flora with about a thousand coloured illustrations, Matthioli (1500–77), Donati (1606–59), Zannichelli (1662–1729) and Micheli (1679–1737). Goethe, who was in Venice in 1786, recorded observations on the animals and plants and actually was the first to suggest a connection between the succulence of halophytes and the saltiness of their environment (*Italienische Reise*, Oct. 8, 1786).

Symptomatic of the times was the publication in Venice for twelve years (1765–76) of a journal entitled *Giornale d'Italia spettante alle Scienze Naturali*. This was edited by Grisellini (1717–83) who was himself a good naturalist and made observations on the occurrence and habits of marine animals. But the foundations of systematic zoological study were laid by Chiereghin (1745–1820) and Olivi (1769–95). Chiereghin was a priest who gave all his spare time to natural history. His *Descrizione de' Pesci, de' Crostacei, e de' Testacei che abitano le Lagune e il Golfo Veneto* in twelve volumes with coloured illustrations was the first comprehensive zoological work, but it was never published and is preserved in manuscript. Olivi of Chioggia was a remarkable genius who, owing to ill health, spent the penultimate five of his twenty-six years of life in retirement and philosophic meditation. He was a naturalist at an early age and at fourteen he inspired a group of fishermen for three years to collect specimens and information on the habits of marine animals. His last five years were spent at Padua where he studied his material and developed his ideas with striking originality. His main work *Zoologia Adriatica*, which was never finished, was both a fauna and a study of the biology of the animals mentioned. Before his death he proposed a scheme of work to the Societa Venetiana di Medicina entitled *Tipografia zoologica e fitologica dell 'Estuario*, some of which is quoted by Prof. Minio (*loc. cit.*). He proposed to investigate the organisms in relation to various types of habitat which differed in respect of certain factors, *e.g.* extent to which inundated by tides, depth and salinity of water, etc. He thus foretold the methods of the modern ecologist, but did not live to pursue them. He is also regarded as one of the founders of biometry.

Renier (1759–1830), also from Chioggia, who succeeded Vallisnieri as professor at Padua, did much to increase the knowledge of the invertebrate fauna of the Lagoon, and the greatest of eighteenth-century Italian biologists, Spallanzani (1729–99), whose work on the biology of the eel was done on the Commacchio Lagoon south of the Po, visited Venice on several occasions while Professor at

Pavia. At Chioggia he made some of his celebrated observations on the production of electricity by the Torpedo Ray.

In the eighteenth century, Chioggia was the centre of biological studies. In the nineteenth it moved to Venice, and Contarini (1780–1849) and Nardo (1802–77) were the leading lights. Contarini, a country gentleman with estates on the mainland, was a keen sportsman and indefatigable walker, was intimate with most of the farmers and fishermen of the district and knew every inch of the countryside. He studied and wrote about Actinians, Insects, Arachnids and Birds and was the first notable ornithologist. He contributed sections on these groups to the monograph *Venezia e le sue Lagune* (1841). The section on the marine fauna was written by Nardo, whose most important original research was on Sponges and Cœlenterates. But Nardo was a physician with very wide interests who contributed to other branches of zoology as well as to Algology, Anatomy, practical fishery problems and to bibliographical studies of the works of previous naturalists. His house was the meeting-place of the biologists of the time.

Another important figure was the cultured German von Martens (1788–1872), born and brought up at Mira on the Brenta and a friend of Contarini. His *Reise nach Venedig* (1824) was the first general physical and biological account of the region. Of the many other naturalists interested in the Lagoon during the nineteenth century mention should be made of Zanardini (1804–73), algologist, who wrote the botanical section to *Venezia e le sue Lagune*, and Ninni (1837–92) who studied birds and fish as well as practical fishery problems and collaborated with Saccardo, a mycologist, in a contribution to the monograph *La Provincia di Venezia* (1880).

RECENT ECOLOGICAL WORK

In recent times biologists have worked largely on ecological problems, such as the distribution of species with reference to environmental factors. The magnificently produced monograph *La Laguna di Venezia*, now in course of publication, edited by Brunelli, Miliani and Orsi, so far includes volumes on the History, Meteorology, Botany and Fisheries. The zoological sections are in course of preparation.

A general idea of modern ecological problems can be got from Brunelli (1923, 1934) and D'Ancona (1941). The biologically significant features of the Lagoon are (1) its shallowness, (2) the muddy bottom, (3) the fluctuating temperature and salinity (see above), and (4) the high organic content of water and mud. This last is in part due to the unstable conditions and the consequent high death rate among most of the organisms coming from the sea, in part to the intensive cultivation of the islands and surrounding land as well as to the very large population, including that of Venice (*c.* 250,000), whose sewage is discharged into the Lagoon. The ecological situation is in fact determined in several ways by the works of man. Apart from the buildings, piles, sluice-gates, sea walls, etc., there would have been little opportunity for the establishment of the barnacles (*Balanidæ*), zoophytes, algæ and other organisms which are attached in vast numbers to such structures all over the Lagoon and which present some interesting problems (Brian, 1938; Rigo, 1942). The swarms of *Ligia italica* (Crustacea, Isopoda), which scurry like cockroaches into the crevices of the house fronts when disturbed by the splash of a passing gondola, are perhaps the only marine animals normally observed in a state of nature by the modern pilgrim to San Marco, and these owe their habitat to human agency.

The fauna of the Lagoon is primarily marine, poor in species but rich in numbers as compared with the sea, and many species are only temporary visitors. D'Ancona (1941) suggests that the fluctuating conditions of temperature and salinity select those species which can stand such extremes, and for these it affords a very favourable environment. But for the preservation of new mutations and thus for the appearance of truly autocthonous elements in the fauna, a more stable environment is required. There is presumably another selection factor at work, namely the nature of the food supply. Shallow muddy water with an abundance of decaying organic matter is naturally populated by scavenging animals. Thus the bottom living crustacea, *e.g.* crabs such as *Carcinus mœnas*, *Idotea*, etc., gastropod molluscs and polychæt worms are very abundant and account for the large numbers of migratory birds which feed on the Lagoon's edge, especially during winter.

Of the fifty-three known species of Polychæts, *Nereis cultrifera* is the most abundant, but the most curious is *Merceriella enigmatica* (Serpulidæ). This is a tube-living form and was first noticed in 1934 but is now abundant in many parts of the Lagoon. It was originally discovered in a canal in northern France in 1924 and appears to be spreading, having since been recorded from brackish waters in Spain, North Africa, Black Sea, India and U.S.A. (Fauvel, 1938).

The plankton is relatively poor except in the main channels of the *laguna viva* where the organisms are continually being washed in by the tides (Issel, 1935). But, as might be expected, in shallow brackish water rich in nutrient salts there is ample opportunity for the growth of algæ. The Lagoon is, in fact, an algologist's paradise, and full advantage has been taken of the facilities for the study of systematics and ecology (see Schiffner & Vatova, 1938; Zanon, 1938; and Sighel, 1938). The number of recorded species and varieties are Chlorophyceæ, 59; Mixophyceæ, 5; Phæophyceæ, 19; Rodophyceæ, 90; and Diatomaceæ, 498; which include several endemic species. *Enteromorpha*, *Chætomorpha* and *Polysiphonia* are among the commonest types and *Gracilaria* is collected for the production of agar. The mud just above water on the inner periphery of the Lagoon is often coated with *Vaucheria* to give a deep green in contrast to the red-brown of the *Salicornia* in the background. Prodigious numbers of diatoms are to be found amongst the submerged algæ and in the extensive beds of sea grass (*Zostera marina* and *nana*) and *Ruppia maritima*. The less saline regions and channels in summer are often choked with *Potomogeton pectinatus*, and the peripheral fresh-water channels with several other species of *Potomogeton* and *Elodea canadensis*.[1] The submerged vegetation harbours an abundant fauna particularly of small Crustacea (Brian, 1939, for Amphipoda) and in the less saline regions insect larvæ. An interesting comparative study of two contiguous regions of different salinity was made by Schreiber (1930).

MALARIA

Ecological work on the lagoons of the Veneto coast has been stimulated by two important practical considerations—the prevention of malaria and the maintenance and improvement of the fisheries. Malaria is and has always been the most serious problem of local hygiene, and the region is one of the most highly malarious in Europe.[2] But it is a striking fact that the area of intense malaria is a narrow coastal strip only a few kilometres wide, which stretches with few breaks from Ravenna to Trieste. This is satisfactorily explained as being determined by the distribution of the particular mosquito which is responsible for transmission (Sepulcri, 1936). Northern Italy harbours several species and varieties of Anopheline mosquito, but only one, *Anopheles sacharovi*,[3] owing to its partiality for human blood, is a serious vector of malaria, as it is also in south-eastern Europe and the Middle East. It is necessary therefore to study the factors determining the distribution of this mosquito. The apparent association of intense malaria in Italy with the proximity of coastal salt marshes has long been recognised, and recent surveys on the Veneto coast (*e.g.* Corradetti, 1937) have shown that *A. sacharovi* is never found in any numbers far from brackish water. It is also known that it will breed in brackish water and that the larvæ survive and develop in water containing 1·7–2·0 per cent. salts or approximately half the salinity of sea water. But, since *sacharovi* cannot be recognised as the larva and must therefore be reared to the adult, thorough surveys of the breeding-places have not yet been made. It seems, however, that a simple solution to the problem in terms of salinity is not likely. In the Middle East there are some highly malarious inland areas where *A. sacharovi* is the vector and is breeding in purely fresh water. And a survey which we made last summer (1945) on the Veneto coast showed that it was breeding as much in fresh as in brackish water. But the fact remains that these fresh-water breeding-places were close to the salt marshes, though chloride estimations showed that there was no actual contamination with sea water. Whatever the nature of the conditions which favour the breeding and development of this mosquito, they are certainly in some way associated in Italy with salt marshes. But it is not essential that the breeding-places themselves should be brackish, and water more saline than about 50 per cent. sea water is definitely toxic to the larvæ. The ideal conditions, which may also be associated with climatic factors, are found around the inner periphery of these coastal lagoons, and with a high summer temperature and a dense agricultural population the stage is set for intense malaria.

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[1] For a good account of the systematics and ecology of the flowering plants, see Beguinot (1941).

[2] Benign tertian malaria (due to *Plasmodium vivax*) predominates, but the more serious malignant tertian (*P. falciparum*) appears in the second half of the summer and in some places accounts for more than 30 per cent. of the cases (Missiroli, 1933).

[3] Previously regarded as the variety *elutus* of *Anopheles maculipennis*. The five European varieties of *maculipennis*, all of which are found in Italy, are distinguished only on the patterning of the eggs. Var. *elutus* (*A.*

A difficulty for investigation arises from the scarcity of breeding-places which was also noted by Corradetti (1937). The introduction of the larvivorous fish *Gambusia holbrooki* has been so successful that larvæ are rarely found in the canals and interconnected channels. We found them only in isolated pools and ditches which were for some reason cut off from the main channel system and thus inaccessible to the *Gambusia*. But the high concentration of *A. sacharovi* which could be produced from scanty and scattered

sacharovi) is, however, recognisable as the adult. Var. *labranchiæ* is a serious vector in southern Italy and French North Africa. Var. *atroparvus* is mainly responsible for the small amount of malaria now remaining in northern Europe and was probably the chief transmitter of northern European "ague" in the past.

breeding-places was very striking and has its moral for the control of malaria. Destruction of larvæ must be very thorough to be at all effective.

The government scheme for the investigation and control of malaria on the Veneto coast is centred on the Istituto Antimalarico in Venice. It comprises a network of outstations concerned with the collection of statistics on the prevalence and distribution of the disease and of the anopheline mosquitoes, with treatment and prophylaxis and with mosquito control. By these means the situation had steadily improved up to the beginning of the war and certain areas had been entirely freed from malaria. But during the last two years of the war the organisation quite naturally began to break down, mainly through lack of materials and transport, and to the flooding of reclaimed coastal land by the German army in anticipation of allied landings from the sea. As a consequence there was a considerable increase in malaria in the summer of 1944.

It was fortunate that the allied armies arrived in the Po valley and Veneto plain and that the war ended in the spring of 1945, more than two months before the beginning of the malaria season. Malaria, rather than "military necessity," was now the major factor in deciding the detailed location of the occupying troops. Another favourable circumstance was the unusually dry summer, favourable unfortunately in this one respect only that the anopheline breeding-places were thereby reduced, and the civilian malaria figures were consequently far lower than had been expected in view of the breakdown in the antimalaria organisation.

As far as can be seen at the moment the modern development of insecticides, such as D.D.T., will in the future throw the emphasis primarily upon the destruction of the adult mosquito as the main means of combating malaria in southern Europe and the Middle East. During the winter the vector species hibernate as the adult in well-defined resting-places (stables, etc.) and can thus be attacked *en masse*. To what extent the study of the breeding habits of these mosquitoes will thus be rendered "academic" remains to be seen.

THE FISHERIES

There is no indication, however, that the practical problems connected with the Lagoon fisheries are in any danger of sudden solution by some dramatic technical development. The biological ramifications and the still unopened fields for investigation are numerous and of great interest. The physico-chemical conditions in the water and mud in relation to climatic and tidal phenomena, the biology of the plankton, of the algæ and of the bottom living invertebrates—these are subjects already briefly indicated which are basic to the study of the productivity of the Lagoon, and in several directions research on modern lines has yet to be begun.

It should be emphasized that the Adriatic lagoon fisheries are very extensive and of considerable importance. They extend along some 200 km. of coast from the lagoon of Commacchio (noted for the classical work of Spallanzani and Bellini on the life history of the eel) to Grado, about 30 km. short of Trieste. The total area of the *valli* or " cultivated " water exceeds 100,000 hectares (247,000 acres). It has been estimated that the average yield of fish from all the lagoons is approximately 60–100 kg. per hectare per annum or 55–90 lb. per acre of water surface (Bullo, quoted by D'Ancona, 1938). The average annual eel catch in the lagoon of Commacchio between 1899 and 1905 was about 600,000 kg. (approx. 600 tons) which represented about 80 per cent. of the total fish caught (Henking *et al.*, 1908). The productivity of the Venetian Lagoon is now estimated at approximately 1,500,000 kg. of fish per annum (1500 tons). In normal times the fish are transported fresh and preserved all over Italy as well as being sent to other parts of Europe.

To a greater extent than the rest of the fauna, the fish reflect the unfavourable nature of the Lagoon for permanent colonisation, except by a few species which are particularly adaptable (D'Ancona, 1938). About 85 species in all have been recorded, but 10 of these, including the tunny, the shark *Carcharinus milberti* and the sunfish *Mola mola* are rare and accidental immigrants. About 15 species including the Anchovy (*Engraulis encrasicolus*) and the mackerel (*Scomber scomber*) enter the Lagoon during the summer months in search of food at a time when the salinity is approaching that of the sea (see Table), and depart at the approach of the rains and the colder autumn weather.

More than 20 species remain longer, some spending several years, during which they feed and mature, and return to the sea only for breeding. Amongst these are the species which are fished commercially, the chief of which are the eel (*Anguilla anguilla*), the grey mullets (*Mugil cephalus*, *capito*, *auratus*, *saliens* and *chelo*) and the " Orada " (*Sparus auratus*). The smelts (*Atherina mochon* and *boyeri*) and the bass (*Morone labrax*) are also fished.

The following only can be considered as truly autocthonous, *i.e.* they breed in the Lagoon—2 species of goby (*Gobius jozo* and *ophiocephalus*), the sea horse (*Hippocampus guttulatus*), 2 species of pipe fish (*Syngnathus typhle* and *abaster*) and in the periphery of the Lagoon the Sticklebat (*Gasterosteus aculeatus*) and *Cyprinodon fasciatus*, a well-known coastal and inland brackish water fish in southern Europe and Africa. The few fresh-water fish found at the edge of the Lagoon, such as the pike (*Esox lucius*) and the tench (*Tinca tinca*), are accidental immigrants during floodtimes and are not found in the more saline regions.

The Lagoon is thus a temporary but rich feeding-ground for Adriatic fish similar to the Sea of Azow and its associated lagoons (Limans) for the fish of the Black Sea, and to the Zuider Zee (before its enclosure) for those of the North Sea.

The method used for the large-scale rearing and trapping of fish is known as *Vallicoltura*, and is of very ancient origin, the first records dating from the eleventh century. During the nineteenth century a study of the technique was made by Coste (*Voyage d'Exploration sur le littoral de la France et de l'Italie*, Paris, 1861). There is much variation in detail but the basic principle is as follows. A portion of the Lagoon, usually several square kilometres in area, is enclosed by banks (*argini*) or stakes (*arelle*) but can be opened to the main lagoon through sluice gates (*chiaviche*) and to the inland fresh-water canal system (*acquadore dolce*). A piece of water so enclosed is known as a *Valle da Pesca* and the type which is enclosed by banks (*valle arginata*) is shown in Fig. 2. The average *valle* is tended by 4–6 men under a foreman, who live on the spot in houses built on the banks or spits of land projecting into the Lagoon. There is often also a house for the owner or manager (*padrone*) which is mainly occupied during the winter shooting season.[1]

In March the sluices are opened so that fresh water flows in and water of low salinity passes out into the Lagoon. This attracts the fish, which enter the *valle* through one-way valves, and enormous numbers of young eels and mullet will come in during the month. During April and May the gates are so operated with the tides as to collect as much sea water as possible in the *valle*. Through June, July and part of August the *valle* is closed but the salinity may, owing to evaporation, rise to a dangerous level, when it is necessary to control it by periodic inundation from the *acquadore dolce*, which operation is known as summer irrigation (*irrigazione estiva*). Towards the end of August, however, this must stop in order to lower the level in preparation for the September fishing operations. In mid-September on a high inflowing tide the main sluice gate is opened, and the copious inflow of sea water stimulates the outward migration of the mature fish which are caught in elaborate traps

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[1] The game, chiefly duck, are a considerable economic asset. About 400,000 lire's worth per annum was killed in this region before the war. Wooden painted models, as well as tame ducks tied by the leg, are used as decoys.

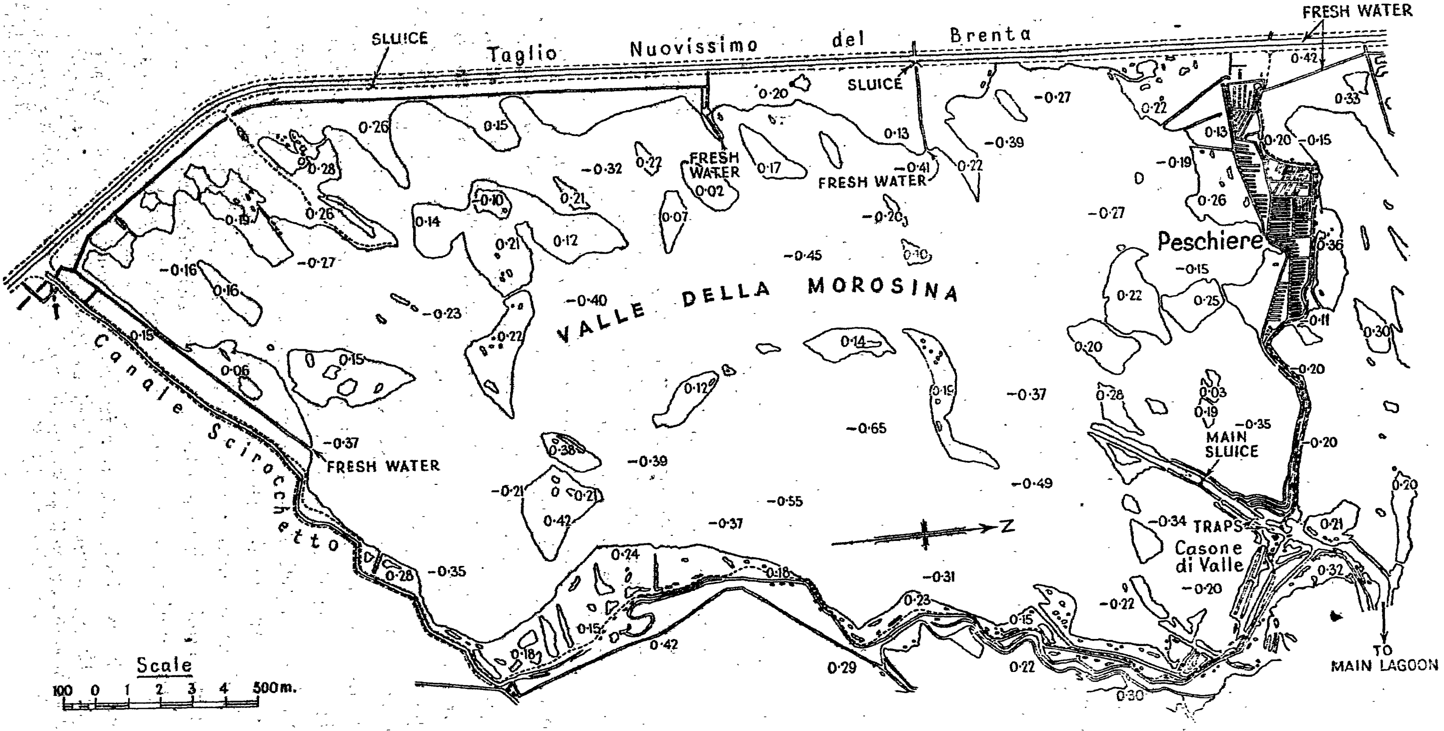

FIG. 2.—Plan of a typical *Valle da Pesca* which has been completely enclosed by banks (*valle arginata*). Depths relative to sea-level given in metres.

Reproduced from "Bonifica e Vallicoltura," Rome, 1937, by kind permission of the Magistrato alle Acque, Venice.)

constructed in the exit channel. This process is repeated on all high tides until the end of December. The biggest catches are made on the highest tides between full and new moon, especially in stormy weather.

In mid-winter the temperatures in such shallow water are dangerously low, and to protect the immature fish, many of which spend several years in the *valle*, a system of especially deep channels is provided (*peschiere*, Figs. 2 and 4) which are protected from the cold winds by dense vegetation such as tamarisk (*Tamarix gallica*), which will grow in a salty soil. On the approach of the cold weather the fish are attracted into the *peschiere* through valves by irrigation with fresh water from the *acquadore dolce*.

Advantage is thus taken, in an interesting manner, of the behaviour reactions of the fish to salinity changes and water currents relative to the degree of sexual development, the external factors being so regulated as to conduct the fish to their feeding grounds and away into the traps at the appropriate times. Very little is as yet known either of the relative importance of the external stimuli, of the sensory-motor mechanisms through which the fish reacts, or of the nature of the internal control associated with sexual development. It appears from the evidence that the outward migration is occasionally from water of high salinity in the *valle* (due to summer evaporation) into inflowing sea water which is actually of lower salinity (Henking *et al.*, 1908), and in this respect the situation is the reverse of that under normal estuarine conditions. But water currents are probably the most important factor directing seaward migration. The Adriatic *Valli da Pesca* would seem to offer admirable facilities for the study of the factors involved in the migrations of these fish and especially of the eel.

Venice has recently founded an Institute for Fisheries Research, the Osservatorio di Pesca di Venezia. All data relative to the fauna and flora of the Lagoon can be obtained from the Museo di Storia Naturale di Venezia (Director, Prof. M. Minio) where some historic collections, including those of Olivi and Nardo, are to be seen. A few years ago the University of Padua built a small but well-appointed laboratory at Chioggia (directed by the Professor of Zoology, Prof. U. D'Ancona) specifically for the study of lagoon biology. Visits from foreign scientists to these institutions are welcomed and it is to be hoped that British biologists who are interested in the ecology and physiology of brackish water organisms and in the attractive problems associated with the practice of *vallicoltura* may be able to take advantage of this opportunity.

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RECENT ADVANCES IN SCIENCE

ASTRONOMY.

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UNTIL quite recently, astronomers had little in common with radio engineers. To be sure, the technique of the electronics laboratory was useful to astrophysicists who were developing methods of measuring solar and stellar radiation by photoelectric and thermoelectric means, but this was of secondary concern: the design of high-gain D.C. amplifiers, for example, could hardly be said to be a matter which struck deep into astronomical research. But since the development for war-time uses of ultra-short-wave radio technique, the field of radio physics has begun to overlap that of astronomy. Recent work has shown that extra-terrestrial sources exist which radiate electromagnetic waves in the range 1 cm. to 30 m. These sources are our own sun and some unknown emitter in the Milky Way, and it will be convenient to consider them separately and in that order.

The observable solar spectrum as the astronomer has hitherto understood it extends from about 3000 A. to 240,000 A. The short-wave limit is set by the absorption of solar radiation in the Earth's atmosphere by ozone and eventually by oxygen. The long-wave limit quoted, attained by Adel at the Lowell Observatory in 1941, represents the extreme to which the infra-red solar work has yet been pushed, though it may well be extended in the future. Between these limits, when allowance is made for absorption both in the sun's atmosphere (Fraunhofer lines) and in the Earth's (telluric bands) it is roughly true to say that the spectrum of the sun does not greatly diverge from that of a black body. There are astronomical reasons for believing that this circumstance is fortuitous, and the black-body distribution may not hold for very much hotter or very much cooler stars; but this need not concern us here. Now the importance of the conception of a black body is, at least to some extent, that the distribution of radiation in its spectrum is amenable to mathematical treatment, and it would not be unfair to say that astronomers, though they may in their own approximations use the concept, are agreeably surprised if any of the celestial objects that they observe turn out to be black or even " grey " bodies (*i.e.* bodies emitting some fraction, constant with wave-length, of black-body radiation). At any rate they do not assume beforehand that the sun would radiate as a black body throughout its spectrum. In particular, the Planck curve, conceived for a range of frequencies which covers the visible and infra-red spectrum, would not necessarily be expected to hold well away from these regions. That it nevertheless does so in the centimetric waves and even beyond is a recent and rather surprising discovery (Southworth, *J. Franklin Inst.*, **239**, 285, 1945).

The apparatus used in this work consisted of a double-detection radio receiver designed to work at any of three wave-lengths between 1 and 10 cm., using an antenna at the focus of a parabolic mirror. The whole array is mounted in altazimuth fashion and is of fairly high directivity. Standard methods of calibration allow the " noise " power received to be measured in terms of second detector plate current. The acceptance band was about a megacycle wide at all three wave-lengths. At a wave-length of 10 cm., continuous radiation from the sun of constant intensity less than three times [1] that given by the Rayleigh-Jeans formula (*i.e.* with the long-wave end of a Planck curve) for an object of the sun's size at 6000° K. is observed throughout the hours of daylight. There is no diurnal variation such as would be expected near sunrise and sunset if the Earth's atmosphere absorbed radiation of this wave-length ; the only variation observed corresponds with the geometrical occultation of the sun's disk at the instant of sunrise or sunset. At 3 cm. the intensity found is rather more than twice [1] the black-body value, again with no marked diurnal variation. At $1\frac{1}{4}$ cm. the intensity has dropped to 30 per cent. [1] of the Planck value, and the radiation is actually less than at 3 cm., though the theoretical curve, of course, rises steadily towards optical wave-lengths. At the shortest wave-length there is evidence of prolongation over an hour or so of the sunrise and sunset effects, as if the absorption coefficient of the atmosphere were becoming appreciable in this region.

The direction of maximum intensity is always the general direction of the sun within a degree or so, but there are rapid variations in the angle of signal arrival which suggest that the radio image of the sun suffers from a shimmering, akin to the optical " boiling " familiar to solar observers but very much more pronounced. It will be realised, of course, that the directional pattern, on which depends the resolving power of the radio telescope, varies with wave-length: at the shortest wave-length the aperture angle of the receiver is a little larger than that of the sun, but at the intermediate wave-length it is three times, and at the long wave-length eight times, the sun's angular diameter. The pattern obtained by letting a local point source of radio waves drift across the mirror aperture is much narrower than that obtained by allowing the sun to do so, especially at the shortest wave-length: some of this is doubtless due to the fact that the sun is a disk and not a point source, but the broadening is too great to be explained by this alone. Here again it is suggested that the angle of arrival varies rapidly over a considerable range by refraction anomalies, or that scattering in the Earth's atmosphere causes an apparent increase in the sun's angular diameter. In this latter case, some of the discrepancy between the calculated and observed intensities might be removed: the calculations should be carried out using the effective diameter of the sun, not assuming it to be a point source.

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[1] These figures differ from those deducible from Southworth's fig. 2, in which the Planck curve is drawn too high by a factor of nearly 3.

So far we have been considering observations in the wave-length range 1–10 cm. Reber (*Astrophysical Journal*, **100**, 279, 1944) has reported some interesting work at 187 cm., at which wave-length he finds that the power radiated is about 27 per cent. more than the black-body value. In earlier work Jansky had failed to detect appreciable solar radiation at 1460 cm. So far, too, we have been concerned with the sun's normal output of radiation; it may appropriately be recalled here that the last minimum of solar activity was in early 1944, so that observations such as those of Reber and Southworth in 1943 and 1944 were conducted with an effectively undisturbed sun. The lifting of war-time security restrictions, however, has recently brought to light some interesting observations made when the sun was active—active, that is to say, in the visible and ultraviolet regions of the spectrum. Collation of various observations from amateur radio enthusiasts from 1936 onwards leads Appleton to the conclusion (*Nature*, **156**, 534, 1945) that short-wave electromagnetic radiation of abnormally high intensity may be emitted from active areas on the sun immediately before the occurrence of a solar flare. The radiation appears as a hissing noise in the speakers of ordinary short-wave receivers tuned to the wave-length range $7\frac{1}{2}$–30 m. Since on black-body theory the flux of solar energy at the Earth's surface is proportional to λ^{-2}, and since for aerials consisting of tuned half-wave dipoles the power absorbed is proportional to λ^2, the solar "noise" should be independent of wave-length. It can be shown that the black-body power received is in fact only about 10^{-4} of the receiver "resistance noise" associated with the temperature of the Earth's surface. In the ordinary way it is therefore quite undetectable with conventional receivers, and the only reason that it has ever been detected is that directive aerial arrays of high power-gain (10^4 or more) have been employed. Such aerials, which use parabolic mirrors, though manageable enough at centimetric wavelengths, become prohibitively large for the wave-lengths used in long-distance radio work. This means that unless the sun emits considerably more radiation in the frequency band accepted than a black body of the same size at 6000° K. would, there is no chance of detecting it with the conventional telecommunications receiver. That it has been so detected on occasion by amateurs must mean that on these occasions the active area of the sun was emitting at over 10^4 times the rate of the whole disk considered as a black body at 6000° K.

Some isolated observations in the range 4–6 m. (Hey, *Nature*, **157**, 47, 1946) confirm similar enormous enhancement of ordinary solar radiation at rather shorter wave-lengths than those discussed by Appleton. The observations were made on standard A.A. fire control radar equipment (of pre-magnetron vintage) operated by Army personnel in the ordinary course of their war-time duties, and the phenomenon is reported as "strong directional radiations similar in character to the random fluctuations of internal receiver noise (thermal and valve noise)." The astronomical interest of these observations lies in the precise time at which they were made. The radiation was first detected in the afternoon of February 26, 1942, and went on practically continuously, though with some intensity variations, for the rest of the day and throughout February 27 and 28 as well. It was apparent only between dawn and sunset, it extended over the whole wave-length range 4–6 m., it was observed by nine stations all over Great Britain, and it emanated always from a point within a few degrees of the sun. There has been no recurrence since February 28, 1942. No precise measurements of the intensity were made, but its magnitude on the display cathode-ray tubes was several times normal noise level, which puts it at about 10^{-17} watt/cm.^{2} per Megacycle band, or about 10^5 times the calculated black-body value. The associated observations in the optical region of the solar spectrum are as follows. A large sunspot appeared round the east limb on February 22, made its central meridian passage on February 28, and was carried round the west limb by the solar rotation on March 7. During its passage across the disk, solar flares were observed in the red hydrogen line on February 27d. 11h. (intensity 1), February 28d. 2½h. (int. 2), February 28d. 11h. (int. 3), February 28d. 12h. (int. 3) and March 1d. 10½h. (int. 2). The biggest of these, that on February 28d. 12h., was of quite unusual duration, lasting 3½ hours as against the usual ½–1 hour, and was accompanied by a short-wave radio fade-out lasting on some channels for 8 hours. Some 19½ hours later a great magnetic storm started. There seems little doubt that these geophysical phenomena were associated with the particular flare noted, but the radio noise reported from Army sources began about 45 hours before and was, indeed, the earliest of the terrestrial phenomena observed. It is therefore unlikely that the connection between flare and radio noise is as directly causal as that between flare and fade-out, or between flare and magnetic storm.

Further general confirmation that disturbed areas of the sun's disk are associated with abnormal radio emission is given by Pawsey, Payne-Scott and McCready (*Nature*, **157**, 158, 1946), who observed that the sun emitted during October 1945 at a wave-length of 1·5 m. a considerable amount of "fluctuation noise." The mean daily intensity varied throughout the month in a way which was fairly closely parallel to the variation in the area of the solar disk covered by visible sunspots, the peaks of radio intensity coinciding with the central meridian passage of large spot-groups. On October 4–5, when two large groups occupying 3000-millionths of the visible disk were crossing the central meridian, the radiation at 1·5 m. was about 2300 times the black-body value. At 50 cm. no effect could be obtained, indicating an intensity less than eight times the black-body value; whilst at 25 cm. a small effect was observed corresponding to the black-body value. Evidently the radiation from the disturbed sun has some of the characteristics of a line spectrum, its intensity varying enormously with wave-length.

In the coming period of solar activity, which should reach its peak in 1948, it may be that radio observations will provide warning of the likely appearance of flares and their associated terrestrial effects—warning which will be welcomed alike by astronomers and by radio engineers. It may even be that improvements in the resolving power of the radio equipment may enable "radio telescopes" using large mirrors and short wave-lengths to distinguish between various parts of the solar disk and thus to locate with some certainty (and independently of cloud) the regions responsible for the emission of this abnormally intense radiation.

So much for the sun's radio spectrum when active and inactive; let us now turn to radio emission from the Milky Way—" cosmic noise" or "cosmic static." The pioneer observations here were made in 1931 by Jansky, who, observing at 14·6 m., established the source of maximum disturbance as fixed in space and roughly coincident with the centre of the Galaxy in Sagittarius. In further work by Reber at 1·87 m., Jansky's rotating unidirectional antenna array was replaced by an aerial at the focus of a stationary parabolic mirror 31 ft. across mounted in the meridian. The rotation of the Earth provides the necessary motion in right ascension, the mirror sweeping across a declination band pre-selected by the observer, who adjusts the tilt of the mirror before observations are begun. The intensity of the radiation is recorded automatically against right ascension on a chart, and when enough charts at different declinations have been obtained, the results may be entered on a celestial map and lines of equal intensity plotted. Reber's results (*Astrophysical Journal*, **100**, 279, 1944) show these iso-lines neatly outlining the Milky Way, with a primary maximum in Sagittarius and secondary maxima in Cygnus, Cassiopeia, Canis Major and Puppis. The lowest minimum is in Perseus. The threshold sensitivity of the arrangement is about $0{\cdot}25 \times 10^{-22}$ watt/cm.^{2} per Megacycle band arriving from a square degree, an intensity which is reached within ± 30° of the galactic equator at most points along the Milky Way. Elsewhere in the sky no radiation is detectable. The greatest intensity in Sagittarius is 14×10^{-22} watt/cm.^{2} per square degree per Megacycle band. The angular discrimination in Jansky's work was relatively poor (he gives the location of the centre of maximum disturbance to ± 30 min. in right ascension and ± 30° in declination), but Reber's apparatus has a resolving power of 8° in the plane of the electric vector and 6° in the plane of the magnetic vector. No doubt the detection of the subsidiary peaks is a result rather of this improved resolution than directly of the shorter wave-length. The resolving power is, of course, inextricably bound up with wave-length, and an important practical difficulty in this work is that at short wave-lengths (say 1 m.) where resolution is intrinsically easy, the power received is small compared with receiver noise, unless an unwieldy aerial system is used, whereas at longer wave-lengths (say 10 m.) where the power received might be expected to rise, the beam width is too great for satisfactory acuity.

A compromise between these two factors is made by Hey, Parsons and Phillips (*Nature*, **157**, 296, 1946), who use a Yagi aerial system giving at 4·7 m. a half-power beam width of ± 6° in altitude and ± 15° in azimuth. The shape of the polar diagram was established by using a small oscillator attached to a balloon. The equipment is used directed at a fixed altitude of 12° and rotated through 360° of azimuth at each half-hour during the observations. The intensity of cosmic noise is thus obtained along a series of intersecting small circles on the celestial sphere. By working backwards from the known polar diagram it is possible to deduce true distributions along these circles which would, when integrated over the whole beam accepted by the antenna array, fit the observed intensity at the receiver. From the resulting lattice of true intensities, contours can then be drawn for the derived distribution of cosmic noise. Once again the distribution is roughly symmetrical about the galactic equator (though in the Scutum-Aquila-Cygnus region there is a puzzling divergence of the observed line of maximum some degrees to the south which may be real, since Reber also observed it, though to a less extent), the intensity dropping to half value within (very roughly) ± 20° of the galactic equator. The main source is in Scorpius and Sagittarius, corresponding closely to the direction of the galactic centre, whilst a secondary peak only $\frac{2}{3}$ as high occurs in Cygnus close to the one found by Reber. The Sagittarius maximum reaches 4×10^{-22} watt/cm.^{2} per Megacycle per square degree, as compared with Reber's 14×10^{-22} at a wave-length $\frac{2}{5}$ as long.

The variation of intensity with galactic longitude is so marked and is likely to be so important for investigating the origin of this electromagnetic radiation that it might perhaps be worth while to improve the geometry of the observational method. In the British work, for example, partly no doubt as an inheritance of war-time technique, where the fundamental plane of reference was that of the horizon, the intensity contours are obtained by a rather roundabout process from observations with apparatus mounted in altazimuth fashion. But the astronomically important plane is, of course, that of the Galaxy itself, and the logical way to mount the telescope is as an undriven galactic equatorial with a "polar" axis pointing to the north galactic pole and a "latitude" axis at right angles to this. Allowance for the diurnal motion of the north galactic pole could be made by making the polar axis easily adjustable in altitude and azimuth, or, if prolonged observations are contemplated, by mounting it equatorially in the ordinary sense of the word and driving at the celestial rate. Some such arrangement as this would be of help if (as seems to be the case) the intensity varies with time. Certainly a great many more observations, both directly of the cosmic noise at various wave-lengths and indirectly of associated quantities such as attenuation in the atmosphere, are needed before we can gain a clear idea of the facts.

This need not deter the theorists, however—nor indeed has it. Whipple and Greenstein (*Proc. Nat. Acad. Sci.*, **23**, 177, 1937) first entered the field by trying to explain the cosmic noise in terms of the thermal emission from interstellar dust particles, for the existence of which there is now very good evidence, and which are concentrated closely to the Milky Way. Rejecting, with Jansky, the suggestion that the stars of the Milky Way can themselves be responsible either directly or through the action of secondary radiation induced in the Earth's atmosphere, Whipple and Greenstein calculate first of all the temperature likely to be reached by interstellar dust. Making plausible assumptions as to the total luminosity of the central mass of the Galaxy and the total extinction produced by interstellar matter, they calculate that "black" dust will attain about 10° K. Redistributing, according to a black-body curve for 10° K., that 15 per cent. of the stellar radiation which is absorbed and re-emitted, they show that the hypothesis fails by a factor of 10^6 to account for the observed intensity at 14·6 m. They then try to improve matters by allowing for a sort of galactic greenhouse effect. The interstellar material is relatively opaque to radiation in the optical region, where the stars radiate most strongly. The resulting increase in the energy density in the galactic nucleus should therefore raise the temperature of the interstellar particles and result in greater emission of radio waves, to which the matter is transparent. But even so, the detailed discussion shows that even in the most favourable case, the black-body radiation theory fails by a factor of 10^4.

Henyey and Keenan (*Astrophys. J.*, **91**, 625, 1940) follow Reber in suggesting that the radiation is interstellar in source but originates not in temperature radiation from dust particles but in free-free transitions by electrons in the field of protons. The principal source of the particles is the ionising radiation of the hot stars, so the electron temperature, as distinct from the black-body temperature, is of the order of 10,000° K. It is principally as a result of this that the calculations show good agreement with observation. Using a value of 1 cm.^{-3} for the electron density (many lines of astronomical evidence confirm that this is the right order of magnitude); and assuming a Maxwellian distribution of velocities in the electron gas and an optical depth computed on the assumption that the emitting layer is 16,000 parsecs thick, the total radiation from the free electrons in the Galaxy is found to follow a Planck curve for 10,000° K. from the longest waves to about 5 m. At this wave-length the layer ceases to be opaque and the radiation falls off to smaller values (determined essentially by bound-free transitions) in the optical region. The observations of Hey, Parsons and Phillips at 4·7 m. and of Reber at 1·87 m. agree well with the theoretical curve, though the concentration of the radiation to the Milky Way is considerably less than would be expected for interstellar matter.

The connection between solar noise and cosmic noise is, of course, a matter of some interest. The existence of abnormally intense radiation from the sun at the wave-lengths at which radiation reaches us from the Milky Way tempts one to conclude that the stars of the Galaxy rather than interstellar matter might be responsible for cosmic noise. It is a well-established article of faith with astronomers that the sun is a pretty average star. If the Milky Way were composed entirely of solar-type stars its radiation at radio wave-lengths would bear the same relation to that of the sun, area for area, as does its optical radiation. Now in Sagittarius a square degree of the Milky Way is visually equivalent to a star of magnitude 1·9. The visual brightness of the sun is $-26{\cdot}7$, so that allowing for the fact that the sun's area is rather less than $\frac{1}{4}$ square degree, the Milky Way is 30·4 magnitudes fainter visually than the sun, a light ratio of $1{\cdot}4 \times 10^{12}$. If this ratio were to hold also in the radio range, cosmic noise from this source would be quite undetectable. However, it *is* observed: at Reber's wave-length (1·87 m.) the ratio is in fact about unity. The discrepancy can be reduced if the stars radiated at all times as the sun does when large spot-groups are passing across the disk: but only by a factor of 10^5 (Hey, 5 m.) or 2×10^3 (Pawsey, Payne-Scott and McCready, 1·5 m.). And, of course, a constant emission at the maximum rate is not a likely supposition. A time-average would have to be taken; and for the sun we shall not be in a position to do this until we have observations over the whole 11-year cycle at a series of wave-lengths. At the very best, then, we are left with a discrepancy considerably greater than 10^7. Nor can any reasonable amount of interstellar absorption affecting the Milky Way clouds at visual but not at radio wave-lengths improve matters much. To save the hypothesis we should be forced to assume that the sun is not a typical star, but radiates in the radio range perhaps a million times less than the average Milky Way star. In these circumstances most astronomers would probably agree that until more is known about solar noise and cosmic noise we should do well to consider them as different phenomena of distinct origins.

PHYSICS. By F. A. VICK, O.B.E., Ph.D., F.Inst.P., The University, Manchester.

MUSICAL ACOUSTICS.—"Whenever there is a difference of opinion between a musician and a physicist concerning some point having to do, primarily, with the musical result obtained, the musician is, in 90 per cent. of the cases, right and the man of science wrong. However, matters do not usually end here, because the musician usually makes the mistake of giving the scientific reasons for what he says. Then everything he says is wrong, and the man of science has only to stand by and jump at him at the end of every sentence, reducing him to complete defeat, but leaving him unconvinced, nevertheless" (W. F. G. Swann, *J. Frank. Inst.*, **239**, 1, 1945).

The instruments of the orchestra were not invented by physicists but evolved in the hands of generations of practising musicians and craftsmen. (See, for example, the fascinating account by K. Geiringer, *Musical Instruments*, 1943.) Similarly, the intervals of the major and minor scales were not devised by mathematicians, in spite of the efforts of the followers of Pythagoras. Nevertheless, the behaviour of musical instruments and the qualities of the sounds produced by them singly and in combination have raised various problems and questions which have intrigued scientists for over three centuries. These are tackled just because they are interesting and their solution will advance the science of acoustics. In addition, however, the physicist may be of direct help to the musical instrument maker in, for example, the choice of materials, and the positioning of the finger holes on wind instruments (by analogy with electrical transmission line theory). Becoming more bold, physicists interested in music (and some who are not ?) have recently been designing quite new instruments based, in most cases, on advances in electronic engineering. The subject of musical acoustics is thus very live in most countries. Summaries of progress have been published from time to time by E. G. Richardson in the Physical Society Reports on Progress in Physics. One of the problems which has excited most interest is the explanation of the excellence of the tone of Stradivarius and other Italian violins of the period, compared with instruments made before and since. The question has been studied by physicists at Harvard for over ten years, and a recent report summarises the work to date (F. A. Saunders, *J. Acoust. Soc. Amer.*, **17**, 169, 1946). The University was fortunate enough to have available seven Stradivarius violins, as well as other good Italian instruments. These were compared with modern violins selected by Heifetz and other musicians from over a hundred submitted by various makers, and a few "cheap and nasty " instruments. Among the tests made were (*a*) frequency response curves obtained by several methods, the final one involving excitation of a phosphor bronze wire taking the place of the G string, (*b*) the range of loudness, consistent with good tone (*c*) the least force between bow and string to make the instrument speak, (*d*) the damping (log. dec.) of the freely vibrating strings at various frequencies. No physical qualities were found in the celebrated old Italian violins which were not also present in the best new ones. Within the accuracies of measurement, it was concluded that any differences in tone quality between the old Italian instruments and the best new ones cannot be attributed to (1) the distribution of intensity among the partials of steady tones, (2) the mechanical ease with which the steady tones are produced, (3) the distribution of intensity with frequency over the range of the violin, (4) the duration of tone after the excitation ceases. For all good instruments, the least force between bow and string to make it speak was in the region of 12 gm. wt., the log. dec. was about 0·1, and the range of loudness about 30 db. In agreement with previous observations, it was found that for good tone the frequency response should be reasonably flat up to about 6000 cycles, and then fall off sharply. There was no evidence of special formants. Some violas and cellos were also tested. In general, the main body resonance was found to be 17 ± 2 semitones above the lowest note for all good instruments, and the air resonance 6 semitones up for violins, 9 for violas, and 7–8 for cellos. The very careful work at Harvard has brought to light no reason why the best modern violins should not be as good as a Stradivarius. As Saunders and others have pointed out, however, the old Italian violins are not now in the same condition as when new. For example, in order to withstand the greater string pressure consequent upon rise in standard pitch, heavier bass bars have been fitted. It has already been shown that the varnish, so long as it is good, has no appreciable influence on violin tone, but the effect of ageing of the wood is uncertain. On the average, the new violins are 7 per cent. heavier than the old Italian ones. Quality of strings, position of bridge and soundpost, etc., influence tone quality and, of course, a poor violin may produce much more agreeable sounds in the hands of an artist like Heifetz than a Stradivarius played by a mediocre performer. The main problem still remains, however, since experienced violinists were able to identify the tone of old Italian instruments when played behind curtains by the same artist and under the same conditions as good modern violins.

Another instrument which has been examined in great detail in recent years is the piano, *e.g.* in Germany, India (by Ghosh) and U.S.A. A very interesting recent paper is by O. H. Schuck and R. W. Young (*J. Acoust. Soc. Amer.*, **15**, 1, 1943). They have studied in detail the dying away of the various partials of each of the strings of a concert grand piano. As is to be expected, the higher partials die away most rapidly, and the tone quality changes as the sound diminishes. Particularly in the tone produced by the longer strings, the amplitudes of the partials show large fluctuations of form suggesting half sine waves. The higher the partial, the more rapid the fluctuations. Such fluctuations may be due to rotation of the plane of vibration of the string and there may be transfer of energy from one mode of vibration to another. Resonances in the room may have some effect, of course. The seventh partial was not found to be particularly weak, in contradiction to the usual view. The frequencies of the partials were not in a harmonic series, but sharp, the inharmonicity in cents being proportional to the square of the number of the partial, as predicted by theory for stiff strings (Rayleigh, *Theory of Sound*). A cent is one-hundredth the *interval* (frequency ratio) of a semitone. The sharpening is least in the two octaves below middle C and increases rapidly above and below this region. The inharmonicity is less for long strings than for short. Long strings give a "full round tone" because the partials are more nearly in a harmonic series and the difference tones consequently more nearly the fundamental frequency. The fundamental may be weak or almost absent in bass strings. The inharmonicity has an effect on tuning pianos, since the tuner starts at the middle register and tunes the others with the aid of beats with upper partials. The tuning becomes progressively sharp in the high register and flat in the low.

There have appeared in the *Journal of the Acoustic Society of America* during the last few years a number of short papers giving results of experiments on brass and wood wind instruments. For example, D. W. Martin (1942) studied the movement of the lips of a cornet player by photographing them through a special mouthpiece of transparent plastic. Knauss and Yeager (1941) concluded that the vibrations of the metal of a cornet contribute little, if any, to the tone of the instrument. H. W. Henderson (1942) studied the most efficient lip technique for producing good trumpet tone and found that, contrary to the statements in most textbooks, the upper lip vibrates as a single reed against the lower lip, which partly controls the vibration rate. R. W. Young (1946) records the dependence of the tuning of fifteen different wind instruments on temperature.

In the patent literature, inventions of new electronic instruments occupy an increasingly prominent place, but it is probable that most of the instruments will not survive. Most interest has centred on electronic organs, owing to their relatively low cost, compactness, steadiness of tuning and inherent flexibility. While, as yet, even the best electronic organ cannot be said to be a complete substitute for a good pipe organ, sufficient progress has been made for the instruments to be acceptable to the musical ear when played with due regard for both the limitations and capabilities of the instrument. G. T. Winch and A. M. Midgley (*J.I.E.E.*, **86**, June 1940) have given a good account of the main types of electronic organ to that date. S. K. Lewer's paper (*Electronic Engineering*, **17**, 149, 1944) is of interest to those who aspire to construct organs for themselves, since it illustrates some of the practical difficulties.

So much for the study of instruments old and new. Is it not conceivable that the physicist might be able to help the performer and teacher of musical instruments by studying the " mechanics " of playing and tone production ? For instance, a good teacher can show by example the method of holding a violin bow which long experience has shown to be best. But if the pupil were able to understand how the forces he exerts on the bow through his arm, hand and fingers are communicated to the string, and why certain forces only are desirable, would he not avoid much loss of time and gain greater confidence ? Some useful suggestions of this kind are made by W. F. G. Swann in a lecture reprinted in *J. Frank. Inst.*, **239**, (1945). This branch of musical acoustics is in its infancy, but, combined with other studies of the type outlined above may do much to prevent the disagreements between scientists and musicians mentioned in the quotation at the head of this article, especially if acoustics is included in the subjects taught in Music Departments—as is already done in some Universities.

Clerk Maxwell, in his Rede lecture for 1878, remarked on " . . . that untrodden wild between acoustics and music, that Serbonian bog where whole armies of scientific musicians and musical men of science have sunk without filling it up." Some progress has been made since that date in building safe roads across the bog, as may be seen from papers cited in this article, and recent books, such as those by Alexander Wood, Sir James Jeans and L. S. Lloyd.

In a previous article (SCIENCE PROGRESS, **33**, 288, October 1938) some account was given of progress in the study of the acoustics of rooms. The whole subject has now been admirably reviewed by P. M. Morse and R. H. Bolt (*Rev. Mod. Phys.*, **16**, 69–150, April 1944), in eight chapters and a comprehensive bibliography.

PHYSICAL CHEMISTRY.

By J. W. SMITH, D.Sc., Ph.D., F.R.I.C., Battersea Polytechnic.

MAGNETIC SUSCEPTIBILITY AND MOLECULAR STRUCTURE.—If two magnetic poles of strengths m_1 and m_2 are separated by a distance r, the force F between them depends on the nature of the medium, and is expressed by the relationship $F = m_1m_2/\mu r$, where μ, the magnetic analogue of dielectric constant, is known as the magnetic permeability of the medium. Its value is taken as unity for a vacuum; it is very high for "ferromagnetic" substances, slightly greater than unity for a "paramagnetic" substance, and slightly less than unity for a "diamagnetic" substance. Thus magnetic lines of force pass more readily through a paramagnetic substance, and less readily through a diamagnetic substance than they do through a vacuum. Therefore, when suspended in a uniform magnetic field, rods of paramagnetic and diamagnetic materials will tend to set themselves parallel to and at right angles to the lines of force, respectively, so that the latter can travel the maximum and minimum distances, respectively, through the material; further, in a non-homogeneous field in a vacuum or medium of permeability approximately unity, paramagnetic substances tend to be drawn into the more intense region of the field, whilst diamagnetic substances tend to be repelled from it.

If a magnetic field of strength H gauss induces in a medium a magnetic moment I per unit volume in the direction of the field, I is called the Intensity of Magnetisation. The total magnetic flux, then, in the medium is given by $B = H + 4\pi I$. But the ratio $B/H = \mu$, since for a vacuum $I = 0$ and $B = H$; also the ratio I/H, the magnetic moment per unit volume in the direction of the field produced by a field of 1 gauss, is known as the Volume Susceptibility κ, hence $\mu = 1 + 4\pi\kappa$. For chemical purposes the magnetic susceptibilities per gram (χ), per gram atom (χ_A) or per gram molecule (χ_M) are generally used. These are related to κ by the expressions $\chi = \kappa/d$, $\chi_A = A\kappa/d$, and $\chi_M = M\kappa/d$, where A and M are the atomic and molecular weights, respectively, and d the density. The molecular susceptibility, χ_M, then, has the significance of being the magnetic moment in the direction of the field produced in one gram-molecule of the substance by a field of 1 gauss. Evidently $\kappa = 0$ for a vacuum, so κ and χ_M are positive for a paramagnetic substance and negative for a diamagnetic substance.

In close analogy to the case of the dielectric constant, these effects arise through the establishment of an induced magnetic moment in the molecule itself, together with the orientation of any permanent magnetic dipole present in the molecule. Of possible contributory factors, the spin of the nucleus makes a very small contribution, which is negligible except at very low temperatures, and therefore magnetic properties are determined by the number and distribution of the extra-nuclear electrons. Diamagnetism arises through the interaction of the magnetic field with the completed electron groups of the atoms of the medium, whereby the orbital motion of the electrons is caused to precess around the direction of the lines of force so as to give rise to a field opposed to the applied field. Paramagnetism is attributable to the magnetic moment which each electron possesses through its orbital motion and spin. These moments cancel out for all completed sub-groups of electrons in the atom, that is, when it has in the same electronic shell two *s*, six *p*, ten *d*, or fourteen *f* electrons, where the letters *s*, *p*, *d*, *f* indicate the second or series quantum numbers of 0, 1, 2, and 3, respectively. Therefore paramagnetism occurs mainly in compounds containing an odd number of electrons or containing atoms with incomplete sub-groups, *i.e.* the transitional and rare-earth elements.

Van Vleck has shown theoretically that in the case of molecules a small contribution (χ_R) due to resonance between the ground state and the higher energy states of the molecule must also be included. Therefore for the general case χ_M will be the sum of three factors, the induced diamagnetic contribution χ_d, which is negative, the paramagnetic contribution χ_μ, due to permanent magnetic dipoles, which is positive, and the resonance contribution χ_R, which is again positive, *i.e.* $\chi_M = \chi_d + \chi_\mu + \chi_R$. For most simple molecules with an even number of extra-nuclear electrons $\chi_\mu = 0$, and χ_R is less than χ_d, so the substance is diamagnetic. In rare cases only, *e.g.* potassium permanganate, χ_R is greater than χ_d and slight paramagnetism is observed which is invariant with temperature.

For a diamagnetic substance the action of a magnetic field of H gauss causes the electrons to precess around the direction of the lines of force with angular velocity $eH/2mc$, where e and m are the charge and mass of the electron and c the velocity of light. This produces a change in the magnetic moment of the system of $-e^2H\bar{r}_1^2/4mc^2$ for each electron, where $\bar{r}_1^2$ is the mean square radius of the electronic orbit as projected on a plane at right angles to the field. For an atom containing n electrons the resultant moment will be $-(e^2H/4mc^2)\sum\limits_n \bar{r}_1^2$, whilst if it has spherical symmetry $\sum\limits_n \bar{r}_1^2 = \frac{2}{3}\sum\limits_n \bar{r}^2$, where $\bar{r}^2$ is the mean square of the actual radius of the electronic orbit. Hence the moment of each molecule $\Delta M_\mu = -(e^2H/6mc^2)\Sigma\,\bar{r}^2$, and since $\chi_d = \Delta M.N/H$, where N is the Avogadro number, $\chi_d = -(\mathrm{N}e^2/6mc^2).\sum_n \bar{r}^2$. On substitution of the values of the universal constants, this gives

$$\chi_d = -2{\cdot}84 \times 10^{10}\Sigma\bar{r}^2 \text{ e.m.u.}$$

Since $\bar{r}$ is of the order of Angstrom units, *i.e.* 10^{-8} cm., it is evident that χ_d will be a multiple of 10^{-6} e.m.u. For simplicity, in the figures quoted in this note, the diamagnetic susceptibility will be given with this power, and also the negative sign omitted, *i.e.* in terms of $\bar{\chi}_d$, where $\bar{\chi}_d = -\chi_d \times 10^6$.

For monatomic molecules and ions with a rare gas structure, χ_μ and χ_R are zero, and χ_d represents the total magnetic susceptibility of these substances. The above relation indicates that χ_d should be independent of temperature, and with certain exceptions this is generally observed. Also the value of $\bar{r}$ deduced from observed values of χ_d should give a measure of the orbital radius. For helium $\bar{\chi}_d = 1{\cdot}9$, leading to $\bar{r} = 0{\cdot}578$ Å, of the same order as the atomic radius deduced from other data.

The actual calculation of the diamagnetic susceptibility from first principles is by no means so simple or satisfactory, however. Independently van Vleck (*Proc. Nat. Acad. Sci.*, 1926, **12,** 662) and Pauling (*Proc. Roy. Soc.*, 1927, **A, 114,** 181) deduced that for each electron r^2 can be expressed in terms of its principal and serial quantum numbers n and l as

$$r^2 = a_0 n^2/(\mathrm{Z}-\mathrm{S})^2\{5n^2 - 3l(l+1) - 1\}/2$$

where a_0 is the radius of the electron orbit in the ground state of the hydrogen atom (0·528 Å), Z the nuclear charge, and S the screening constant, *i.e.* (Z − S) is the effective nuclear charge. The uncertainty of this last term, which was estimated by Pauling for various electronic groups, is probably responsible for the poor agreement between the calculated and observed values of χ_d, even for the inert gases, which should present the simplest examples of all.

Other lines of approach have been scarcely more successful. Stoner (*Proc. Leeds Phil. Soc.*, 1929) applied the space-charge distribution method, but the susceptibilities calculated by means of the formula which he developed are, like Pauling's values, generally greater than the observed values. On the other hand, Slater (*Phys. Rev.*, 1930, **36,** 57), utilising the fact that the nodes in a wave function are unimportant to calculate the wave functions of atoms or ions, and taking as the radial part of the wave function of one electron in a symmetrical atom $\psi = r^{(n^*-1)}.e^{-(\mathrm{Z}-\mathrm{S})/n^*r}$, where n^* is the effective quantum number, deduced that

$$\bar{r}^2 = (n^*)^2(n^*+\tfrac{1}{2})(n^*+1)/(\mathrm{Z}-\mathrm{S})^2.$$

He gave rules for the evaluation of n^* and S for any electron group, and therefore the value of $\Sigma\bar{r}^2$ and hence of χ_d could be calculated. The application of this method and a modification by Angus (*Proc. Roy. Soc.*, 1932, **A**, **136**, 569) in which the *s* and *p* electrons of the same quantum number are treated separately, give values in better accord with experiment than those of Pauling or Stoner. Finally, theoretical values of χ_d have been deduced from polarisability data, particularly by Brindley (*Physical Rev.*, 1933, **43**, 1030), the results being again in fair accord with observation.

The following data show the extreme variations of the values reported for the experimental values of $\bar{\chi}_d$, together with the theoretical values deduced by various methods, for the case of the rare gases.

	He	Ne	A	Kr	Xe
Exp. vals. . . .	1·88–1·95	6·66–7·65	18·1–19·7	28·0–29·2	42·4–44·1
Calc. vals.:					
Pauling . . .	1·54	5·7	21·5	42·0	66·0
Slater	1·68	5·7	18·9	31·7	48·0
Angus	1·68	5·1	16·95	29·3	44·8
Brindley . . .	1·97	6·1	16·7	29·3	45·5

Besides the rare gases, the other non-metallic elements excepting oxygen and diatomic sulphur are all diamagnetic, and so are the elements of the B sub-groups of the Periodic Table. In each group $\bar{\chi}_d$ increases with increasing atomic number.

The susceptibilities of a number of salts have been measured in aqueous solution, and, since replacement of one cation or anion by another leads to constant changes in $\bar{\chi}_d$, independent of the other ion present it can be inferred that the ionic susceptibilities are additive. The calculation of the contribution of each ion to the total observed value, however, involves assumptions. Calculations similar to those for the inert gas atoms can be applied to simple ions, but the accord between calculated and observed values is again not good. The best approach seems to be that of Brindley and Hoare (*Proc. Roy. Soc.*, 1935, **A**, **152**, 342), who took for $Li^{\cdot}$ the theoretical value of 0·7, arguing that an error in this small value will not influence greatly the values for other ions. From this basis the values of typical ionic susceptibilities are :—

	$\bar{\chi}_d$		$\bar{\chi}_d$		$\bar{\chi}_d$
$Li^{\cdot}$ . . .	0·7	$Mg^{\cdot\cdot}$ . . .	4·3	F'	9·4
$Na^{\cdot}$ . . .	6·1	$Ca^{\cdot\cdot}$ . . .	10·7	Cl'	24·2
$K^{\cdot}$ . . .	14·6	$Sr^{\cdot\cdot}$ . . .	18·0	Br'	34·5
$Rb^{\cdot}$ . . .	22·0	$Ba^{\cdot\cdot}$ . . .	29·0	I'	50·6
$Cs^{\cdot}$ . . .	35·1				

In an isoelectric series of atoms or ions, the values of $\bar{r}^2$ and hence of $\bar{\chi}_d$ should decrease with increasing nuclear charge, *i.e.* be less for cations and greater for anions, owing to the increased interaction between the nucleus and electrons. This is the case, *e.g.* F' 9·4, Ne 7·6, $Na^{\cdot}$ 6·1, $Mg^{\cdot\cdot}$ 4·3, Cl' 24·2, A 18·0, $K^{\cdot}$ 14·6, $Ca^{\cdot\cdot}$ 10·7. These differences are approximately those which would be expected from the crystallographic values of the ionic radii.

In considering the diamagnetic susceptibility of molecules, the resonance term must be included, and the only purely theoretical value that can be readily deduced is for hydrogen. This leads to a value of $\chi_d = -4{\cdot}7 \times 10^{-6}$ and $\chi_r = +0{\cdot}5 \times 10^{-6}$, giving a predicted value of $\chi_M = -4{\cdot}2 \times 10^{-6}$ in fair accord with the observed value of $-4{\cdot}0 \times 10^{-6}$. For other simple molecules χ_M is related approximately to their dimensions, *e.g.* N_2 12, Cl_2 41·5, Br_2 55·4, I_2 92·4, H_2O 13·0, N_2O 19, NH_3 19, CO_2 19, SO_2 19, HF 8·6, HCl 22·3, HBr 33·2, HI 47·9, $(CN)_2$ 22·5, N_2O_4 25·2, etc. There is generally little difference between the values for the liquid and gaseous states. The value for water varies somewhat with temperature, this being attributed to a decrease of hydrogen bonding with rise of temperature.

It was shown by Pascal (1908–13) that for covalent compounds χ_M is additive. As a first approximation, the formation of a covalency can be regarded as involving two electrons only, without much change occurring in the other electron orbits, so that the arrangement of electron density around any one kind of atoms will be almost the same in all molecules. On the other hand, the formation of double bonds, aromatic ring structures, etc., cause larger changes in the electronic orbits, and to allow for these, constitutional factors have to be introduced, very similar to the corrections for such structures which have been shown to be necessary in interpreting molecular refraction and parachor data. Typical values of the contributions of each atom are shown below; these are based on Pascal's data but corrected to a value of $\chi = 0{\cdot}72 \times 10^{-6}$ for water instead of the higher value of $0{\cdot}75 \times 10^{-6}$ used by him.

	$\chi_M \times 10^6$		$\chi_M \times 10^6$
H	− 2·85	F	− 11·5
C	− 6·0	Cl	− 20·1
—O— (in ethers and alcohols)	− 4·6	Br	− 30·6
=O (in ketones)	+ 1·73	I	− 44·6
=O (in acids and esters)	− 3·4	S	− 15·1
Double Bond C=C	+ 5·57	N (in amines)	− 5·6
N=N	+ 1·8	P	− 26·6
N=C	+ 8·2		
Triple Bond C≡C	+ 0·77	Benzene ring	− 1·5
C≡N	− 0·77	Naphthalene ring	− 6·1

Thus for ethyl alcohol

$$\chi_M^{\times 10^6} = 2 \times (-6{\cdot}0) + 6 \times (-2{\cdot}85) + (-4{\cdot}6) = -33{\cdot}7$$

(observed value = – 31·5).

The structural factors should permit diamagnetism to be used to decide between alternative structures proposed for some compounds. In many cases, however, considerable difficulty arises through the divergence in the experimental values found by different workers, and hence the uncertainty, particularly of the structural factors. Of the attempts which have been made to extend Pascal's method and put his constitutive factors on a theoretical instead of empirical basis the most interesting is that of Gray and Cruikshank (*Trans. Faraday Soc.*, 1935, **31**, 1491), who express their susceptibilities in terms of atomic and ionic susceptibilities, calculated by a modification of Pauling's method, together with bonding depressions, *i.e.* decreases produced by various forms of linkage. From the electrical dipole moment of the linkages, they deduce for each atom a "residual charge," given by the ratio bond moment/internuclear distance, which represents the fraction of time the atom can be regarded as being in a charged ionic state. By multiplying the susceptibilities of the atom for each state by the appropriate time factors a mean atomic susceptibility of the atom is obtained, and these values, summed for the molecule, give a theoretical value for $\bar{\chi}_M$. The difference between the calculated and observed values is taken as the depression caused by the formation of linkages. The method has been applied with pronounced success for a variety of diamagnetic compounds, but the calculations involve a knowledge of the screening constants, bond moments, and intermolecular distances, which are not known accurately.

The whole additivity relationship is by no means strictly obeyed, however. A recent study by Angus and Hill (*Trans. Faraday Soc.*, 1943, **39**, 185, 190, 197) shows that in a homologous series, the mean increment in $\bar{\chi}_M$ for each CH_2 group added is 11·69 ± 0·003, but, on the other hand, the conversion of an acid into its methyl ester causes a change of only 10·66, so that in general a monocarboxylic acid is more diamagnetic than its isomeric esters by about 1. Branching of a hydrocarbon chain increases $\bar{\chi}_M$, and amongst isomeric compounds the order of the isomers is *sec.* > *iso* > *n*-, the tertiary compounds being slightly greater still than the secondary.

In the case of disubstituted aromatic compounds the matter is even more confusing. Bhatnagar and his coworkers (*Phil. Mag.*, 1930 (vii), **10**, 101; 1931 (vii), **11**, 914) reached the conclusion that the susceptibilities of these isomers were in the order *o*- > *p*- > *m* and attempted to correlate the differences between their values with the sizes of the substituent groups, but no close relationship is observable. On the other hand, French (*Trans. Faraday Soc.*, 1945, **41**, 676) has found that in compounds of the type $C_6H_4X_2$, where X = OH, OMe, NH_2, Cl, NO_2 or COOH, when X is *o*-*p*-directing, the values decrease in the order $o > m > p$, whereas, when X is *m*-directive the *m*-compound has the highest value. She suggests that since an *o*-*p*-directive group repels electrons to the ring, producing a maximum electron excess in the *o*-positions, it is to be expected that a compound with two electron-repelling substituents *o*- to one another would have the highest susceptibility, since on the Langevin treatment a decrease in effective positive charge results in an increase in diamagnetism, but it would be anticipated that the effect with a *p*-compound would be greater than with the *m*-compound and only slightly less than for the *o*-compound, which is not the case. On similar reasoning it would be expected that with *m*-directive substituents the compound with the two groups *m*- to one another should have the highest susceptibility, as is observed. On the other hand, the *o*-compound would be expected to have the lowest value, but again this is contrary to observation, as is shown in the figures below :

Compounds.	χ_d			Corresponding Mono-derivative.*
	o-	*m*-	*p*-	
$C_6H_4(OH)_2$	68·76	67·26	66·47	60·7
$C_6H_4(OMe)_2$	87·39	87·21	86·65	74·05
$C_6H_4(NH_2)_2$	71·98	70·53	70·28	63·84
$C_6H_4Cl_2$	84·36	83·19	82·93	72·45
$C_6H_4(NO_2)_2$	65·98	70·53	68·30	61·53
$C_6H_4(COOH)_2$	83·61	84·64	83·51	70·2
$C_6H_4(NO_2)(COOH)$	76·11	80·22	78·81	
$C_6H_4(NO_2)(CHO)$	68·23	68·55	66·57	

* Values taken from the results of Bhatnagar.

Paramagnetic substances interact more strongly with a magnetic field than do diamagnetic substances, and hence the predominating term in the expression for χ_M is χ_μ. Each molecule or atom behaves as an elementary magnet which tends to orient itself along the lines of force, whilst thermal agitation tends to produce random orientation. By arguments analogous to those used for the case of electrical dipoles in an electrostatic field, Langevin (1905) showed that if the magnetic moment of the elementary magnet of a gaseous molecule is μ_A, then the mean moment $\overline{m}$ in the direction of the external magnetic field is given by $\overline{m} = \mu_A{}^2 H/3kT$. But by definition $\chi_\mu = M\kappa/d = MI/dH = VI/H$, where V is the molecular volume. But VI is the total moment per gram-mol. and hence $\chi_\mu = \overline{m}N/H = N\mu_A{}^2/3kT$ or $\mu_A = \sqrt{3kT\chi_\mu/N}$. The effective moment per gram-mol. ($\mu_{eff.}$) is therefore given by $\mu_{eff.} = N\mu_A = \sqrt{3NkT\chi_\mu}$ or $\sqrt{3RT\chi_\mu}$.

Evidently χ_μ should vary inversely as the absolute temperature. Actually few substances obey this law, put forward empirically by Curie, but a modified relationship $\chi_\mu \doteq c/(T - \theta)$ (Curie-Weiss Law) is more generally applicable. θ can be interpreted as the critical temperature at which the intrinsic molecular field can maintain the molecular orientation against thermal agitation. It therefore marks the temperature (Curie point) at which χ becomes infinite, *i.e.* paramagnetism passes into ferromagnetism.

An electron revolving in its orbit or spinning about its axis amounts to a circular electric current, and as such is a magnet. Classical methods show that its moment will be given by $m_e = ep/2mc$, where p is the total mechanical angular momentum and c is the velocity of light. As this momentum is quantised in units of $h/2\pi$, a unit of magnetic moment will be obtained by assuming a quantum number of unity, *i.e.* $m_e = he/4\pi mc$.

The first quantum level of an atom has one *s*, the second one *s*, and three *p*, the third one *s*, three *p*, and five *d* orbitals, each of which can accommodate two electrons with equal and opposite angular momentum, which will compensate one another (pairing or coupling). When all orbitals are not filled, however, the most stable (lowest energy) state of the atom is that in which the fewest electrons in a sub-group are coupled. The elementary magnet of paramagnetic ions and molecules can therefore be identified with the magnetic effect of unpaired electrons present. Hence only atoms or ions with incomplete electron shells are paramagnetic, and if no complications arose the magnetic moment should be a multiple of the moment due to one electron, *i.e.* m_e, known as the Bohr magneton (B.m.), which has a value of 5564 gauss-cm. for one gram-mol. Hence, expressing R in ergs, $\mu_{eff.} = \sqrt{3RT\chi}/5564 = 2{\cdot}84\sqrt{\chi_M T}$ B.m. The old empirical unit, the Weiss magneton (1123·5 gauss-cm. or 0·2019 B.m.) has no theoretical significance.

Actually, however, $\mu_{eff.}$ will not be a whole number of Bohr magnetons owing to the restrictions set by the quantum theory on the coupling of orbital and spin moments. It has been shown that, apart from a few cases, $\mu_{eff.}$ for an atom or ion can be expressed $\mu_{eff.} = g\sqrt{J(J+1)}$ B.m., where J is the inner quantum number defining the total angular momentum of the atom and g is Lande's "splitting factor."

The view that paramagnetism indicates the presence of unpaired electrons is borne out by the fact that paramagnetic ions are formed by transitional and rare-earth metals, where the $3d$ and $4f$ sub-groups of electrons, respectively, are being filled in.

When the general equation is applied to ions of transitional elements, however, the results do not accord with experiment. In the solid state and in solution, under which conditions such ions are studied, the perturbing effect of the surrounding molecules destroys or "quenches" the contribution of the orbital momentum of the electrons of the incomplete $3d$ sub-group. Hence the spin moment alone determines the paramagnetism and the Lande splitting factor becomes 2, giving $\mu_{eff.} = 2\sqrt{S(S+1)}$ B.m., where S is the total spin quantum number, *i.e.* 2S is the number of unpaired electrons (Pauling, *J. Amer. Chem. Soc.*, 1931, **53**, 1367).

Hence $\mu_{eff.}$ should increase in the case of simple ionic compounds according to the number of unpaired electrons, the theoretical values being

Number of Unpaired Electrons.	S	$\mu_{eff.}$ B.m.).
0	0	0·0
1	$\frac{1}{2}$	1·73
2	1	2·83
3	$1\frac{1}{2}$	3·88
4	2	4·90
5	$2\frac{1}{2}$	5·91

To give an indication of the value of the susceptibility itself, if $\mu_{eff.} = 1{\cdot}73$ and $T = 290°$ K., $\chi_{\mu} = (2{\cdot}84\mu_{eff.})^2/T = 1300 \times 10^{-6}$ e.m.u.

Typical values of $\mu_{eff.}$ observed for a number of ions are:

	Ion.									
	V^{4+}	$Cr^{\cdot\cdot\cdot}$	$Cr^{\cdot\cdot}$	$Mn^{\cdot\cdot\cdot}$	$Mn^{\cdot\cdot}$	$Fe^{\cdot\cdot\cdot}$	$Fe^{\cdot\cdot}$	$Co^{\cdot\cdot}$	$Ni^{\cdot\cdot}$	$Cu^{\cdot\cdot}$
No. of unpaired electrons .	1	3	4	4	5	5	4	3	2	1
observed . .	1·7	3·85	4·85	4·85	5·9	5·86	5·23	5·04	3·42	1·9
calc. . . .	1·73	3·88	4·90	4·90	5·91	5·91	4·90	3·98	3·83	1·73

As anticipated, $K^{\cdot}$, $Ca^{\cdot\cdot}$, $Zn^{\cdot\cdot}$, $Sc^{\cdot\cdot\cdot}$, $Ti^{\cdot\cdot\cdot\cdot}$, and also $Cu^{\cdot}$, all of which have no unpaired electrons, are diamagnetic.

Most free metals, particularly those of the A sub-groups, are paramagnetic, but their effective moments are less than are required for a single unpaired electron, even in the cases of metals with an odd number of electrons. This arises because in the metallic state the valency electrons form a separate mobile system (electron-gas) unassociated with the nuclei, and in this state show only feeble paramagnetism. Metals dissolved in liquid ammonia have been shown to have similar magnetic properties to free metals (Freed and Sugarman, *J. Chem. Physics*, 1943, **11**, 354).

Oxygen is paramagnetic, with $\mu_{eff.} = 2{\cdot}8$ B.m., corresponding with two unpaired electrons. This was at one time perplexing, since spectroscopic evidence shows definitely that the oxygen atoms are linked by a double linkage or the equivalent, thereby eliminating the possibility of a structure such as

$$:\underset{\cdot}{\ddot{O}}:\underset{\cdot}{\ddot{O}}:$$

with septets of electrons around each oxygen atom. It can now be explained on Pauling's suggested structure

$$:O\overset{\cdot\,\cdot\,\cdot}{\underset{\cdot\,\cdot\,\cdot}{:}}O:$$

with two three-electron bonds. On the basis of molecular orbitals it can be shown that on the gradual addition of sixteen electrons to two oxygen nuclei the formation of a stable nucleus requires the last two electrons to have uncoupled spin. The S_2 molecule in sulphur vapour is also paramagnetic, and so presumably has a similar structure to oxygen.

As would be anticipated, paramagnetic properties are also possessed by molecules with an odd number of electrons, *e.g.* NO, ClO_2, and NO_2, all of which have a susceptibility corresponding to one electron with uncoupled spin. It is noteworthy that N_2O_4 is diamagnetic whereas NO_2 is paramagnetic. Such magnetic properties can be utilised to elucidate structural problems in certain cases. Thus sodium and silver hypophosphites have the empirical formulae $NaHPO_3,3H_2O$ and Ag_2PO_3, respectively. The monomeric ion would have an odd electron, and hence be paramagnetic. Actually they are diamagnetic and hence must have the double formulae (Bell and Sugden, *J. Chem. Soc.*, 1933, 48). Similarly Fremy's salt $(KSO_3)_2NO$ is yellow and diamagnetic in the solid state, but blue and paramagnetic in solution, so it is inferred that it is dimeric in the solid and monomeric in solution.

The measurement of magnetic susceptibility has also been of great value in studying organic free radicals. These, having an odd number of electrons, are paramagnetic, and the susceptibilities observed can be used to calculate the proportion of free radical form present. For instance tri-4-diphenylmethyl, both in the solid state and in solution, has $\chi_M = 1200 \times 10^{-6}$ e.m.u., and hence is almost unassociated (Müller-Rodloff and Bunge, *Annalen*, 1935, **520**, 235). For less completely dissociated compounds χ_μ must be calculated from the observed value of χ_M and the value of χ_d deduced from Pascal's data, but owing to the uncertainty of the last term the method is limited in accuracy, particularly when the degree of dissociation is low (*cf.* Hey, *Ann. Rep. Chem. Soc.*, 1940, **37**, 265).

Amongst other organic compounds it has been found that the ketyl $(C_6H_5)_2COK$, with an odd number of electrons, has a moment of 1·45 B.m., as anticipated for one unpaired electron (Allen and Sugden, *J. Chem. Soc.*, 1936, 440).

Magnetic measurements have also proved a valuable tool in the study of the rare earth metals. It has been shown by van Vleck (*Theory of Electric and Magnetic Susceptibilities*, 1932) that the general relationship $\mu_{eff.} = g\sqrt{J(J + 1)}$ gives good agreement with most experimental data for these metals, because the incomplete 4*f* shell of electrons is screened from disturbance by a complete shell of eight electrons in the 5*sp* sub-groups, and hence their orbital components are not quenched. The susceptibilities of the metals themselves are determined almost solely by the paramagnetism of the ions, as the diamagnetism and the weak paramagnetism of the electron gas is negligible. This has been applied (Klemm and Bommer, *Z. anorgan. Chem.*, 1937, **231**, 138) to the determination of the ions present. The observed moments (in B.m.) and the values calculated for divalent, trivalent, and tetravalent ions, are shown below :

Atomic Number.	Metal.	μ_{eff}(obs.)	M^{IV}	M^{III}	M^{II}	Valencies Present.
57	La	very small	—	0·0	—	3
58	Ce	2·34	0·0	2·56	—	3,4
59	Pr	3·22	—	3·61	—	3,4
60	Nd	3·56	3·62	3·66	2·8	3,4
62	Sm	2·07	—	1·55	3·4	3,2
63	Eu	8·3	1·5	3·4	7·9	2
64	Gd	7·8	3·4	7·9	9·7	3
65	Tb	(9·0)	7·9	9·7	10·6	
66	Dy	10·9	9·7	10·6	10·6	3
67	Ho	—	10·6	10·6	9·6	
68	Er	9·5	10·6	9·6	7·6	3
69	Tm	7·6	9·6	7·6	4·5	3
70	Yb	0·0	7·6	4·5	0·0	2
71	Lu	0·0	4·5	0·0	$\gg$ 0·0	3

The observed values quoted for the elements from europium to lutecium are the paramagnetic moments calculated from the relation $\mu = 2{\cdot}84\sqrt{\chi_A(T - \theta)}$, because ferromagnetism is exhibited by this group of metals.

The other interesting field to which magnetic measurements have been applied with pronounced success is that of co-ordination compounds. In the formation of complex ions and similar compounds, very extensive electronic rearrangements may occur, leading to magnetic susceptibility values which are at first sight surprising. For instance $[Co(NH_3)_6]Cl_3$, $K_4[(Fe(CN)_6]$, and $K_2[Ni(CN)_4]$ would be expected to have effective moments of about 4·9, 4·9, and 2·8 B.m., respectively, if all the unpaired 3*d* electrons of the central atom were exerting their maximum spin. Actually these compounds are diamagnetic, as are many other co-ordination compounds such as $[Co(NH_3)_4CO_3]Cl$, $[Fe(dipyridyl)_3]Cl_2$, and the nickel glyoximes as well as the carbonyls $Fe(Co)_5$, $Fe_3(CO)_{12}$, $Co_2(CO)_8$, and $Ni(CO)_4$. On the other hand, there are many other compounds of which the structures might lead one to expect an analogous result, but which are actually found to be paramagnetic, *e.g.* $K_4[Cr(CN)_6]$ (3·3 B.m.), $K_3[Mn(CN)_6]$ (3·0 B.m.), $[Fe(NH_3)_6]Cl_2$ (5·25 B.m.), $K_3[Fe(CN)_6]$ (2 B.m.) and $[Co(N_2H_4)_2]Cl_2$ (4·93 B.m.).

A considerable measure of success in explaining this behaviour was achieved by Pauling's theory of directed wave functions (*J. Amer. Chem. Soc.*, 1931, **53**, 1367). This assumes that in the formation of complex ions from elements of the first transition series (Ti to Cu), bonds are formed by use of only the 3*d*, 4*s* and 4*p* electrons. The various combinations of these different orbitals gives rise to the various geometrical configurations of the directions of the bonds, thus:

(*a*) Combination of *s* and *p* orbitals only, or of one *s* or *p* and three *d* orbitals yields four bonds directed towards the corners of a regular tetrahedron, as in the familiar case of carbon.

(*b*) Combination of one *s*, two *p* and one *d* orbitals yields four strong bonds in one plane directed towards the corners of a square.

(*c*) Combination of one *s*, three *p*, and two *d* orbitals leads to six bonds directed towards the corners of a regular tetrahedron.

Consequently, if a $Ni^{\cdot\cdot}$ ion forms a planar complex by using one *d* orbital for the co-ordination of electrons from the co-ordinating group, its eight *d* electrons must occupy the remaining four *d* orbitals, and hence couple their spins, leading to a zero moment.

This appears to be the case, *e.g.* in the $[Ni(CN)_4]''$ ion and in the glyoxime and salicylaldehyde derivatives (Fobes and Tyson, *J. Amer. Chem. Soc.*, 1941, **63**, 3530), which on other grounds have been shown to be planar. On the other hand, in the $[Ni(NH_3)_4]^{\cdot\cdot}$ ion the NH_3 groups have been shown to be arranged tetrahedrally and must be produced by combining one *s* and three *p* orbitals. In this case, since the $Ni^{\cdot\cdot}$ ion has eight *d* electrons, two must remain with uncoupled spin, in accord with the observed moment of 2·7 B.m. Although $Ni(CO)_4$ has a tetrahedral structure, eight electrons have been added to the nickel atom, so it attains the diamagnetic krypton structure. The nickel derivatives $Ni(SR)_2$ of several mercaptans and thiophenols have been shown by Jensen (*Z. anorg. Chem.*, 1944, **252**, 227) to be diamagnetic. These must therefore be of the planar 4-covalent type, and as they cannot attain this state internally they are assumed to be highly polymerised, a conclusion in accord with their low solubilities.

The tetrahedral configuration of the groupings around the central ion also explains the paramagnetism of $[Co(N_2H_4)_2]Cl_2$, which has a moment (4·93 B.m.) approximately equal to that of the central atom.

Amongst six-covalent compounds with octahedral structure two *d* orbitals must be used, so in the $[Fe(CN)_6'''']$ ion the position is the same as for planar nickel compounds; the six *d* electrons of the $Fe^{\cdot\cdot}$ ion must occupy the remaining three *d* orbitals, leaving no electrons with unpaired spin. The same argument holds for $[Co(NH_3)_6^{\cdot\cdot\cdot}]$ since $Co^{\cdot\cdot\cdot}$ has the same electron configuration as $Fe^{\cdot\cdot}$, and also for the six-covalent compounds of Ir^{III} and Pt^{IV}. In the $[Fe(CN)_6]'''$ ion, however, there is one electron less than in the ferrocyanide ion, and hence there will be one *d* electron with unpaired spin; the observed moment of 2 B.m. accords with this view. Similarly $[Ni(NH_2OH)_6]SO_4$ and K_2ReI_6 have moments of 2·9 and 3·57 B.m., corresponding with two and three unpaired electrons, respectively (Jenson, *Z. anorg. Chem.*, 1944, **25**, 227, 317). When the central ion has very few *d* electrons, the use of two *d* orbitals in bond formation may not decrease the magnetic moment, for instance in the $[Cr(CN)_6''']$ ion there are still three *d* orbitals to be occupied by the three *d* electrons of the $Cr^{\cdot\cdot\cdot}$ ion, so the moment is the same as for the free chromic ion.

There are still many unexplained anomalies, however. Many hydrated ions and complexes containing the fluoride ion have moments approximately equal to those of the central ions, although the groups are octahedrally arranged around the central atom. Pauling has suggested that strongly electro-negative groups such as the F′ ion may be linked to the central atom by bonds which are predominantly ionic instead of covalent, whilst in hydrated ions and loose ammoniates the co-ordinated molecules may be held by electrostatic forces only. This does not mean that the bonds are of an extreme ionic type, since it has been suggested that the two forms of linkage are of limiting types, and that the co-ordinate linkage is often a bond of fractional order between pure physical and pure chemical linkages.

As pointed out by Sugden (*J. Chem. Soc.*, 1943, 328) it is difficult to accept such a simple view, as it implies that the linkages present in certain compounds are quite different to those in other analogous compounds, although there is a close similarity in physical properties. Thus the acetylacetonates $Al(acac)_3$, $Fe(acac)_3$, and $Co(acac)_3$ have $\mu_{eff.}$ 0, 5·9 and 0 B.m., respectively, whereas they should be approximately 0, 1·8, and 0 for chemical linkages or 0, 5·9, and 4·9 for physical linkages. All are volatile and soluble in non-polar solvents. Also $[Ni(dipy)_3]Br_2,6H_2O$ has a moment of 3·0 B.m., so none of its 3*d* levels can be used to form the linkages, and so on Pauling's theory these must be ionic links which are not broken in solution. The analogous iron compound has zero moment, in accord with a chemical linkage.

A rather different approach was made by van Vleck (*J. Chem. Physics*, 1935, **3**, 803), who attributes the fall in magnetic moment produced by the formation of a complex ion to a strong perturbation of the incomplete *d* sub-group through the close approach of other atoms. Such interactions, if powerful enough, can not only quench the orbital contribution to the moment, mentioned in connection with the transitional elements, but also cause pairing of spins. The most stable state of the atom is that of maximum spin, but the energy associated with atomic interactions is a minimum when the spin is least. Hence there are two opposing tendencies in the formation of a complex; if the interactions are not too great the tendency towards maximum spin predominates, whereas very strong interaction causes the *d* electrons to pair their spins. On this view, then, a decrease in magnetic moment on formation of a complex indicates a more intimate bonding. As pointed out by Pauling, however (*Nature of the Chemical Bond*, 1940), this does not occur in the complex of iron with fluorine, the most electronegative of all atoms, and hence it would not be expected to occur in any complex. On the other hand, van Vleck's view does give a picture of the variations in behaviour encountered, but it must be assumed that quite small changes in the nature of the co-ordinating groups can have a pronounced effect on the coupling of spins in the central ion. This subject has been studied very considerably by recent workers (*cf.* Sugden, *loc. cit.*).

Finally, mention may be made of the application of magnetic methods to the detection of the presence of various ionic species in solution. For instance, Das and Ray (*J. Indian Chem. Soc.*, 1944, **21**, 159) measured the magnetic susceptibilities of solutions of sodium molybdate progressively acidified with nitric acid. They found that the susceptibility-*p*H curve obtained showed four inflexion points corresponding with the Mo_3O_{11}'''', HMo_6O_{21}''''', $H_3Mo_60_{21}'''$, and $H_7Mo_{12}O_{41}'''$, respectively, but could obtain no evidence for the existence of $H_2Mo_6O_{21}''''$ in solution.

BIOCHEMISTRY. By W. O. KERMACK, D.Sc., LL.D., F.R.S., Royal College of Physicians' Laboratory, Edinburgh.

STARCH AND GLYCOGEN.—Amongst the many advances in biochemistry of the past ten years, the clarification of the structure of starch and glycogen and the enzymatic *in vitro* synthesis of these substances is of special importance because of the almost universal occurrence of such polysaccharides in plant and animal cells, and the part they play in industry and human and animal nutrition. Ten years ago, even the general plan of the starch molecule was highly controversial. There is now a very surprising degree of agreement as to the main features of the structure of starch and glycogen, and at the same time the rôle of various enzymes in the breakdown and synthesis of these polysaccharides has been ascertained.

STRUCTURE OF STARCH.—Ordinary starch is not a single substance. This has been suspected for some time, but it is only recently that the nature of the component fractions has been established, and suitable methods of separating them developed. It is now generally agreed that most starches consist of two distinct fractions, amylose and amylopectin. Perhaps the most striking difference between these two substances is that amylose gives an intense blue colour with iodine, whilst with amylopectin the colour is violet or red with little trace of blue. Physico-chemical investigation indicates that the particles of amylose are relatively long and thread-like and those of amylopectin are much more nearly spherical and compact.

Both amylose and amylopectin are formed from molecules of glucose, joined together by glucosidic linkages, the type of linkage present in maltose. All the glucose molecules are present as the pyranose, or six-membered ring form. Amylose is the simpler of the two substances. It consists of a long chain of glucose units, each one being united by an oxygen atom (the glucosidic link) from its own first carbon atom to the fourth carbon atom of the next. As the chain of glucose units is an open one, the first and last units have only one neighbour, and only one link each. There are three hundred or more such linearly arranged glucose units in the amylose molecule.

The architecture of amylopectin is somewhat more complicated. Here the chain of glucose units is not straight but is branched. The molecule which is considerably larger than that of amylose—it may consist of several thousand glucose units—contains many branch-points. These branch-points occur when the glucose unit, besides being joined to two adjacent units through the first and fourth carbon atoms, is also attached to a third unit through carbon atom number 6. This third glucose unit is attached to carbon atom number 6 by its own first carbon atom, through a glucosidic link, and it forms the first unit of a side-chain which itself may be branched. The details of the resulting multiple-branched structure are still obscure, but it is known that there is about one branch end-point to every twenty to thirty glucose units, and the end branches may contain between ten to twelve units each.

These proposed structures for amylose and amylopectin are based on a large amount of physical and chemical evidence, to which the work of the British school, led by Haworth and Hirst, has decisively contributed. They are in conformity with the fact that the amylose molecule is long and thread-like while that of amylopectin is more nearly spherical and compact. The difference in the action of the enzyme, β-amylase, is also readily explained; the characteristic action of this enzyme is to split off maltose, that is pairs of glucose units, from the end of a chain. With the straight-chain amylose molecule, this process can proceed until all the chain, save perhaps one odd glucose unit, is converted into maltose, but, with amylopectin, the process stops when a glucose unit forming a branch-point is reached, for this involves a configuration unsuitable for the action of the enzyme. The consequence is that only about half the amylopectin is converted into maltose; the rest is left as a dextrin resistant to the action of β-amylase.

SEPARATION OF AMYLOSE AND AMYLOPECTIN.—During recent years, important methods have been introduced for separating amylose and amylopectin from natural starch. Schoch (*J.A.C.S.*, 1942, **64**, 2957) found that suitable treatment of starch paste with butanol resulted in the separation of an insoluble amylose-butanol complex, from which amylose of high purity could be obtained, and from the mother-liquor amylopectin, practically free from amylose, could be recovered. Haworth, Peat, and Sagrott (*Nature*, 1946, **157**, 19) have improved this method by using thymol in place of butanol. From the starch solution containing sodium chloride and saturated with thymol, the amylose-thymol complex separates in forty-eight hours. Cyclohexanol and other alcohols may also be used, and a mixture of cyclohexanol and thymol which precipitates amylose, contaminated with some amylopectin, is very useful, as the amylopectin left in solution appears to be unusually free from amylose.

Haworth, Peat, and Sagrott obtained from potato starch by the thymol method a yield of amylose varying from 20 to 22 per cent. of the weight of the starch. The relative proportion of the two components varies in different types of starch; the proportion of amylose is usually between 15 and 25 per cent., but certain "waxy" starches seem to consist entirely of amylopectin.

STRUCTURE OF GLYCOGEN.—The molecule of glycogen resembles that of amylopectin rather than that of amylose. It differs from the amylopectin molecule in two respects; it is larger, and it is more intensely branched. The molecules of glycogen are probably not all of the same size, but in general seem to have molecular weights of several millions, and therefore contain tens of thousands of glucose units. The branches are on the average only about half as long as those of amylopectin. The consequence is that the molecule of glycogen is large, compact, and very nearly spherical.

THE SYNTHESIS OF STARCH AND GLYCOGEN *IN VITRO*.—Two different types of enzymes are found in plant and animal cells which break up the molecules of amylose, amylopectin, and glycogen. The amylases, such as the β-amylase already referred to, split the molecule with the addition of water. This hydrolytic fission of starch or glycogen is accompanied by the liberation of appreciable heat, and is definitely not reversible. The other type of enzyme attacks the polysaccharides in the presence of inorganic phosphate. Fission takes place with the addition of a molecule of phosphoric acid, in place of a molecule of water; terminal glucose units are split off and the product is glucose-1-phosphate; such an enzyme is called a phosphorylase. Glucose-1-phosphate is often called the Cori ester as it was first discovered by Cori and Cori (*Proc. Soc. Exp. Biol. Med.*, **34**, 702–5) in 1936 (see Fig. 1).

The first indication that the reaction polysaccharides + phosphate $\rightleftharpoons$ glucose-1-phosphate is reversible was published by Schäffner and Specht (*Naturwiss.*, 1938, **26**, 494) who reported that a phosphorylase present in yeast, when incubated with glucose-1-

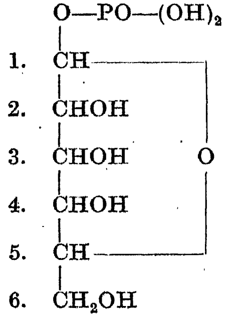

FIG. 1.—Glucose-1-phosphate (Cori ester).

phosphate, liberated inorganic phosphate, and produced a product apparently glycogen, which gave the brown colour with iodine characteristic of this polysaccharide.

From this point progress was rapid. The results of Schäffner and Specht were confirmed by Kiessling (*Biochem. Zeit.*, 1939, **302**, 50–76) who purified yeast phosphorylase from associated enzymes and, *inter alia*, showed that in presence of this phosphorylase the reaction glycogen + inorganic phosphate $\rightleftharpoons$ glucose-1-phosphate is a truly reversible one, reaching the same equilibrium position from both sides. Ostern and Holmes (*Nature*, 1939, **144**, 34) reported that liver brei in presence of fluoride catalysed the formation of a glycogen-like substance from glucose-1-phosphate. Cori, Schmidt and Cori (*Science*, 1939, **89**, 464) prepared a phosphorylase from muscle and observed that the activity of the enzyme so obtained depended on the presence of adenylic acid presumably as co-enzyme. From hexose-1-phosphate it synthesised a polysaccharide giving a blue colour with iodine.

About the same time a thorough investigation of plant phosphorylase was being made by Hanes, and his results were published in two important papers (*Proc. Roy. Soc.*, 1940, **B**, **128**, 421; 1940, **B**, **129**, 174). He first worked with extracts of ungerminated peas and later with potato juice phosphorylase, which could be readily freed from amylase. When he allowed this phosphorylase to act on glucose-1-phosphate he found that a starch-like material separated from solution, and that at the same time inorganic phosphate was liberated. It was clear that polysaccharide synthesis had been brought about, and as the product gave a deep blue colour with iodine, it appeared to be a true *in vitro* synthesis of starch. Further investigations, chemical, physical, and physico-chemical, confirmed this conclusion with one important reservation. We have already seen that starch consists of both amylose and amylopectin. Hanes' synthetic starch agrees very closely in its properties with amylose; it is, indeed, synthetic amylose, but the chains are shorter than those of natural amylose, as they contain only about 100 glucose units.

Hanes and the Coris, like Kiessling, found that a true equilibrium existed between the glucose-1-phosphate on the one hand and the polysaccharide + inorganic phosphate on the other. Starting with glucose-1-phosphate the phosphorylases of different origin all synthesise a polyhexose; those from yeast and muscle produce an amylose-like compound giving a blue colour with iodine, whilst those from liver, heart, or brain produce a substance closely resembling glycogen.

Hanes (*loc. cit.*) drew attention to a very interesting phenomenon; an induction period was observed when glucose-1-phosphate was incubated in presence of phosphorylase. The conversion into starch was at first very slow, but the rate of the reaction increased gradually with time. This induction period could be abolished and the reaction much accelerated by the addition of a small amount of natural starch, and certain other polysaccharides have a similar effect. Hanes thought that maltose also catalysed the reaction, but later work (*cf.* Green and Stumpf, *J. Biol. Chem.*, 1942, **142**, 355) indicates that pure maltose is inactive, though ordinary maltose, which contains traces of dextrins, has a positive effect.

This dependence of the rate of the synthetic action of phosphorylase on the presence of preformed polysaccharide was also observed by other workers. Cori and Cori (*J. Biol. Chem.*, 1939, **131**, 397) using muscle phosphorylase found that glycogen promoted the reaction, which did not proceed in its absence. Starch has also an activating effect on this phosphorylase. Hanes had observed that the acceleration of the synthetic activity of potato phosphorylase by the amylose synthetically produced was not so great as by natural starch. Green and Stumpf came to the conclusion that synthetic starch is without any activating effect. Synthetic amylose is likewise ineffective with muscle phosphorylase.

With regard to the animal phosphorylases in general, the field has been dominated by the very impressive and thorough work of C. F. and G. T. Cori and their colleagues. Recently (*J. Biol. Chem.*, 1943, **151**, 21, 31, 39, 57; 1945, **158**, 321, 333, 341) these investigators have recorded an important advance, the crystallisation of muscle phosphorylase. Actually, two forms, A and B, of the enzyme were obtained. One, A, was highly crystalline; it contained a prosthetic group firmly united and non-dialysable, and it was active without the addition of adenylic acid. The other form, B, was less crystalline; it was by itself completely inactive but it was activated on the addition of adenylic acid. An enzyme exists in rabbit muscle which converts the A form into B, with the disappearance of the prosthetic group from the molecule; it is also removed by trypsin. In neither case does free adenylic acid appear, but a product containing ribose and phosphate is split off the enzyme molecule. Evidently adenylic acid is the co-enzyme of phosphorylase B; in the phosphorylase A this co-enzyme, or something closely related, is firmly attached to the rest of the enzyme molecule, whilst the B form lacks such a prosthetic group.

The polysaccharide formed from glucose-1-phosphate in presence of crystalline muscle phosphorylase is of the amylose type, composed of long straight chains of glucose units. Cori, Cori, and Green (*J. Biol. Chem.*, 1943, **151**, 57) demonstrated that when a supplementary enzyme obtained from liver, but devoid of phosphorylase activity, was also added the polysaccharide formed no longer gave a blue colour with iodine, but resembled glycogen, and evidently contained branched chains.

Very recently, Peat and Bourne (*J.C.S.*, 1945, 877) have published an investigation of the synthesis of starch by the enzymes present in potato juice. It will be recalled that Hanes, working with a purified, but not crystalline, phosphorylase, obtained a product from glucose-1-phosphate of the amylose type. Peat and Bourne now find that the crude juice contains a second enzyme which can be separated from the phosphorylase; they call the phosphorylase proper P-enzyme and the other, Q-enzyme. This Q-enzyme has the property of converting natural or synthetic amylose into a polysaccharide resembling amylopectin and evidently containing branched chains. When glucose-1-phosphate is incubated in presence of both enzymes an amylopectin-like product is formed; Q-enzyme alone appears to be without effect on glucose-1-phosphate, but this conclusion requires confirmation, as Q-enzyme absolutely free from P-enzyme has not yet been obtained.

There is evidently a close similarity between the potato phosphorylase and the muscle phosphorylase systems. Both bring about the same chemical reaction, both produce the linear polyhexose when no other enzyme is present, both establish an equilibrium between amylose and phosphate on the one hand and glucose-1-phosphate on the other. But there are also well-marked differences. Muscle phosphorylase requires a prosthetic group, either present in the crystalline enzyme (phosphorylase A), or added as adenylic acid to phosphorylase B. The potato enzyme requires no co-enzyme nor does it appear to contain a prosthetic group related to adenylic acid. The Q-enzyme of the potato converts the linear amylose into the branched amylopectin, whereas the glycogen-forming supplementary liver enzyme of Cori, Cori, and Green does not act on amylose, but has to be present along with phosphorylase during the process of polyhexose formation. It appears to be a phosphorylase, bringing about the addition of a glucose unit as ordinary phosphorylase does but effecting a 1 : 6 linkage, and thus introducing a branching of the chain. Indeed, by varying the relative proportions of the two enzymes, a product with different degrees of branching, resembling glycogen, amylopectin or amylose, may be formed at will.

Let us return for a moment to the very interesting phenomenon of the dependence of the synthetic reaction on preformed polysaccharide. Cori, Cori, and Green showed that the linear amylose formed synthetically with crystalline phosphorylase has no autocatalytic effect on the synthetic reaction; this accords with the similar result obtained by Green and Stumpf using potato phosphorylase. With these enzymes, the reaction is not autocatalytic. When, however, the glycogen-forming enzyme systems of heart or liver are used, autocatalysis is observed, the rate of the reaction increasing owing to the activating effect of the glycogen produced. Very striking is the observation that the reaction promoted by the combination of muscle enzymes and supplementary liver enzyme is also autocatalytic. In fact, the synthetic glycogen produced in this way is just as active in promoting the synthesis as a specimen of ordinary liver glycogen. It is not yet clear whether the synthetic amylopectin produced by the combination of the P- and Q-potato enzymes on glucose-1-phosphate is active like natural amylopectin; if it were, it would explain why Hanes, using P-enzyme which might have contained a little Q, observed some autocatalysis, while Green and Stumpf, with presumably more highly purified P-enzyme, could detect no trace of this effect. In any event if we regard the buffer solution containing, say, phosphorylase, the liver enzyme, and glucose-1-phosphate as a medium, which is sterile until we inoculate it with a trace of glycogen, we have a very interesting analogy to bacterial growth. The glycogen multiplies but not so as to entirely exhaust the medium. The phosphate formed plays the rôle of a toxic by-product. The new glycogen could be used to inoculate other flasks and so "growth" could be continued indefinitely. The analogy, of course, has its limitations but it is suggestive.

THE GLUCOSE PHOSPHATES AND CARBOHYDRATE METABOLISM.—In conclusion, some indication may be given of the relation of glucose-1-phosphate to some of the other aspects of carbohydrate metabolism. It has been known for some time that an enzyme exists, phosphoglucomutase, in presence of which an equilibrium is established between glucose-1-phosphate (Fig. 1) and glucose-6-phosphate (Fig. 2). Now the phosphorylation of glucose demands energy, so that it is not accomplished directly from glucose and inorganic phosphate, even in presence of an enzyme; the phosphate must be supplied in a " high energy " form. For this purpose adenosinetriphosphate (ATP) is effective. In presence of the enzyme, hexokinase, it reacts with glucose and forms glucose-6-phosphate. ATP is present in yeast cells and in many plant and animal tissues; it acts as a vehicle for storing the energy liberated in various oxidative and glycolytic reactions, and supplies it, when required, to bring about some other chemical process. Colowick and Sutherland (*J. Biol. Chem.*, 1942, **144**, 423) were able to demonstrate the synthesis of polysaccharide from glucose in presence of the enzymes, hexokinase, phosphoglucomutase, and phosphorylase, provided an adequate supply of ATP was present.

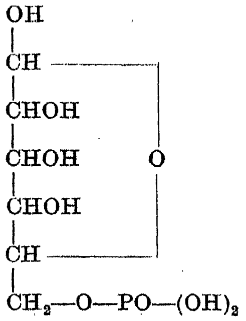

FIG. 2.—Glucose-6-phosphate.

Enzyme systems also exist which convert the glucose-6-phosphate into fructose-6-phosphate, and hence into the Harden and Young hexose-diphosphate and from this the well-known Ebden-Meyerhof (*cf.* SCIENCE PROGRESS, 1934, **28**, 686) series of glycolytic reactions proceeds. The present evidence is that the essential reactions, concerned with the utilisation and breakdown of carbohydrates, are very much the same in the yeast cell, in plants, and in animals. The relative rates and even the direction of the separate reactions may vary according to local conditions, and the end products may differ within certain limits, but the basic scheme appears to be the same.

For further information, the reader may consult the following articles and reviews: W. Z. Hassid, The Molecular Constitution of Starch and the Mechanism of its Formation (*Quart. Rev. Biol.*, 1943, **18**, 311–30); K. H. Meyer, The Chemistry of Glycogen (*Advances in Enzymology*, 1943, **3**, 109–35); W. N. Haworth, The Structure, Function and Synthesis of Polysaccharides (Bakerian Lecture) (*Proc. Roy. Soc.*, 1946, **A 186**, 1–20); Symposium on the Formation of Disaccharides, Polysaccharides and Nucleosides (*Fed. Proc.*, 1945, **4**, 226–52).

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., D.Sc., F.R.S.E., The University, Glasgow.

IGNEOUS ROCKS: GREAT BRITAIN, NORTH ATLANTIC, NORTH AMERICA.—An important paper by S. I. Tomkeieff deals with "The Basalt Lavas of the Giant's Causeway District of Northern Ireland" (*Bull. Volc., Naples*, Sér. II, VI, 1940, 89–143). The famous columnar basalt lava belongs to an upper series of olivine-free tholeiitic basalts and projects seaward in contact with a lower series of olivine-dolerite lavas. A new terminology of columnar lavas is proposed; the lower zone of regular columns is called the *colonnade*, and the upper part of the flow with its wavy and often confused columns is referred to as the *entablature*. In a fully developed columnar flow the entablature is divided into a lower "curvi-columnar" and an upper "pseudo-columnar" zone. The three main zones are explained as the result of varying velocity of movement of the isotherms during the cooling of the flow.

Dr. Tomkeieff has also described "The Tertiary Lavas of Rum" (*Geol. Mag.*, LXXIX, 1942, 1–13) as dolerites and basalts, trachybasalts ("mugearites"), and amygdaloidal trachybasalts. He shows that the so-called "mugearites" of Rum and Canna are much nearer in composition to the tholeiitic basalts than to the type-rocks of Skye. The amygdaloidal rocks contain about 50 per cent. of amygdaloidal material, and the author questions its origin in the usual way by infiltrations into the vesicles. He inclines to favour a "hypothesis of liquid immiscibility according to which the amygdaloidal rock is really nothing but a solidified macro-emulsion." He is careful to state that this hypothesis is applicable only to the occurrence on Rum.

In a further work "On the Petrology of the Ultrabasic and Basic Plutonic Rocks of the Isle of Rum," Dr. Tomkeieff (*Min. Mag.*, XXVII, 1945, 127–36) states that the Tertiary plutonic rocks of Rum consist of (1) an ultrabasic peridotite—allivalite banded complex, intruded by (2) olivine-eucrite, and (3) an ad-joining granophyre of a later period. Dr. Tomkeieff discusses the order of crystallisation of the minerals in the ultrabasic rocks, which appears to have been conditioned by the composition of the initial magma-fractions; origin of the banding; and the parallelism between the mechanism of differentiation in the ultrabasic and basic magmas.

S. R. Nockolds and J. E. Richey describe "Replacement Veins in the Mourne Mountains Granites, North Ireland" (*Amer. Journ. Sci.*, **237**, 1939, 27–47), which are mostly of the greisen type. The chief variety contains abundant siderophyllite of which a new analysis is given. Later fissure veins carry fluorite, apophyllite, and an iron-rich chlorite (daphnite). The chief constituents added during the metasomatism were iron, fluorine, and water, with minor amounts of aluminium and lithium. Concomitantly, nearly all the sodium, and some of the potassium, calcium, and silicon, were removed from the affected rock.

"Late-stage Veins in the Cone-sheet of Bloody Bridge, Mourne Mountains," a paper by E. M. Patterson (*Irish Nat. Journ.*, VIII, 1943, 72–6), is a description, with a good analysis, of an epimagmatic vein of granophyre penetrating a Tertiary cone-sheet of quartz-feldspar-porphyry. It is correlated with the aplite veins of the Mourne Granite (see above).

Veins similar to those of the Mourne Granite mark the contacts between the two granites of the Northern complex of Arran, as is shown by W. R. Flett in his paper on "The Contact between the Granites of North Arran" (*Trans. Geol. Soc. Glasgow*, XX, Pt. 2, 1942, 180–204). This and other points emphasise the remarkable similarity between the two granites. The re-survey of the contact between the fine and coarse granites confirms the view of earlier investigators that the fine-grained Inner Granite is intrusive into the coarse Outer Granite. The contact is shown to be mostly wall-like, and no support was found for the view that the Inner Granite forms anywhere a roof to the Outer Granite.

The majority of the dikes in "The Killough-Ardglass Dyke Swarm," described by S. I. Tomkeieff and C. E. Marshall (*Quart. Journ. Geol. Soc.*, XCVI, 1940, 321–38), consist of olivine-dolerites and olivine-basalts with some associated spilites, which are referred to an alkali-enriched plateau-magma type. Dikes of trachybasalts are associated with the above and are connected with them by transitional varieties. The magmatic characteristics and igneous sequence in this swarm are quite different from those of the adjacent Antrim and Mourne Swarms.

A valuable summary and discussion on "The Dykes of Scotland" has been given by J. E. Richey (*Trans. Edin. Geol. Soc.*, XIII, Pt. IV, 1939, 393–435). In Scotland, the origin of the term "dyke," there are appropriately no fewer than eight periods of dike intrusion. Dr. Richey discusses the upward and downward extension of the dikes; whether in some cases they represent the feeders of lava flows; the parallelism between the dikes and tectonic lines of the same general age; dike-echelons (Permo-Carboniferous) possibly due to torsional stresses; and the ultimate origin of dike fissures.

Writing on "The Rhythmic Eruptions of Ben Hiant, Ardnamurchan, a Tertiary Volcano," J. E. Richey (*Bull. Volc., Naples*, Sér. II, III, 1938, 1–21) shows that the mountain consists of two or perhaps three volcanic vents, mainly filled with agglomerate and tuff together with the remnants of their bounding walls, and of a transgressive intrusion of quartz-dolerite which forms the central peak. The main lavas involved are trachyte, andesitic pitchstone, and porphyritic basalt. Two stages of eruption occurred in these volcanoes: (1) formation of the initial cavity by explosion; and (2) infilling by the products of a series of minor explosions.

S. I. Tomkeieff describes the "Petrology of the Carboniferous Igneous Rocks of the Tweed Basin" (*Trans. Edin. Geol. Soc.*, XIV, Pt. 1, 1945, 53–75). The lavas to the north of the Eden Water are mainly of Dalmeny and Jedburgh types, but in the Eden, Tweed and Teviot sections the lower group of lavas consist of Jedburghs and Markles, the upper group of Dalmeny and Dunsapie types. The lavas are highly altered, and in the author's opinion the alteration is endogenous and due to the richness of the magma in water and CO_2.

"The Carboniferous Lavas of the Ballycastle District" described by the same author (*Irish Nat. Journ.*, VIII, 1943, 67–72) are mainly of Jedburgh and Markle types. At Murlough Bay an altered basalt has an ellipsoidal upper part, but it is not certain whether this is a true pillow-lava.

E. M. Patterson has described "A Xenolithic Minor Intrusion at Slievenagriddle, Co. Down" (*Geol. Mag.*, LXXIX, 1942, 297–311). The main rock of the intrusion, which is of Caledonian age, is identified as markfieldite. Its marginal parts are crowded with xenoliths of sediments and vein-quartz. Feldspathic replacement rims and rheomorphic veins are associated with the xenoliths, and are derived from them by the metasomatic action of the magma.

In describing "The Lundy Complex: its Petrology and Tectonics," A. J. T. Dollar (*Quart. Journ. Geol. Soc.*, XCVII, 1941, 39–77) correlates the Lundy Slate Series with the Morte Slates of North Devonshire. The granites intrusive into them are so like those of Cornubia that they are assigned to the same parentage and age as the latter. A dike-swarm consisting of 120 major and 30 minor dikes, with six sills, cuts both the slates and the granite. They consist mainly of crinanites and olivine-dolerites, with quartz-dolerites and tholeiites, and a few orthophyres and trachytes. The main trend of the swarm is north-west to south-east and this, together with its petrographical characters, permits the author to assign it to the Kainozoic.

"The Igneous Rocks of the Stanner-Hanter District, Radnorshire," described by N. Holgate and K. A. Knight Hallowes (*Geol. Mag.*, LXXVIII, 1941, 241–67), consist of fine-grained dolerites in thin sills, followed by gabbro possibly in laccolithic form, and an aplogranite magma with associated hybrid rocks. Later on quartz-porphyries and dolerites were intruded. These rocks occur along the line of the Church Stretton belt of disturbance and are now regarded as inliers of Pre-Cambrian age.

The minor intrusions which intersect the folded Ordovician sediments and pyroclastic rocks of the Shelve District of Shropshire are described by F. G. H. Blyth (*Quart. Journ. Geol. Soc.*, XCIX, 1944, 169–204) as mainly basic rocks, but ranging from picrite to alkali-rich andesite. They include the famous Corndon Phacolith of dolerite which occupies a faulted dome in shales near the main anticlinal axis of the area. The intrusions are believed to have accompanied the folding that occurred at the end of Ordovician times.

"Age Determinations of Carboniferous Basic Rocks of Shropshire and Colonsay," by W. D. Urry and A. Holmes (*Geol. Mag.*, LXXVIII, 1941, 45–61), carried out by the helium method for fine-grained basic igneous rocks, have shown that the olivine-basalt of Little Wenlock is of Lower Carboniferous age, the analcite-olivine-dolerite of the Clee Hills Upper Carboniferous, and the monchiquites of Colonsay late Carboniferous or Permian, with considerable probability in favour of the latter. The last-named therefore appear to belong to the same magmatic period as the Permian provinces of Oslo and the Midland Valley of Scotland.

"The Barrow Hill Intrusion, South Staffordshire," described by C. E. Marshall (*Quart. Journ. Geol. Soc.*, CI, 1946, 177–205), is a markedly transgressive and steeply-plunging mass of fine-grained analcitic olivine-basalt which cuts the Middle Coal Measures and Etruria Marl. Its form has been determined not only by geological but by magnetic and resistivity survey. The intrusion carries numerous xenoliths which have been greatly altered by the activity of volatiles and late magmatic solutions. It is regarded as one of the feeders or vents of the Midland Cycle of Coal Measures—Post-Carboniferous volcanic activity.

A. G. Jhingram describes "The Cheviot Granite" (*Quart. Journ. Geol. Soc.*, XCVIII, 1943, 241–54) as of laccolithic form and as consisting of three main types: a fine-grained plagioclase-rich marginal rock, a central area of coarse granite, and an intermediate granophyric variety. The first two varieties carry both orthorhombic and monoclinic pyroxenes, but the granophyric type has biotite instead of pyroxene. Variability in the granite mass does not seem to result from contamination by the surrounding lavas, but Silurian greywackes and shales may have been responsible for the observed effects, although the poorness of the exposures introduces much uncertainty.

In continuation of the studies of the Oslo petrographic region now proceeding (see SCIENCE PROGRESS, XXXIV, July 1946, p. 595), E. Saether has described "The South-eastern Part of the Baerum-Sörkedal Cauldron" (*Norske Vidensk.-Akad. Oslo, I. Math.-Nat. Kl.*, 1945, No. 6, 60 pp.). This area is part of a cauldron of subsidence along a ring-fissure which is filled with nordmarkite-porphyry. The adjacent external bed-rock consists of Ordovician and Silurian sediments from which the overlying rhomb-porphyry lavas have been removed by denudation. Inside the ring-dike the lavas have been preserved, and the subsidence is thus calculated to amount to between 1000 and 2000 m. The lavas are injected by subjacent masses of syenite and akerite. Finally, a large intrusion of nordmarkite at the roots of the volcano effected contact-metamorphism in both the lavas and sediments.

J. H. Lindal has described "The Interglacial Formation in Viðidal, Northern Iceland" (*Quart. Journ. Geol. Soc.*, XCV, 1939, 261–74). It is 70 m. thick and consists, in ascending order, of moraines and fluvio-glacial deposits; varved claystones (lacustrine); fossiliferous sandstones with remains of alder, birch and willow, heather and grasses; river gravel and lavas. They occur on the floor of a broad valley cut in the Tertiary lavas, and owe their preservation to a covering of basaltic lavas erupted at the end of the Interglacial stage. It is thought that the phenomena indicate a major retreat of the ice sheet, not merely a local recession and re-advance.

A unique feature of Icelandic volcanicity is eruption under glaciers and ice sheets, the characteristic product of which is the "Palagonite" formation. A. Noe-Nygaard has made a penetrating study of "Sub-Glacial Volcanic Activity in Ancient and Recent Times (Studies in the Palagonite System of Iceland, No. 1)" (*Fol.*

Geogr. Danica, I, No. 2, 1941, 67 pp.), dealing, in turn, with its manifestations in the Siða district in south central Iceland, as found below present-day ice sheets especially the Vatnajökull, and as revealed along the ice margins in the zones of oscillation of recent glaciers. He establishes a connection between tillites on the one hand, massive globular basalts and their breccias and associated intrusions on the other. He thus confirms, as he says, Peacock's sub-glacial extrusion hypothesis. The very characteristic "globular basalts" are believed to be due to melt-water rather than to actual contact with the ice. Palagonitisation, too, is regarded as due to glacial drainage water and, as a rule, began immediately after the outburst of sub-glacial volcanicity.

Skjaldbreið is one of the most regularly-formed and perfect of the small shield volcanoes of Icelandic type. T. Tryggvason has made a thorough petrographic study of its products with six new chemical analyses (*Bull. Geol. Inst. Upsala*, XXX, 1943, 273–320). The lavas are basalts which have a remarkably high content of CaO and small amounts of alkalis—a chemical feature which seems to be characteristic of all the shield volcanoes of Iceland. According to this author the shield volcanoes may be analogous to the fissure eruptions, the difference being that fissure eruption takes place much more rapidly than that of shield volcanoes.

T. Krokström's memoir "Petrological Studies on some Basaltic Rocks from East Greenland" is accompanied by a long Appendix by H. G. Backlund on "The Field Position of some Basalts intermediate between the Northern and Southern Areas in East Greenland" (*Medd. om Grönland*, **103**, No. 6, 1944, 75 pp.). The basalts are Tertiary and come mainly from the Hurry Fjord area on Scoresby Sound. They consist mainly of olivine-dolerite, plagioclase-porphyrite, and basalts. The plagioclase-porphyrite appears to be the same as the porphyritic basalts of the Tertiary in West Scotland. Three new chemical analyses are provided as well as a wealth of mineralogical detail. Backlund's Appendix is mainly devoted to showing that sill-intrusion may have been instrumental in building the thick and regular basalt formation of East Greenland to a much greater extent than has hitherto been thought.

In a discussion of "Fractional Crystallisation of Basaltic Magmas" H Kuno (*Jap. Journ. Geol. & Geogr.*, XIV, 1937, 189–208) uses analyses of basalts from Mull, Hawaii, the Deccan, the Karroo, Oregon and Iceland, and shows that there is a definite paragenetic relation between the normative feldspars and pyroxenes. Feldspars comparatively rich in An and Or tend to consort with pyroxenes rich in En alone or in En and Wo ; whereas feldspars rich in Ab tend to be associated with pyroxenes rich in Fs and Wo, or in Fs alone. These relations are held to imply that the basalts are differentiates from a more basic magma which is identified as olivine-rich eucrite. This parental magma may give rise to an alkaline series on the one hand, or to a calc-alkali series on the other, according to differences in the degree of reaction between the early-formed crystals and the residual liquids.

The "Igneous Geology and Structure of the Mount Taylor Volcanic Field, New Mexico," has been described by C. B. Hunt (*U.S. Geol. Surv. Prof. Paper*, 189-B, 1938, 51–80). The first eruptions of dacite and andesite took place in Miocene times from Mt. Taylor, a conspicuous volcano which, although considerably lowered by erosion, still shows a central caldera. It is estimated to have included at least 12¼ cubic miles of eruptive rocks. This eruption was followed in Pliocene times by a large number of basalt sheets erupted from small cones. Erosion has cut downwards about 2000 ft. and the lavas now cap high mesas, while the volcanoes are marked by prominent plugs of basalt and volcanic breccia.

Mt. Baker, the northernmost of the great Cascade volcanoes, has been described by H. A. Coombs (*Bull. Geol. Soc. Amer.*, **50,** 1939, 1493–510). The actual cone is 6000 ft. in height and 8 miles in diameter. Eruption began in the Pleistocene and continued into historic times. The lavas are all pyroxene-andesites containing a hypersthene with inclined extinction.

"The Petrographic Province of Central Montana," described by E. S. Larsen (*Bull. Geol. Soc. Amer.*, **51,** 1940, 887–948), is arbitrarily defined to include the Tertiary igneous rocks along the eastern flanks of the Rocky Mountains from the Yellowstone Park on the south to the Canadian boundary on the north. Each of the numerous mountain ranges in this region consists of one or more sub-provinces of igneous rocks. Thus, the Yellowstone Park contains at least three, the Highwood Mountains four, and the Bearpaw Mountains seven. The felsic rocks of all the provinces tend towards a common granitic type, but the mafic rocks differ much more widely. They range from basalts or gabbros to orthoclase-gabbros, plagioclase-shonkinites, and ordinary shonkinites. Richness in potash is a common but not invariable chemical characteristic. The primary magmas are believed to be represented by the mafic rocks, and to have been derived by the removal of calcic plagioclase and hypersthene from primordial basalt in depth. Variation in each sub-province is ascribed to crystal settling combined with assimilation of granitic material in those provinces in which quartzo-feldspathic rocks are present.

The comprehensive monograph, " Igneous Rocks of the Highwood Mountains, Montana," by E. S. Larsen, C. S. Hurlbut, Jr., D. Griggs, B. F. Buie, and C. H. Burgess (*Bull. Geol. Soc. Amer.*, **50**, 1939, 1733–868), consists of seven parts, dealing respectively with the Laccoliths, the Extrusive Rocks, Dikes and related Rocks, the Stocks, Contact Metamorphism, Mineralogy and Petrology. A contribution of outstanding importance is that on the Laccoliths, by C. S. Hurlbut, in which the famous Shonkin Sag intrusion of great controversial interest is dealt with. The field and laboratory observations on this and the other laccoliths are explained on the basis of the theory of differentiation in place. This theory postulates the settling of heavy crystals accompanied by minor rising of leucite in the cooling body of magma.

W. T. Pecora, in a paper on " Structure and Petrology of the Boxelder Laccolith, Bearpaw Mountains, Montana " (*Bull. Geol. Soc. Amer.*, **52**, 1941, 817–54), describes the intrusion as a partly eroded laccolith of the Shonkin Sag type about 2 miles in diameter and 450 ft. thick. An upper layer of cliff-forming syenite 120 ft. thick grades downwards into a coarse friable shonkinite 175 ft. thick. Below this there is a cliff-forming shonkinite 40 ft. thick, and a friable " ribboned " shonkinite 50 ft. thick, in which the " ribbons " consist of syenite bands thin and closely spaced near the base, but becoming thicker and more widely spaced upwards. Qualitatively the rocks are much alike, the variations being caused by differences in the relative proportions of the minerals. Crystal settling in place accounts for the major differentiation. The syenite ribbons are ascribed to local segregation of residual liquid from the crystallising shonkinite.

From another intrusion, the Rocky Boy Stock, Bearpaw Mts., W. T. Pecora (*Amer. Min.*, **27**, 1942, 397–424) describes a mass of alkaline rocks with nepheline-syenite-pegmatites which bears a close resemblance to those of the Kola Peninsula. The stock occupies 12 square miles and consists of pyroxenites, shonkinites, syenites, nepheline-shonkinites, and nepheline-syenites. The pegmatites are characterised by containing strontium-, titanium-, and zirconium-bearing minerals.

J. B. Lyons has described the " Igneous Rocks of the Northern Big Belt Range, Montana " (*Bull. Geol. Soc. Amer.*, **55**, 1944, 445–72). The range is underlain by Cretaceous potash-rich trachybasalts which were extruded on to an eroded surface of Cretaceous sediments, and were folded with them into anticlines and synclines of north-western trend. The trachybasaltic breccias, agglomerates and flows, 3200 ft. thick, are intruded by many chonoliths, sills and dikes ranging from gabbro to quartz-monzonite. Some of the trachybasalts carry an abundance of phenocrysts of primary analcite. The evidence favours crystal settling as the cause of magmatic differentiation within the province.

A detailed investigation with 15 chemical analyses of the Palisade Diabase Sill of New Jersey by F. Walker (*Bull. Geol. Soc. Amer.*, **51**, 1940, 1059–106) has fully substantiated the citation of this intrusion as a typical case of gravitational differentiation. The main differentiation of the sill was effected by the settling of early-formed olivine crystals followed at a later stage by pyroxenes, resulting in the formation of an olivine-rich zone near the base of the sill. Pegmatite veins and schlieren, followed by strong hydrothermal activity, characterised the later stages of crystallisation. The final magmatic residues were highly sodic and gave rise to thin quartz-albite-pyroxene veins near the margins. This sequence of events seems to be typical of quartz-dolerite sills the world over, but the actual formation of an olivine-rich layer is exceptional.

In a paper on " Phase Petrology in the Cortlandt Complex, New York," S. J. Shand (*Bull. Geol. Soc. Amer.*, **53**, 1942, 409–28) combines Balk's study of the structure of the mass with his own and G. S. Rogers's petrographical work. The Cortlandt Complex consists of rocks ranging from peridotite and pyroxenite, through norite, to diorite. Balk showed that the complex was a funnel-shaped pluton, within which three smaller funnels are developed. Rogers's petrographical map does not correspond with Balk's structural map, and Shand finds the explanation of the discrepancy in the indefinite character and delimitation of Rogers's rock-species. To understand the complex then requires the abandonment of " species petrology " in favour of " phase petrology," or the mapping of critical mineral phases of which hornblende is the most promising. On this basis a satisfactory agreement between petrography and structure is established.

The " Anorthosite and Associated Rocks about Nain, Labrador," described by E. P. Wheeler, 2nd (*Journ. Geol.*, **50**, 1942, 611–42), include an anorthositic gabbro-diorite group with a younger diorite, an adamellite group with tonalite and monzonite ; dike rocks and pegmatites ; gneisses with garnet, cordierite and graphite indicating original sedimentary components. Small masses of ultrabasic composition are probably to be regarded as skarns. Banding and granulation in the igneous rocks are primary, and the anorthosite mass, at least in part, has a basement.

J. M. Harrison describes " Anorthosites in South-eastern Ontario, Canada " (*Bull. Geol. Soc. Amer.*, **55**, 1944, 1401–30), which are associated with gabbros and related types. The evidence indicates that the parent magma had a composition about gabbro-anorthosite. The more feldspathic masses are at a lower level in the crust than the more ferro-magnesian. It is suggested that the upward concentration of the dark minerals was accomplished by rising volatile-rich fluids which carried iron, titanium, phosphorus, etc., and by the assimilation of limy sediments. The igneous rocks are injected into the Grenville sediments and a granitic formation.

The paper on " Mafic and Ultramafic Rocks of the Baie Verde Area, Newfoundland," by K. de P. Watson (*Journ. Geol.*, **51**, 1943, 116–30), is a discussion of the field relations, petrography and chemical composition of a group of igneous rocks characteristic of geosynclinal belts ; *i.e.* greenstones (with pillow lavas), metagabbros, and ultrabasic rocks, including harzburgite and serpentine. Adjacent to a large mass of quartz-diorite the metamorphism of the greenstones has been heightened to that of the amphibolite facies. It is suggested that the rocks are Taconic (Ordovician) in age.

PLANT PHYSIOLOGY. By PROFESSOR WALTER STILES, Sc.D., F.R.S., The University, Birmingham.

RESPIRATION.—The writer has recently published a review of the advances in our knowledge of plant respiration during the decade 1935–45 (*Bot. Rev.*, **12**, 165–204, 1946). Since this was written a number of publications on this subject have been received. The writer had pointed out that there was a tendency for the term " anaerobic respiration " to be replaced by " fermentation," but that apart from the question of brevity there appeared to be little advantage in the change. W. Seifriz (" Anaerobic respiration," *Science*, **101**, 88–9, 1945) also expresses the view that no change in nomenclature of this kind is called for and argues for the broadening of the meaning of respiration so that it includes all biological energy-yielding reactions.

The case for the replacement of the term " anaerobic respiration " by " fermentation " has been stated by D. R. Goddard (" Anaerobic respiration or fermentation," *Science*, **101**, 352–3, 1945). The chief arguments against the use of the term anaerobic respiration are that this process can occur in the presence of oxygen and that it leads to a confusion of ideas since a degradation of a quantity of hexose in aerobic respiration will give three times as much carbon dioxide as that yielded by the breaking down of the same quantity of hexose in anaerobic respiration. To the present writer there seems to be little force in the second argument, the case against the continued use of anaerobic respiration resting entirely on the first argument.

The apple has formed a favourite subject for respiration studies. A simple apparatus for determining the rate of respiration and the respiratory quotient of apples has been described by S. M. Sykes ("A modified respirometer for studies on the respiratory quotient of apples," *Proc. Linn. Soc. N.S.W.*, **69**, 87–90, 1944). The principle of the apparatus is that of the Barcroft differential manometer. Two vessels, each of about 500 ml. capacity and consisting of two thick-walled hemispheres with flat ground flanges which can be quickly sealed together with petroleum jelly and as readily separated, are connected by a capillary glass tube as in the Fenn type of differential manometer. The apparatus is used in the same way as other types of differential manometer, except that soda-lime is used as the absorbent of carbon dioxide in the vessels when oxygen intake is measured. To determine the respiratory quotient successive measurements are made with the same apple, first with soda-lime present in the vessels and then without any absorbent. The two measurements provide data from which oxygen absorption, carbon dioxide evolution and hence the respiratory quotient can be calculated.

A number of papers dealing with the respiration of apples have been published by Miss F. M. V. Hackney ("Studies in the metabolism of apples, II. The respiratory metabolism of Granny Smith apples of commercial maturity after various periods of cool storage," *Proc. Linn. Soc. N.S.W.*, **68**, 33–47, 1943; "III. Preliminary investigations on the effects of an artificial coating on the respiratory metabolism of Granny Smith apples after various periods of cool storage," *ibid.*, **68**, 48–56, 1943; "IV. Further studies in the respiratory metabolism of Granny Smith apples, with special reference to the importance of oxygen supply," *ibid.*, **69**, 91–107, 1944; "V. The respiratory metabolism of Delicious apples of commercial maturity after various periods of cool storage," *ibid.*, **69**, 108–19, 1944). In the first of these papers measurements were made by means of the Pettenkofer method of Granny Smith apples kept in storage at 1° C. Determinations were also made of the concentrations of oxygen and carbon dioxide in the internal atmosphere of the apples, and from the respiration rates and internal gas concentrations the resistances of the skin to the passage of oxygen and carbon dioxide were calculated. Samples of apples were withdrawn from store after various times and the course of respiration at 18·3° or 21° C. followed for usually several weeks. With apples that had been in storage for periods up to 3 or 4 months the respiration rate fell steadily over the periods of observation at the higher temperatures. This fall in respiratory activity ran parallel with falling internal oxygen concentration and increasing resistance of the skin to gaseous diffusion, and it was concluded that the respiration of the fruit at this stage was controlled by the internal oxygen concentration. Later the respiration rates, although generally tending to fall, showed marked fluctuations. The internal oxygen concentrations also showed fluctuations, but the fluctuations in respiration rate did not appear to be directly related to these. Except in very old samples the resistance to diffusion of oxygen generally rose. In these later stages of storage the internal concentration of oxygen appeared to result from two factors, respiration rate and resistance of the skin to the diffusion of oxygen. Determination of the alcohol content of apples suggested that a small amount of anaerobic respiration occurs as a result of oxygen shortage in low concentrations of this gas.

Miss Hackney's second paper gives an account of similar work with apples coated with a 10 per cent. solution of castor oil and shellac in alcohol. This increases the resistance of the skin to the diffusion of carbon dioxide. The general effects of this treatment were found to be a lowering of the internal oxygen concentration, a temporary increase in the internal carbon dioxide concentration, and a lowering of the rate of respiration. As regards the last in samples removed early from store (after 3 to 16 weeks in storage) the respiration fell continuously for 6 to 8 days to an approximately constant rate. With apples that had been longer in store (for 20 to 31 weeks) the respiration rate rose very soon after the fruit was coated to a maximum and then fell continuously.

In the next paper measurements were made of the respiration rate and composition of the internal atmosphere of immature apples picked at various stages of development and on mature fruits kept at 0° C. and removed from store after various periods of storage. It was found that with immature fruits the respiration rate decreased as the age of the fruit at picking increased until about 6 weeks before the normal time of harvesting the fruit. Although the internal oxygen concentration decreased slightly and the internal carbon dioxide concentration rose slightly during development, the respiratory quotient remained approximately at unity.

The course of respiration at 18·3° C. of mature fruits taken from storage at 0° C. depended on the length of time the fruit had been in store. After storage for periods of less than 4 months the respiration rate generally rose to a maximum which was reached shortly after removal from storage and then fell continuously. This progressive decrease in respiration rate was associated with decreasing internal oxygen concentration and increasing resistance of the skin to the diffusion of oxygen. After a longer time in storage the respiration rate at 18·3° C. fell continuously from the time of removal from store. In these apples the internal oxygen concentration showed little change over the period when the respiration rate was measured, but the resistance of the skin to the diffusion of oxygen, high initially, generally increased.

A number of observations were made on the respiration of apples in pure oxygen. These showed that the respiration rate of immature fruits was about the same in oxygen as in air, but the respiration of mature fruits increased as a result of transference from air to pure oxygen, provided the fruit had not been in store for more than 4 months.

The general conclusion drawn from the work on respiration of Granny Smith apples is that throughout the greater part of the life in storage of these apples oxygen supply is the main factor limiting the rate of respiration.

The last of Miss Hackney's papers deals with similar observations on another variety of apple, Delicious. With these the parallelism between respiration rate and internal oxygen concentration was only sometimes, and not generally, observed. Frequently the respiration rate fell without change in the internal oxygen concentration, so that it would seem that a factor other than oxygen supply may limit the respiratory intensity of apples of this variety.

The effect of oxygen concentration on the respiration of another fruit, the avocado, has been examined by J. B. Biale ("Effect of oxygen concentration on respiration of the Fuerte Avocado fruit," *Amer. J. Bot.*, **33**, 363–73, 1946). The respiratory activity of the fruit was determined by passing a stream of gas of known composition free from carbon dioxide over the fruit and then absorbing the carbon dioxide evolved by the fruit with standard sodium hydroxide and titrating against standard hydrochloric acid. For the simultaneous determination of carbon dioxide evolved and oxygen absorbed a closed system was used, the actual procedure being a modification of that described by Haller and Rose 14 years ago.

Respiration at 15° C. was measured with oxygen-nitrogen mixtures varying in oxygen content from 0 and 2·5 per cent to 99·4 per cent. Experiments at 10°, 7·5° and 5° C. were also carried out with oxygen concentrations of 2·5, 5·0 and 10·0 per cent. and with air. At oxygen concentrations of 30 per cent. and over the course and magnitude of respiration did not appreciably differ from that in air, but when the oxygen concentration was lower than that of air both the course and intensity of respiration was affected, the climacteric peak being both delayed and lowered with decrease in oxygen concentration, while in absence of oxygen the climacteric rise did not occur and the rate of carbon dioxide evolution was very low. The course of oxygen absorption ran closely parallel with that of carbon dioxide evolution. With fruit in air there was, however, a slight tendency for the respiratory quotient (R.Q.) to increase to a maximum of 0·948 two days before the climacteric peak was reached, after which the R.Q. slowly fell. A similar change in the value of the R.Q. for the pre-climacteric period was observed with 10 per cent. oxygen, but with 5 per cent. oxygen the R.Q. fell during the pre-climacteric period from 0·922 on June 17 to 0·806 on June 22, followed by a rise to 0·960 on June 29 after the peak. With 2·5 per cent. oxygen there was a continuous rise from 0·784 on June 19 to 0·985 on July 1.

Temperature appeared to have a definite effect on the relation of respiration to oxygen concentration, the effect of the latter being less the lower the temperature.

A comparison of the rate of carbon dioxide evolution in air and in nitrogen by a number of different plant organs has been made by C. O. Appleman and R. G. Brown ("Relation of anaerobic to aerobic respiration in some storage organs with special reference to the Pasteur effect in higher plants," *Amer. J. Bot.*, **33**, 170–81, 1946). The material used included potato tubers, roots of turnip and parsnip and onion bulbs after short exposure to low temperature, potato tubers after storage for a long period at various temperatures and under different conditions of humidity, potato tubers and carrot and parsnip roots at low (1·5° to 3·5° C.) and high (22° to 28° C.) temperatures, maize and tomatoes at different stages of ripening and at low and high temperatures.

The carbon dioxide output in air and in nitrogen was measured on similar material in parallel experiments, a continuous flow of gas being used and the carbon dioxide evolved being absorbed in standard sodium hydroxide, which was then titrated against standard hydrochloric acid. The rate of output of carbon dioxide per hour in nitrogen to that in air (I/N) was then calculated. Where this ratio exceeded 1/3 it may be concluded that the Pasteur effect operated. The investigators found that in some organs the ratio I/N was close to 1/3; these organs included maize (green sweet corn) at 27° C., potatoes (var. Russet Burbank) and onion bulbs after removal from cold storage, and potatoes both after wounding and after treatment with ethyl bromide. With most organs the ratio was much higher, notably with two varieties of potato (Irish Cobbler and McCormick), and roots of carrot and parsnip. The result with carrot thus confirms the previous finding of Choudhury (*Proc. Roy. Soc.* B, **127,** 238–257, 1939).

A paper on the relation between respiration and cell division in plants has been published by A. V. Beatty ("Respiration and cell division in plants. I. Oxygen consumption and cell division in the leaves of *Ligustrum lucidum* and *Hedera helix*," *Amer. J. Bot.*, **33,** 145–8, 1946). To measure oxygen consumption a modified form of the Warburg manometer was used while values for the extent of cell division were obtained by determining the number of mitoses at hourly intervals between 6 p.m. and 8 a.m. in leaves of *Ligustrum lucidum* and between 6 p.m. and 6 a.m. in leaves of *Hedera helix*. Curves were plotted between mitotic frequency and time and between oxygen consumption per g. per hour and time for the same period. The investigator concludes that for both species the graphs showing oxygen consumption against time and mitotic frequency against time have common characteristics and that a correlation between the two processes is therefore indicated.

ENTOMOLOGY. By A. D. Lees, M.A., Ph.D., Agricultural Research Council, Unit of Insect Physiology, Cambridge.

The Control of Moulting and Metamorphosis in Insects.—It has now been known for many years that moulting and metamorphosis—two of the most dramatic events in the insect's life—are under the control of hormones which circulate freely in the blood. The source of the hormones concerned is in many cases unknown but in different insects the endocrine functions of three organs in particular have been proved. They are the corpora allata, small roundish bodies lying above the aorta and behind the brain; the corpora cardiaca, glandular organs closely associated with the corpora allata and innervated by nerves arising from the posterior region of the brain; and finally the brain itself.

Two theories have been proposed to account for the initiation of moulting and metamorphosis. On the one hand, two interacting hormones may be concerned: the one activating the adult potencies of the tissue and leading to growth and differentiation; the other a juvenile or inhibitory hormone which restrains development towards the adult state. On the other hand, the alternative theory seeks to show that moulting and metamorphosis are each controlled by a particular hormone. The first possibility has been shown to be correct in the blood-sucking bug *Rhodnius* (V. B. Wigglesworth, 1940, *J. exp. Biol.*, **17,** 201). Here the moulting hormone is secreted by the large fuchsinophil cells in the dorsal part of the brain and the inhibitory hormone by the unpaired corpus allatum. There are five nymphal stages in *Rhodnius*. The attainment of the adult state after the fifth moult is accompanied by far-reaching changes in the cuticle structure, external genitalia and wings. During the first four moults both moulting and inhibitory hormones are secreted, but at the fifth moult the corpus allatum ceases to secrete inhibitory hormone and the insect becomes adult. Even in the adult insect, however, the epidermis can still respond to the two hormones. Thus when the bug is transfused with the blood of two moulting fifth-stage nymphs it can be made to moult, again laying down cuticle of the adult type. What is more remarkable still, metamorphosis is partially reversible, for if the moulting adults are provided with inhibitory hormone by joining them with young moulting nymphs the less specialised of the imaginal structures, such as the general cuticle and bristles, become nymphal again.

The corpora allata have a similar function in other developing insects. Recently Pfeiffer (*Trans. Conn. Acad. Arts & Sci.*, 1945, **36**, 489) has shown in the grasshopper *Melanoplus* that when adult corpora allata are transplanted into fifth-stage nymphs, the insects undergo one or two supernumerary moults, instead of becoming adult after the sixth moult. After the first extra ecdysis some insects became adult but others remained nymphal in appearance, having short wings and greenish instead of yellow blood. It is interesting that the presence of additional quantities of inhibitory hormone from the implanted corpora allata should prolong the production of moulting hormone.

In the case of the wax moth *Galleria* also, an extra larval moult can be induced, and pupation prevented, by implanting corpora allata from young larvæ into nearly mature larvæ (H. Piepho, *Biol. Zbl.*, 1940, **60**, 367).

Among the higher Diptera the situation is complicated by the fact that corpora cardiaca and corpus allatum are fused into a single organ—Weismann's ring or the ring gland as it is now known. A developmental study in *Drosophila* has shown that the embryonic ring gland includes the following components: small dorsal cells derived from the ectoderm and representing the single median corpus allatum; large cells in the lateral part of the ring which arise from the dorsal wall of the stomodæum and represent the paired larval corpora cardiaca; and very small ventral cells closely associated with the hypocerebral ganglion. The latter are the imaginal cells of the corpora cardiaca (D. F. Poulson, *Trans. Conn. Acad. Arts & Sci.*, 1945, **36**, 449).

It has been known for some years that the presence of the ring gland is necessary for the formation of the puparium in higher

Diptera. Recently the ring gland has been shown to induce larval moulting and also differentiation of the imaginal buds. The control of moulting in *Drosophila* has been demonstrated by transplanting the heads of young larvæ deprived of the brain and ring gland into male flies together with the ring glands from mature larvæ (D. Bodenstein, *Biol. Bull.*, 1944, **86**, 113). The larval heads then undergo one or two moults, as can easily be judged by the presence of two or three pairs of mouth hooks. Metamorphosis at the level of single organs is illustrated by the course of development of the larval salivary glands of *Drosophila* which are made up, as are the other organs, of both larval and imaginal cells. At metamorphosis the larval cells are histolysed and the imaginal cells differentiate to give rise to the adult gland (D. Bodenstein, *Biol. Bull.*, 1943, **84**, 13). Larval salivary glands undergo metamorphosis if transplanted together with the ring glands from mature larvæ into the abdomen of male flies, while failing to do so in the absence of the ring gland. If the transplanted glands are from very young larvæ they respond to the hormone first by growth and only afterwards by differentiation. Bodenstein therefore views imaginal development as a balance between the effective level of the hormone and the competence of the tissue to react. In the absence of precise knowledge of the functions of the different parts of the ring gland, it is still undecided whether the differentiation of the imaginal discs is due to release from the influence of an inhibitory hormone or to activation by a particular hormone favouring differentiation.

As the last examples show, the corpus allatum of insects belonging to several different orders secretes a hormone which is inhibitory in nature. And this is true not only of insects with incomplete metamorphosis such as the stick insect *Dixippus*, *Rhodnius* and *Melanoplus*, but of insects with complete metamorphosis, such as certain Lepidoptera (*Galleria* and the silkworm *Bombyx*). There is less apparent uniformity as to the origin of the moulting hormone. In *Rhodnius* and probably in Lepidoptera it is secreted by the brain; in others again, like *Melanoplus*, the corpus cardiacum has a certain influence, for moulting is delayed but not prevented by the extirpation of these organs. Finally, in the higher Diptera the brain is without influence and moulting is controlled by the ring gland—perhaps by the cardiacum component entirely. Scharrer and Scharrer (*Biol. Bull.*, 1944, **87**, 242) point out that the dorsal part of the brain (pars cerebralis) and the corpora cardiaca may best be considered as a single system. The histological evidence in insects such as *Leucophæa* rests on the observation that the neurosecretory cells of the brain innervate the corpora cardiaca and secretory droplets of believed physiological significance can be traced along the nerve fibres into the organ, whose cells may also contain deeply staining colloid masses. A striking analogy can be drawn between this system and the association of brain and pituitary in the Vertebrata. In both cases neurosecretory cells in the brain innervate a composite endocrine organ ; and in both cases the non-neural component, the posterior lobe of the pituitary and the corpus allatum in insects, exerts a controlling influence on growth and ovarial function. The significance may lie in the close association of the brain and the "master glands."

THE CONTROL OF OVARIAL DEVELOPMENT.—The rôle of the corpora allata in controlling the growth and function of the ovary in the adult insect was first described in *Rhodnius*. In recent years a similar function has been revealed in other insects. After emergence the female *Melanoplus* lays down abundant fat in the fat body but the ovaries at first only develop to a stage when yolk deposition is about to begin (I. W. Pfeiffer, *J. exp. Zool.*, 1945, **99**, 183). The end of this phase is marked by a rapid depletion of the fat reserves and full maturation of the oocytes. If the corpora allata are removed at emergence this change does not occur : the fat body goes on building up while the ovaries remain starved. When, on the other hand, the grasshopper has the ovaries removed and the corpora allata left intact a very drastic depletion of the fat reserves take place at the normal time. Moreover, owing to the accumulation of unused precursory substances in the hæmolymph there are upsets in the water balance often resulting in a large increase in blood volume. Pfeiffer concludes that the corpora allata control the mobilisation of stored fat through the agency of a metabolic hormone. The ovaries in their turn may influence the corpora allata and prevent excessive depletion of the reserves.

In the dytiscid beetles—insects with a very long adult life—the corpora allata govern the course of the ovarian cycle (P. Joly, *Arch. Zool. exp. et gén.*, 1945, **84**, 49). Most species lay their eggs in the spring, the time of oviposition being inborn and little influenced by external variations in temperature and light. If ten pairs of corpora allata are implanted in female beetles in midwinter they are induced to ripen and lay their eggs out of season. If, on the other hand, the corpora allata are removed from the egg-laying beetles, regression of the ovaries soon follows. The corpora cardiaca are also implicated in some way, for in their absence the corpora allata atrophy. During that part of the year when maturation does not normally take place, the oocytes grow to a stage when they are ready to receive yolk but then they regress and are resorbed. The function of the corpus allatum hormone is to carry the growth of the oocytes past this critical stage, after which they become refractory to regression.

The development of the ovaries in the higher Diptera is also under the control of the corpus allatum. In the blowfly *Calliphora* the corpus allatum is small and flat when the adult emerges but becomes greatly enlarged as the ovaries ripen (E. Thomsen, *Dansk Naturhist. Forening*, 1942, **106**, 320). After extirpation of the corpus allatum maturation of the ovaries does not take place. The male corpus allatum is as effective as the female gland in promoting ovarial growth but, as in other insects, the hormone is not necessary for the production of ripe sperm. In addition, there is a reciprocal influence of the ovary on the corpus allatum, for in the absence of the ovaries the gland hypertrophies. Whether the ovaries secrete a hormone controlling the growth of the corpus allatum or whether the increased size is due to the storage of hormone is not fully understood.

In several insects the maturation of the ovaries can be stimulated by hormone from the corpora allata of a different species. Thomsen has shown, for example, that the ovaries of *Calliphora* will mature if transplanted into a blowfly of the genus *Lucilia* and *vice versa*. On the other hand, Vogt (*Biol. Zbl.*, 1941, **61**, 242) has described several cases of hormone specificity among species of *Drosophila*. For example, larval ovaries of *D. funebris* transplanted into *D. virilis* hosts fail to develop beyond the stage when yolk is about to be deposited in the oocytes; but when two *funebris* ring glands are implanted as well, full maturation takes place.

THE BREAKDOWN OF HÆMOGLOBIN AND CHLOROPHYLL BY BLOOD-SUCKING AND PLANT-FEEDING INSECTS.—The fate of most ingested food substances in the body of the insect are difficult to follow with any precision but, in the particular case of insects feeding on blood or on green vegetation, spectroscopic examination of the distribution of the pigments in the tissues may yield a wealth of detailed information. Such a survey has recently been made in a number of blood-sucking arthropods (V. B. Wigglesworth, *Proc. Roy. Soc. B*, 1943, **131**, 313). In the blood-sucking bug *Rhodnius* most of the hæmoglobin ingested is broken down in the gut, the globin fraction being absorbed as food and the black residue of hæmatin excreted. A small part of the protein is not digested, however, but is absorbed and circulated in the blood as kathæmoglobin. Some of this pigment, in a slightly changed form, is taken up by the salivary glands which assume a bright cherry-red colour.

The same pigment is also transferred to the eggs which become salmon pink; as the bug develops this is concentrated in the stomach and after hatching is digested to yield globin and hæmatin, just as in the parent insect. Even before it has had an opportunity to feed, therefore, the young bug gives the appearance of having taken a small blood meal. The hæmolymph pigment is also taken up by the pericardial nephrocytes and by the epithelial cells of the gut wall. Here it undergoes further changes and is degraded to biliverdin and other pigments. Biliverdin accumulates in the nephrocytes, which become bright green, or is excreted into the gut lumen. While this breakdown is taking place the cells become laden with a deposit of free iron.

In other blood-sucking insects the great bulk of the hæmoglobin is broken down in the gut in a similar way to give hæmatin. But other details of the hæmoglobin katabolism show almost endless variations, even in closely related species. In mosquitoes and the flea no blood pigment is absorbed from the gut. In the bed-bug *Cimex* the absorbed pigment is degraded no further than protohæmatin. Only in the louse *Pediculus* and in the reduviid bugs are bile pigments formed.

The fate of chlorophyll, a magnesium containing porphyrin, presents some similar features to the breakdown of hæmoglobin after ingestion by insects. It has been studied in the squash bug *Anasa tristis* (R. L. Metcalf, *Ann. Ent. Soc. Amer.*, 1945, **38**, 397). In this species chlorophyll is broken down in the gut and free magnesium is present both in the gut lumen and in the circulating hæmolymph. The testes, salivary glands and epidermis are reddish in colour owing to the presence of ruby red crystals of what are probably pheophorbides. The presence of these bodies therefore denotes that the chlorophyll molecule has been deprived of its magnesium and phytil groups. The fat body again is apple green, the colour being due to a chlorophyll derivative of the bile pigment type. No pigment is transferred to the eggs. But unlike hæmoglobin, the ingested chlorophyll can have no value as food for the adult, or for the developing insect.

NOTES

Miscellanea

The honours list published on the occasion of H.M. the King's birthday included the following awards: *Baron:* Sir William Beveridge. *K.C.B.:* Mr. C. S. Wright, director of scientific research, Royal Naval Scientific Service. *K.C.M.G.:* Prof. P. H. Buck, professor of anthropology, Yale University. *K.B.E.:* Dr. J. C. Fryer, secretary of the Agricultural Research Council; Prof. J. E. Lennard-Jones, Plummer professor of theoretical chemistry in the University of Cambridge. *Knights:* Prof. C. Burt, professor of psychology, University College, London; Mr. H. Clay, warden of Nuffield College; Major-General G. Covell, director of the Malaria Institute of India, Delhi; Dr. D. L. Keir, vice-chancellor of the Queen's University, Belfast; Prof. K. S. Krishnan, professor of physics in the University of Allahabad; Mr. P. R. Morris, vice-chancellor of the University of Bristol; Dr. C. C. Paterson, director of the Research Laboratories, General Electric Co., Ltd.; Mr. H. L. Saunders, comptroller-general of the Patent Office. *C.M.G.:* Mr. G. F. March, director of agriculture and forests, Sudan Government; Mr. N. A. Middlemas, director of surveys, land officer and commissioner of mines, Uganda; Colonel S. C. Saward, director of surveys, Gold Coast. *C.I.E.:* Mr. J. F. Russell, chief engineer for irrigation, Bengal; Mr. T. M. Coffey, chief conservator of forests, Bengal; Mr. H. Crookshank, superintending geologist, Geological Survey of India; Lieut.-Colonel D. R. Crone, superintendent of the Survey of India; Mr. H. F. Mooney, chief forest adviser, Eastern States, India. *C.B.E.:* Dr. R. B. Bourdillon, director of the Electro-Medical Research Unit (Medical Research Council), Stoke Mandeville Hospital; Mr. W. A. Damon, chief inspector of alkali works, Ministry of Health; Prof. P. I. Dee, professor of natural philosophy in the University of Glasgow; Prof. W. E. Garner, Leverhulme professor of physical chemistry in the University of Bristol; Mr. J. McD. R. Greig, director of Moredun Institute, Animal Diseases Research Association; Mr. H. Grinsted, director of technical research and development, Ministry of Aircraft Production; Mr. C. E. Horton, chief scientist, Admiralty Signal Establishment; Dr. W. B. Lewis, for services as superintendent, Telecommunications Research Establishment, Ministry of Aircraft Production; Prof. R. P. Linstead, director of the Chemical Research Laboratory, Department of Scientific and Industrial Research; Dr. A. Parker, director of fuel research, Department of Scientific and Industrial Research; Prof. E. S. Pearson, professor of statistics at University College, London; Dr. H. Schofield, principal of Loughborough College; Prof. L. Dudley Stamp, professor of geography at the London School of Economics and Political Science; Prof. W. N. Thomas, professor of engineering at University College, Cardiff; Prof. R. Whiddington, Cavendish professor of physics in the University of Leeds.

The Royal Society has elected the following new foreign members: Dr. H. S. Gasser, director of the Rockefeller Institute for Medical Research, New York; Prof. F. Joliot, head of the National Centre of Scientific Research, Paris; Prof. T. von Karmân, professor of aeronautics and director of the Guggenheim Aeronautics Laboratory at the California Institute of Technology, Pasadena; Prof. E. A. Stensiö, professor of palæontology in the University of Uppsala and director of the Riks Museum, Stockholm.

Prof. S. Chapman, Sedleian professor of natural philosophy in the University of Oxford, has been elected a foreign associate of the U.S. National Academy of Sciences.

Sir Henry Tizard, president of Magdalen College, Oxford, has been awarded the Gold Medal of the Franklin Institute and elected to honorary membership of the Institute.

The Holweck Prize, founded by the Physical Society as a memorial to Dr. Fernand Holweck and other French physicists who suffered death or privation under the German occupation, has been presented to Prof. Charles Sadron of the Institute of Physics, University of Strasbourg, himself a survivor of the Buchenwald concentration camp. The Société Française de Physique has founded a Holweck Medal, to be presented with the Prize, which will be awarded annually for ten years alternately to a French and British physicist for distinguished work in experimental physics.

H.M. the King has approved the award of the Founder's Medal of the Royal Geographical Society for 1945 to Dr. C. Camsell, Deputy Minister of Mines and Resources, Canada, and for 1946 to Brigadier E. A. Glennie, director of the Survey of India; also the Patron's Medal for 1946 to Sub-Inspector H. A. Larsen, of the Royal Canadian Mounted Police, for his achievement of the North-West Passage in the *St. Roch* from west to east in 1940–42, and from east to west in one season in 1944. The Victoria Medal of the Society for 1946 has been awarded to Prof. H. J. Fleure.

Other medals awarded during the past quarter include the Albert Gold Medal of the Royal Society of Arts to Sir Alexander Fleming and Sir Howard Florey, for their services in the discovery and development of penicillin; the Gold Medal of the Institution of Mining and Metallurgy to Mr. Carl Davis, past-president, director and London agent of the Anglo-American Corporation of South Africa, Ltd.; and the Godman-Salvin Gold Medal of the British Ornithologists' Union to Mr. P. R. Lowe, O.B.E.

Brigadier O. H. Wansbrough-Jones, fellow of Trinity Hall, Cambridge, has been appointed scientific adviser to the Army Council in succession to Sir Charles Ellis.

Mr. J. S. L. Gilmour has been appointed director of the Royal Horticultural Society's gardens at Wisley.

Dr. Charles Wilcocks has been appointed director of the Bureau of Hygiene and Tropical Diseases.

The following University appointments have been announced during the quarter: Dr. G. R. de Beer, professor of embryology at University College, London; Dr. J. Černy, professor of egyptology at University College, London; Prof. H. Dingle, professor of history and philosophy of science at University College, London; Prof. R. W. Ditchburn, professor of physics in the University of Reading; Prof. W. J. Duncan, professor of aerodynamics at the College of Aeronautics; Dr. R. H. Evans, professor of civil engineering in the University of Leeds; Mr. E. E. Evans-Pritchard, professor of social anthropology in the University of Oxford; Mr. T. R. C. Fox, professor of chemical engineering in the University of Cambridge; Dr. E. A. Guggenheim, professor of chemistry in the University of Reading; Prof. D. R. Hartree, Plummer professor of mathematical physics in the University of Cambridge; Prof. W. Jackson, professor of electrical engineering at the Imperial College of Science and Technology; Mr. R. L. Lickley, professor of aircraft design at the College of Aeronautics; Prof. J. T. Randall, Wheatstone professor of physics at King's College, London; Dr. A. D. Ross, professor of civil engineering at King's College, London; Prof. P. C. Sarbadhikari, Ghose professor of botany at the University College of Science, Calcutta; Dr. O. A. Saunders, professor of mechanical engineering at the Imperial College of Science and Technology; Dr. Gwyn Williams, professor of chemistry at Royal Holloway College; Dr. C. L. Wilson, professor of chemistry at Notre Dame University, Indiana.

The following Presidents have been elected for the session 1946–47 : British Association for the Advancement of Science, Sir Henry Dale, F.R.S. (from Jan. 1, 1947) ; Chemical Society, Prof. C. N. Hinshelwood, F.R.S. ; Institute of Physics, Prof. A. M. Tyndall, F.R.S. ; Institution of Electrical Engineers, Mr. V. Z. de Ferranti ; Linnean Society, Prof. G. R. de Beer ; Physical Society, Prof. D. Brunt, F.R.S. ; Royal Microscopical Society, Dr. J. A. Murray, F.R.S. ; Royal Society of South Africa, Prof. R. S. Adamson.

We have noted with great regret the announcements of the death of the following scientific workers during the past quarter : Academician Alexander Baikov, metallurgist ; Mr. J. L. Baird, a pioneer of television ; Prof. B. H. Bentley, emeritus professor of botany in the University of Sheffield ; Mr. E. G. Boulenger, formerly director of the Aquarium of the Zoological Society of London ; Prof. F. Broili, professor of palæontology and historical geology in the University of Munich ; Prof. T. H. Bryce, F.R.S., emeritus professor of anatomy in the University of Glasgow ; Dr. H. A. Colwell, known for his work on X-ray therapy and related subjects ; Dr. H. G. Earle, Director of the Henry Lester Institute of Medical Research and Preventive Medicine, Shanghai ; Dr. S. Flexner, For.Mem.R.S., emeritus director of the Rockefeller Institute for Medical Research, New York ; Mr. J. A. Gardner, a founder of the Biochemical Society ; Mr. A. L. Howard, author of *A Manual of Timbers of the World* ; Sir George Julius, chairman of the Council for Scientific and Industrial Research, Australia ; Prof. T. H. Laby, F.R.S., formerly professor of natural philosophy in the University of Melbourne ; Sir Charles Marston, archæologist ; Mr. H. B. Maufe, formerly director of the Geological Survey of Southern Rhodesia ; Prof. J. Park, emeritus professor of mining and mining geology in the University of Otago ; Prof. W. Peddie, formerly Harris professor of physics at University College, Dundee ; Dr. E. B. R. Prideaux, formerly reader in inorganic and physical chemistry at University College, Nottingham ; Dr. A. W. Rogers, F.R.S., formerly director of the Geological Survey of South Africa ; Dr. H. W. Starkweather, research chemist ; Prof. J. W. W. Stephens, F.R.S., emeritus professor of tropical medicine in the University of Liverpool ; Mr. C. A. Watts, founder of C. A. Watts & Co., Ltd. ; Dr. H. E. Wood, formerly Union astronomer and director of the Union Observatory, Johannesburg.

The Bureau, Council and various Committees of the International Union of Chemistry, whose activities have been revived after their suspension during the second world war, met in London on July 24–7,

1946. The meetings, under the chairmanship of Prof. M. T. Bogert, who has been President of the Union since the 13th Conference in Rome in 1938, were held in the rooms of the Royal Society, Sir Robert Robinson, President of the Society, also being a Vice-President of the Union. 19 of the 23 nations belonging to the Union were represented, namely, Argentine, Belgium, Brazil, Bulgaria, Canada, Czechoslovakia, Denmark, France, Great Britain, Holland, Italy, Jugoslavia, Norway, Poland, Roumania, Sweden, Switzerland, the United States, and the U.S.S.R. Representatives from India and Iran were also present. Sir Robert Robinson welcomed the delegates, and Prof. Bogert in his reply described the present position of the Union. The Bureau is comprised of the president, the retiring president, Prof. Biilmann (Denmark), 8 vice-presidents, Prof. Bougault (France), Prof. Dony-Henault (Belgium), Prof. Nesnemyanov (U.S.S.R.), Sir Robert Robinson (Great Britain), Prof. Swain (U.S.A.), Prof. The. Svedberg (Sweden), Prof. Swietoslawski (Poland), Prof. Tomitch (Jugoslavia), and the general secretary, Prof. R. Delaby (Paris). Dr. L. H. Lampitt (London) is president of the Finance Committee.

The majority of the 15 Committees forming part of the Union were able to meet and discuss plans for future research. The Bureau and Council held several meetings, at which it was decided, amongst other important matters, to hold the 14th Conference from July 21 to 24, 1947, during the 11th International Congress of Pure and Applied Chemistry, which will take place in London from July 18 to 25, 1947, following the centenary celebrations of the Chemical Society (July 16–18). Three new international committees were formed, namely, Macromolecular Chemistry, Toxicology, and Standardisation of Laboratory Materials. A Committee for the standardisation of the purity of chemical products will also be constituted; this was created in 1938, but has not yet been able to function. There were important debates on the problems of chemical encyclopædias, the publication of which has been held up in certain countries, and of documentation. The Union is desirous that missing periodicals should be available as soon as possible in all its member countries, and that "publications of an encyclopædic character should in future be published by Academies or Bodies directly dependent on Governments, and not by publishing houses without any connection with these Governments."

During their stay the delegates were entertained by the Chemical Society and the Society of Chemical Industry, and the reunion proved an auspicious prelude to the bigger conference of 1947.

The Association of British Chemical Manufacturers has published a Memorandum on Patent Law Reform, prepared by a Committee representative of the chief chemical societies and associations in the country (pp. 117, price 3*s*., post free). The Memorandum is in two parts, the first of which was submitted to the Board of Trade Patents Committee in 1944. It consists of considered answers to a number of leading questions, *e.g.* " Are British Patents used to the detriment of the public interest, for forming cartels or for retarding industrial development ? How can such abuses be checked ? " The second and larger part was submitted to the same Committee in June last. In it there is developed a new criterion of subject-matter on the basis that every invention which is patentable must be a novel industrial application of a new discovery. Every inventor who seeks a patent must show that he has contributed some item of new knowledge to the art, and that his invention (which must be a " manner of new manufacture ") is based on that new knowledge. It includes an outline scheme for Empire patents, while recognising the difficulties of any arrangement such as is described.

The Royal Meteorological Society has commenced publication of a monthly journal (*Weather*. Price 1*s*. 6*d*., from 49 Cromwell Road, London, S.W.7) intended more especially for readers interested in meteorology as a hobby or because it is of importance in their ordinary work. The first number, which appeared in May last, contained, in addition to other matter, an illustrated article by J. Paton on the Aurora Borealis, another on Meteorology and Weather Lore by Professor Brunt and an account of the British Radiosonde by A. J. Lander.

Professor Brunt considers the reasons for, or the correctness of, familiar sayings relating to the redness of the sky at sunset and sunrise, the mackerel sky, the good visibility which frequently precedes rain, the stimulating effect of the north-west wind and the evil effects of a green winter. A red sky at sunset may be an indication that fine weather is coming from the west—the direction from which our weather usually comes—for it implies that the air in that direction is dry, the dust or haze particles being free of condensed water and thus small enough to scatter red light. Most phenomena referred to in familiar lore have a rational basis and serve as a useful indication of weather conditions likely to be experienced in the succeeding twelve hours. It seems, however, that there is no justification for the belief that a mild January is likely to be followed by a winterish spring and a cold May. Mild winters do, however, usually show alternations of very mild and very cold weather—changes which tend to produce a " fat churchyard."

A radiosonde is a miniature weather station carried into the upper atmosphere by a small rubber balloon. The Kew radiosonde gives information concerning the pressure, temperature and humidity of the air by audio-frequency modulation of an 11·1 metre carrier wave emitted by a small transmitter carried by the balloon. The frequency of modulation (from 700 to 1000 cycles per sec.) is determined by one of three variable inductances, each controlled by the physical condition it is designed to record. Each variable inductance consists of a coil wound on a U-shaped iron core provided with a movable "keeper" (or armature) whose distance from the ends of the core is controlled (*a*) by an aneroid capsule, (*b*) by a bimetallic coil, (*c*) by a strip of goldbeater's skin sensitive to humidity. A little windmill operates a switch which connects each inductance in turn to the circuit which modulates the carrier wave and leaves it connected for five or six seconds, during which time the observer on the ground determines the frequency with the help of a calibrated audio-frequency oscillator and a cathode-ray oscillograph. The meteorological state is deduced from the frequency of the note emitted by the radiosonde. The whole apparatus with its batteries weighs only about 3 lb. It is carried upwards until the balloon bursts and then falls at a safe speed controlled by a parachute. Heights exceeding 60,000 ft. may be attained after a flight of, say, an hour. Many radiosondes are lost but a goodly proportion are recovered (a reward of 5*s*. is given to a finder) and used again after overhaul.

The *G.E.C. Journal* for February 1946 is devoted to a description of the war-time technical work of the Company written by the Editor, H. W. Richardson. Although only bare outlines of the work are given, the account with its eighty photographs occupies 53 pages and covers a quite amazing range of articles, from a tiny medical inspection lamp which could be wrapped up in a postage stamp to aircraft carrier lifts. The output was enormous including, for example, 1,000 million carbons, 100 million batteries, 750,000 small electrical machines and 100,000 telephone and radio sets. The contribution made by the Company to the Radar programme was of great importance. Working in conjunction with the Telecommunications Research Establishment of M.A.P. the Company produced the first aircraft interception system in the 10-cm. wave band. Known as Radar A.I. Mark VII, and with a peak radiation power of 5 kW. it was fitted to the Beaufighter aircraft which played such an important part in the defeat of night raids over this country and formed the mainstay of the night defence of Malta. Gun ranging equipment working in the 25-cm. and 10-cm. bands was used for long-range gun-laying in the battle of Matapan and, in conjunction with Bofors guns, for defence against flying bombs. Radio frequency cables were made with the low-loss thermoplastic polythene as dielectric and very low-loss cables using air as dielectric and spaced polystyrene supporters to hold the wire core concentric with its sheath. The Company designed and manufactured easily portable telephone switchboards capable of dealing with as many as 360 lines for Army use. Much work was done on valves of various kinds, some involving grid-cathode spacing as small as 0·002 in. Among the magnetrons developed for pulse generation were the CV76, operating in the 10-cm. band and giving a peak power of 500 kW. with an efficiency of 55 per cent., and the CV214, working in the 3-cm. band with a peak power of 45 kW. and an efficiency of 30 per cent.

The Company was responsible for most of the machinery used in the A.F.D. XXXV floating dock, which is capable of lifting a 50,000-ton vessel in four hours, the pumps delivering 30,000 tons of sea-water per hour against a head of 30 ft. Standard degaussing outfits for ships were made to Admiralty specifications and cable equipment for mine-sweepers operating against magnetic mines. Steel tank pumpless mercury arc rectifiers were produced in complete units in sizes up to 200 kW. Land-mine detectors, airfield lighting control, range-finding equipment for Oerlikon anti-aircraft guns, capable of use within four minutes of arrival at a site, and even mountings for these guns were manufactured by the Company.

The well-known 5-ft. fluorescent lamp was perhaps the most important single contribution by the lamp industry to the war effort, since it created pleasant lighting conditions in underground factories and other places blacked-out day and night. Since the war ended a 4-ft. lamp of 1·5 in. diameter has been designed. It takes 40 watts and will be available in two colours, daylight and warm white. The former gives rather less blue-green and less red (relative to green and yellow) than noon daylight: the latter, described as very suitable for recreational lighting, rather less blue-green and less red than the gas-filled tungsten filament lamp. A detailed description of these lamps, written by H. G. Jenkins, will be found in the issue of the *Journal* for August 1945. Among searchlights developed was one fitted to night-fighter aircraft, intended for short-period use during actual attack on an enemy plane. It took 140 kW. and 1200 amp. supplied by lead accumulators, and was provided with a para-elliptic mirror giving the necessary spread of light from a relatively small hole in the nose of the aircraft. A ground searchlight, giving a wide spread beam, was designed to consume 600 kW. and 4000 amp. The positive carbon was 64 mm. in diameter and the negative a rotating carbon disc. With a 10-ft. mirror the intensity was measured in thousands of megacandles. Much work was done on electric furnaces, radiant heating (*e.g.* for paint drying), high-frequency heating and many types of measuring apparatus. Work on primary cells resulted in the design of a successful layer type dry battery, which gives 70 per cent. more output per unit volume than the old cylindrical type and has an excellent shelf life. Such batteries are specially useful in portable transmitter-receiver radio sets (the Walkie-Talkie) and in deaf-aid equipment using valve amplifiers.

The *Bell Laboratories Record* for May 1946 contains a description of radio lenses, consisting of an array of metal plates, which focus very short radio waves as ordinary lenses focus light waves. The lenses depend for their action on the well-known fact that short electromagnetic waves travel with increased velocity when they pass between metal plates so that, by suitable arrangement of the length, spacing and contour of the plates, focusing of waves of a selected wave-length is achieved. These waves are directed on to the lens by a horn-shaped wave guide (or at the receiving end are directed by the lens into a similar guide) and it is stated that the spread of a " parallel " beam obtained with a lens 20 ft. across is less than that given by a searchlight mirror with visible radiations. The May number of the *Record* contains also an account of some of the uses of magnesium and its alloys. The alloys of this metal used in industry have only about two-thirds the weight of equivalent aluminium alloys and for equal weights are four times as strong as steel. Unfortunately they corrode very easily and suitable precautions must always be taken to prevent such corrosion. The solid metal does not ignite till raised to a temperature above its melting point, small particles such as turnings and filings burn more easily and are a source of danger, just as coal dust, aluminium powder and flour are, unless reasonable care is taken. The alloys draw and form very easily if worked at temperatures between 450° F. and 650° F.

The June number contains a well-illustrated account of the multi-cavity magnetron. A model of this device was sent to the U.S.A. from England in October 1940 and from this prototype 75 different designs have been developed, giving waves whose length varies from 45 cm. to less than 3 cm. and peak powers from 10 kW. to more than 1000 kW. The July issue contains a somewhat condensed account of the ammonium dihydrogen phosphate (ADP) crystal which is being used in place of Rochelle salt and quartz for piezoelectric purposes. These crystals are used to convert electrical energy (applied in the form of an alternating potential difference) into mechanical energy. The electro-mechanical coupling is the ratio of the mechanical energy stored to the total electrical energy applied. For ADP crystals at ordinary temperatures this coefficient is 0·3 and thus larger than that of any other known crystal. The value rises to 0·4 at – 125° C. when the crystal shatters, probably as a result of the rotation of the ammonium group. ADP crystals give a linear response, show no hysteresis effects and are stable up to 100° C. Potassium dihydrogen phosphate (KDP) forms crystals isomorphous with those of the ammonium salt, but their electromechanical coupling coefficient is only 0·1 at ordinary temperatures. This figure rises to 0·9 at – 151° C. when ferroelectric coupling appears. This happens when the positive and negative charges on the dipoles align themselves along one of the axes of the crystal, so resembling in behaviour the elementary magnets of a ferromagnetic substance. A small applied field can then cause considerable mechanical distortion, but the response is not proportional to the applied field owing to hysteresis effects. Rochelle salt shows a similar effect between – 18° C. and 24° C.

The July number of the *Record* contains also a description of the Western Electric Beachmaster announcing system. This consists of a 250-watt loud-speaker, an amplifier, a 1500-watt alternator driven by a petrol engine and accessories all packed in six water-tight floatable cases. This set was used to control the movement of the invading forces at the Iwo Jima beachhead and at other beachheads in the Pacific. It amplifies the speaker's voice ten million times, so that instructions spoken into the amplifier microphone can be heard above the din of planes, tanks, bulldozers and gunfire.

The *Bulletin* of the Imperial Institute, Vol. XLIV, No. 1 (Jan.–March 1946), contains an account of the fish liver industry in South Africa, based on information supplied by the South African Bureau of Information. A survey of the vitamin A content of liver oils obtained from fish caught in local waters was first made by an unnamed research chemist at Cape Town in 1936 working on his own initiative. Methods of processing were evolved at the same time and by 1938 the scientific production of vitamin oils was firmly established on a commercial basis. At the present time the industry is supplying a quarter of the vitamin A used in this country for the fortification of margarine. The oil is extracted mainly from the liver of the valhaai, a shark which abounds in the sea off Cape Town. One company owns a fleet of "sharkers," others rely on trawlers for their raw material, one, at Walvis Bay, buying as much as 500,000 lb. of fish offal each year. The fish landed in South Africa have, as a general rule, large oily livers. In most species of shark the liver constitutes 10 per cent. of the total weight of the fish, it may be as great as 35 per cent., the oil content ranging from 25 to 80 per cent. The oils are rich in vitamin A; the poorest—that of the dog fish—yielding 4000–6000 international units per gm., the stock-fish 10,000–12,000 and the stonebass from 100,000 to 600,000 units. Cod-liver oil gives only 1000 units per gm. and halibut 50,000. Some oils provide also vitamin D. It is remarkable that the total yield of vitamin A from the South African fisheries far exceeds that given by the much larger Newfoundland fisheries. The oil itself is used in soap, paint and linoleum manufacture, in waterproof fabrics and in the leather industry.

Errata.—Dr. K. Hoselitz, Director of Research, Permanent Magnet Association, has kindly drawn attention to two errors which appeared in the abstract of his paper in the *Journal of Scientific Instruments* (April 1946) printed in our last number. The percentage composition of Alnico V is aluminium 8, nickel 14, cobalt 24 and copper 3 and not aluminium 8, chromium 14, tungsten 24, molybdenum 3 as given on p. 623 line 15; while the composition of Alcomax II differs from that of Alnico V in that it contains 11 per cent. nickel and 6 per cent. copper. Dr. Hoselitz also points out that the minimum dimensions quoted for bar magnets (length 0·08 in. and cross-section 0·01 sq. in.) are inter-related "due to technical problems" and not independent as the wording might suggest.

ESSAY REVIEW

THE FIRST MODERN PHYSIOLOGY By CHARLES SINGER, D.Sc., D.Litt., M.D., Emeritus Professor of History of Medicine in the University of London. Being a Review of **The Endeavour of Jean Fernel, with a List of the Editions of his Writings.** By SIR CHARLES SCOTT SHERRINGTON, O.M. [Pp. x + 223, with 27 figures, including 7 plates.] (Cambridge: at the University Press, 1946. 16*s*. net.)

THE doyen of modern physiologists has applied much literary research and philosophic acumen to an investigation of the first work of modern times which can be said to be devoted to physiology. It is also the first in which the word "physiology" is used in our modern sense. Sir Charles Sherrington's book contains a clear exposition of the life-work of one who may be regarded as his first modern predecessor and is in itself a bibliographical model. Every physiologist and every historian of science will rejoice at this new demonstration of Sir Charles' great literary and bibliographic powers.

Jean Fernel (1485–1558), perhaps the most learned and considered physician of his time, was the author of a standard work on medicine which remained current for more than a century. He was born near Paris, where the whole of his life was passed, and lived the externally uneventful life of a successful practitioner. He was the ablest and most thoughtful representative of medical humanism, which held the field until medical discipline was determined in another direction by the scientific method associated with the names of Vesalius and Harvey. Sir Charles Sherrington has already used Fernel as the type of pre-Harveian medical thinker in his great philosophical work *Man on his Nature.* In this new work he devotes himself to an intensive study of the physiological and philosophical work of the same man. In the secular debate on the nature of the living body Fernel is important for the clarity and completeness with which he presents the final version of the Galenic synthesis. We may think of Fernel as a reincarnation of Galen, equipped with a lucidity, a concise orderliness, and a philosophic insight of which the Greek physician was incapable.

The physiological work of Fernel appeared first in the *De naturali* *parte medicinæ* of 1542, almost exactly contemporary with the *Fabrica* of Vesalius (1514–64). Neither Vesalius nor Fernel shows any deep discomfort with the Galenic scheme which however was, more or less unconsciously, modified by both. Yet Fernel shows no serious doubt of the security of Galenic physiology, while the more youthful Vesalius displays no trace of unease until his second edition of 1555. That date may therefore perhaps be chosen as the birth year of modern physiology.

Fernel was peculiarly well situated for producing his synthesis. He came to the University of Paris when the new classical scholarship had just become fully established there. The tradition entered the medical sphere through Jean Ruel (1479–1537), who occupied an influential position in the Parisian faculty and was very learned in Greek medical texts. Ruel, however, did not interest himself in Galen. The first real Galenic ripple at Paris was in 1514, when the scholar-printer Henri Estienne (died 1520) issued a small collection of texts of Galen translated by the venerable Nicolo Leoniceno of Ferrara (1428–1524), then in his eighty-sixth year. The tide rapidly gained strength. Galenic texts were soon pouring from the presses of Paris. The rate reached about two a month by 1528. By the middle thirties, when Fernel began seriously to interest himself in the physiological outlook of Galen, all the works that bore on the subject were available in handy Latin versions, mostly rendered by Guenther of Andernach. Fernel was probably attending the anatomical lectures of Sylvius and Guenther at Paris at the same time (1533–36) as Charles Estienne and Vesalius, but his outlook was very different from theirs. His natural bent towards philosophy and abstract thinking gave him a dislike for the type of illustration in which Estienne and Vesalius rejoiced. They were extroverts and Fernel very much of a contemplative introvert.

Fernel's treatise on physiology (1542) introduced into Latin literature the word *physiologia* as applied specifically to the physics of the animal body, though in his first edition he still writes the word in Greek letters. In older Latin and Greek writings the term is applied to natural knowledge in general or, rather more narrowly, to something near what we now call *physiography*. Fernel's *physiologia* is thus the physiography of the human body. But though he constructed by far the clearest account of the old physiology, he showed an opposition, now almost unintelligible, to the use of anatomical figures or physiological diagrams. He did not visualise the operations of the body, as we all seek now to do, but thought in terms of the interaction of the three spirits, natural, vital and animal, of the mingling of the four humours blood, yellow bile (choler), black bile (melancholy), and phlegm (pituita) and of the resulting " temperaments " or constitutions. Mind he treated as coming within the realm of physiology because, for him, all the workings of the body were, at source, non-material and incorporeal. All were traceable to the operations of an immaterial soul. Mental events were just one series among many of the activities of that soul. In all animate things, and most in man, the body has been created for the sake of the soul. It is made for that soul and it is formed not only as a habitation but as an adjusted and adjustable instrument for use by its inherent powers.

This conception separates Fernel in some degree from the Stoic conception of Galen, who held that the human soul was essentially part of the world-soul, but far more does it cut him off from the Christian conception of the body as simple brute matter made to live and act by the soul within. This is a crux that Fernel does not fairly face. The extreme confusion of philosophy in the mid-sixteenth century was not conducive to the clear presentation of the issue. Few, if any, of his century perceived the inevitable conclusions to be drawn from this or from any comparable view. Perhaps only recently, and with the philosophic work of Sir Charles himself, has it become apparent that even the modern physiological synthesis is, in the philosophic sense, unintelligible, and must remain so without (or perhaps even with) a far greater knowledge of the integration of physiological and psychological processes than we yet possess.

REVIEWS

PHYSICS

Atomic Energy for Military Purposes. By Henry Dell Smyth. [Pp. viii + 308, with 8 plates.] (Princeton: Princeton University Press; London: Oxford University Press, 1946. 12*s.* 6*d.* net.)

This book is essentially a reprint of the Official Report on the Development of the Atomic Bomb published under the auspices of the United States Government. A few minor alterations and clarifications have been made in the original text, eight pages of photographs have been added, and additional matter, released since the original publication, is included in appendices. For the sake of completeness the not particularly illuminating statements issued by the British and Canadian Governments are also included. The book therefore contains all the information available to the public up to the present date.

Dr. Smyth has given a singularly lucid, objective and restrained account of the most stupendous scientific project ever undertaken. His story covers, step by step, the scientific researches and developments, the administrative background, and the subsequent engineering developments up to the final manufacture of the bomb. Though written under "security" conditions, the report is remarkable for the amount of precise information it discloses. The atomic expert would obviously wish for more numerical data; the chemical engineer would be interested in detailed plans of the manufacturing plant. The general scientific reader could hardly seek for more than is contained in these pages. He certainly could not expect a more lucid, well-ordered and readable account.

The book is not intended to be a "popular" version of the subject. Although an introductory chapter on radioactivity and atomic science includes all the specialised knowledge required to follow the text, a grounding in physics and chemistry is assumed in the reader. Scientists and engineers, whatever their own speciality, should have no difficulty in following Dr. Smyth's very clear account of a very portentous achievement. For the sake of humanity, they should certainly make the attempt. "A weapon has been developed that is potentially destructive beyond the wildest nightmares of the imagination," writes the author in one of the very rare paragraphs in which he allows emotion to colour the deliberate objectivity of his story. "This weapon has been created not by the devilish inspiration of some warped genius, but by the arduous labours of thousands of normal men and women working for the safety of their country." "Here is a new tool for mankind, a tool of unimaginable destructive power. Its development raises many questions that must be answered in the near future. . . . This is one reason for the release of this report. It is a semi-technical report which it is hoped men of science in this country can use to help their fellow-citizens in reaching wise decisions."

J. A. Crowther.

Atomic Artillery and the Atomic Bomb. By J. K. Robertson, F.R.S.C. [Pp. xii + 173, with 30 figures, including 8 plates.] (New York: D. Van Nostrand Co., Inc.; London: Macmillan & Co., Ltd., 1945. 12*s*. 6*d*. net.)

This book is a revised and enlarged edition of *Atomic Artillery* by the same author, reviewed in these columns in 1938 (Vol. XXXIII, p. 361). The main additions are two chapters on Nuclear Fission and the Atomic Bomb, a few paragraphs on the separation of isotopes, an extension to the section on cosmic rays, and a note on Bethe's theory of the origin of solar radiation. There are a few additional photographs.

In its present form, the book is a non-mathematical survey for the educated layman of the progress of nuclear physics during the present century, leading up to the atomic bomb. There are a few loose statements and misprints, but they are trivial blemishes in a readable and balanced account. It should be popular in School and Public Libraries, and can be commended to those who desire an introduction to the Smyth Report on Atomic Energy.

Professor Robertson is head of the Physics Department of Queen's University, Kingston, Canada.

F. A. V.

Aircraft Engines. Vol. I. By A. W. Judge, A.R.C.S., D.I.C., A.M.Inst. Aut.E. Second edition. [Pp. xii + 492, with 308 figures.] (London: Chapman & Hall, Ltd., 1945. 28*s*. net.)

Modern aero-engine developments are so extensive that to cover the subject adequately in a single volume has become impossible. An author is therefore faced with choosing between a fundamentally scientific and a more or less descriptive approach, depending on the type of reader to whom his book is addressed.

Mr. Judge's book seeks a compromise by adopting a semi-descriptive method but making copious use of the literature—indeed his bibliography is one of the fullest in a book of this type. In this way he is able to cover a very large field, and his book will doubtless be of value for reference purposes. The reviewer would, however, hesitate to recommend it to the serious student.

The author's presentation of a considerable mass of experimental work is uncritical and adds nothing to the value of the references quoted. This is perhaps just as well, since where he seeks to elucidate he frequently succeeds in misleading.

The book contains many mis-statements, *e.g.*:—

1. "A petrol engine . . . operating upon a mixture of 14·1 parts of air to 1 part of petrol, by volume" (p. 23). This corresponds to a fuel/air ratio about 640 times chemically correct.

2. On p. 25, there is a sketchy account of dissociation and it is implied that over 1000° C. the CO_2 and H_2O are completely dissociated into CO, O and H. Actually, the degree of dissociation is unimportant below about 1800° C. and probably negligible at 1000° C.

3. On p. 43 it is stated that the recording drum of a Farnboro' indicator is driven by a "crank having the same motion and phase as the engine crank". It is of course driven off the engine crankshaft and provides a time or crank-angle base because it rotates at uniform speed.

4. On p. 148, Fig. 87 shows a consumption loop. The line labelled

"Correct mixture line" is meaningless. There can be no such line on a consumption loop, since "correct mixture" occurs at only one point on the curve.

These examples are typical of a general looseness of expression and a superficial approach which seriously detracts from the value of the mass of material presented.
O. A. S.

A Primer of the Internal Combustion Engine. By H. E. WIMPERIS, C.B., C.B.E., M.A. New edition, revised and enlarged. [Pp. xii + 141, with 60 figures.] (London: Constable & Co., Ltd., 1944. 5*s.* net.)

AT few periods in the history of the internal combustion engine has development progressed more rapidly than during the last decade. Even to the expert it has been difficult enough to keep some semblance of contact with all the various aspects of the subject. To the junior student the subject is overwhelming, unless he has access to a suitable primer in which the development is built up on sound fundamentals. The problem before the author of such a text is therefore heavy. Is he to prune out all the old material to make room for the new? An excellent balance between the old and the new has been struck in the present work.

In most technical schools the introduction to the internal combustion engine is made through a test on a gas engine. This machine is employed on account of the ease of operation and as it is available in small units running at such speeds that a simple indicator can be employed to examine the changes of pressure which occur throughout the cycle. Realising this, the author has discussed many of the researches made during the development of the gas engine. This forms an ideal background for studying the theory of the newer forms of engine.

After a short introductory chapter on the history of the engine, including the gas turbine, the author continues with chapters on the theory of heat, the behaviour of gases and vapours, including an introduction to indicators and their diagrams. The following chapters deal with "ideal" and "real" engines, fuels and gas producers, and finally engine details and tests. At the end of each section there is a well-chosen set of examples with answers.

Possibly criticism may be raised at the inclusion of a paragraph on volumetric heats in preference to the more usual molecular specific heats, while, in discussing the discrepancy between the actual and ideal temperatures resulting from tests on explosions in closed vessels, the author makes no mention of the effect of the turbulence of the charge or of slow combustion, termed "afterburning" by Dugald Clerk. Apart from these points, there is no doubt that Mr. Wimperis and his collaborator, Mr. Stern, are to be congratulated on the production of a primer of real value, broad in outlook, as would be expected from engineers of their experience of practice and teaching.
B. LLOYD-EVANS.

METEOROLOGY

Les climats et l'organisme humain. Par ÉMILE DUHOT. No. 171 of series "Que sais-je?" [Pp. 127.] (Paris: Presses Universitaires de France, 1945.)

THIS little book gives, first a brief account of the climatic elements and of the geographical factors which control them, secondly a discussion of the pathological effects of these elements, and thirdly a brief account of meteorotherapy and climatotherapy, or the beneficent effects of exposure to various meteorological and climatic conditions.

It is on the whole a satisfactory account of the subjects enumerated, taking into consideration the limitations of size of the book. The chapter dealing with the pathological effects of the meteorological elements contains rather a large number of technical medical terms, which will not be familiar to the reader untrained in medicine, but the final chapter on the beneficent effects of exposure to various meteorological conditions can be readily followed by any intelligent reader. In the latter the author discusses in some detail the effects of exposure to the air, to direct action of the wind and to solar radiation. He contrasts the effects of living at the seaside, in a plain, or on high ground of varying altitudes, and for each type of locality gives a catalogue of suitable resorts in France. Finally, special consideration is devoted to the treatment of pulmonary tuberculosis, extra-pulmonary tuberculosis and infantile weaknesses, and brief notes on a variety of other diseases are added.

The book under review contains a considerable amount of information concerning the incidence of various diseases, and some useful general hints, as for example concerning the care with which the body should be exposed to the sun's direct rays. It will be helpful to many general readers, and might perhaps have been still more helpful to the serious student if a short carefully selected bibliography had been added.

D. B.

CHEMISTRY

General and Inorganic Chemistry for University Students. By J. R. Partington, M.B.E., D.Sc. [Pp. xxiii + 916, with 345 figures.] (London: Macmillan & Co., Ltd., 1946. 36*s*.)

The appearance of a new and comprehensive text-book by Prof. Partington will be welcomed by the many readers of his earlier works. The present book is somewhat more advanced than the familiar *Text-book of Inorganic Chemistry for University Students,* and it represents a remarkably successful attempt to provide, in a single volume, material useful to students of chemistry at all but the very elementary stages of their training. The book is divided into two parts; the first deals with the more important aspects of General and Physical Chemistry, and the second, which is more than twice as large, is a systematic treatment of the descriptive Inorganic Chemistry of the elements, arranged according to the shorter Periodic Table. The first part, mainly intended for students of University Scholarship standard, is necessarily less advanced than the second, although the chapters on atomic theory, valency, and the solid state provide adequately for most Degree students. The second part contains all the Inorganic Chemistry normally required by Pass Degree candidates; Honours candidates, besides obtaining from it a sound background knowledge of the subject, will find it a useful basis for their more specialised studies, for numerous references are given to up-to-date reviews and important original memoirs.

The concisely informative style of the book is not unexpected in view of the large bulk of information contained in it; as the author himself states, it is intended for students who are keenly interested in chemistry. For such the book provides, without trimmings, a clear exposition of fundamental chemistry, based on carefully sifted original information. Experimental aspects of the subject are fully emphasised, and the industrial processes described are selected with due regard to their importance at the present time. An historical approach to theoretical topics has been adopted where it facilitates the logical development of fundamental principles. Some of the more difficult subjects, such as the theory of valency, are treated with skilful avoidance of ill-defined ideas.

This book can be recommended strongly to any student proceeding to a Degree in chemistry, and it should find its place in the reference library of every school, polytechnic, and college in which chemistry is seriously taught.

A. J. E. WELCH.

Le Charbon : son Histoire, son Destin. By EUGÈNE SCHNEIDER, Membre de l'Institut. [Pp. 348, with 15 plates.] (Paris : Librairie Plon, 1945. Frs. 160.–.)

THE bituminous coal mined in north-east France has been, in large measure, responsible for the expansion of French industry during the last century. The author of this book belonged to a family, the Schneiders of Creusot, well known for the great part it played in these developments : he completed the book only shortly before his death. The first part of the book, which covers more than half the text, is perhaps the most valuable : it is a well documented and scholarly study of the early beginnings of the utilisation and mining of coal and of the growth of the French coal industry during the Middle Ages and down to the end of the eighteenth century. Besides the technical aspects of the subject, social and legal matters are also discussed : an account is included of the lives of three men, Emmanuel de Croy, François de la Chaise and Phillippe le Bon, who played important parts in these early days. The second part is a relatively brief sketch of the life and work of the mining communities during these times. The third and final part, which covers some 80 pages, discusses somewhat cursorily the constitution of coal, modern theories of its formation, the carbon cycle in nature, chemical industries based on coal and possible new developments. A number of excellent illustrations, particularly of old mining methods and machinery, are included. The book contains much of interest not only for the mining engineer, and technicians and scientists connected with the coal and gas industries, but also for the general reader. The book is essentially French in outlook : no similar text on the British coal industry is available.

H. L. RILEY.

Colloids : Their Properties and Applications. By A. G. WARD, M.A. [Pp. viii + 133, with 6 plates and 28 figures.] (London : Blackie & Son, Ltd., 1945. 5s. net.)

THIS book gives an excellent introduction to a branch of science which is developing very rapidly both in its fundamentals and in its applications to various industries. The nature of the colloidal state is carefully explained, particular attention being paid to the methods of determining particle size. The only important omission is the very recent method of Debye for determining molecular weights from measurements on light scattering. There follows a review of various colloidal systems and the factors controlling their stability. The remainder of the book (amounting to nearly one-half) is devoted to the more important applications of colloid science to the study of rubber (natural and synthetic), cellulose and its derivatives, proteins, viruses, clays, paints, lacquers and detergents. Obviously in a book of this size which covers such an enormous field, the author can do little more than mention many of the phenomena, but Mr. Ward has shown considerable discrimination in giving a little more detail here and there, just where it is needed. He has also provided a useful bibliography.

G. B. B. M. S.

Aqueous Solution and the Phase Diagram. By FREDERICK FIELD PURDON and VICTOR WALLACE SLATER, B.Sc., F.R.I.C., M.I.Chem.E. [Pp. iv + 167, with 47 figures.] (London: Edward Arnold & Co., 1946. 24*s*. net.)

THIS book is designed to serve as a laboratory and desk manual for the chemist who has to solve the often complicated problems of purification and crystallisation on the industrial scale. This function it should fulfil admirably, and it can also be recommended to any student of the Phase Rule as a valuable supplement to the more theoretical textbooks. The methods of constructing and utilising diagrams for multi-component systems are clearly described, and the authors draw on a long experience in selecting those of most value.

An introductory chapter describes the Phase Rule and the terminology of the subject. Thereafter the treatment is restricted to systems consisting of aqueous salt solutions in equilibrium with pure salts (one example involving solid solutions is given). Starting from the plotting of a simple solubility curve the authors describe typical two- and three-component diagrams. For each a complete table of the data is given so that the reader can easily reconstruct the diagram for himself, as he is advised to do. Then follows perhaps the most valuable section of the book, dealing with four component systems. An example here will illustrate the quantitative nature of the treatment; potassium nitrate is to be prepared from a mixture of sodium nitrate and potassium chloride. The data for 25° and 75° C. are given, and used to construct a Jänecke projection by means of which the recrystallisation cycle is planned. An elevation showing the water content of the relevant mixtures is now constructed, and the calculation from this of the quantities of water that must be added or removed at various stages of the cycle is explained; finally, working details of the whole cycle on a weight basis are tabulated.

In the last thirty pages of the book, quinary systems are given a similar treatment, two or three typical systems being discussed on quantitative lines.

Much thought and work have clearly gone into the making of this book, and both authors and publishers deserve congratulations on the result. The diagrams are clear and well labelled, and the volume is stoutly bound, opens flat at any page, and makes an attractive companion.

C. W. D.

Introduction to Emulsions. By GEORGE M. SUTHEIM. [Pp. vi + 260, with frontispiece, 22 figures and 25 tables.] (Brooklyn: Chemical Publishing Co., Inc., 1946. $4.75.)

THIS book consists of a series of lectures given at the Brooklyn Polytechnic Institute, "by a practical man for practical men." It is admirable; although the style is often colloquial and sometimes breezy, the basic scientific principles are very clearly and correctly explained, points of both physical and organic chemistry, and also the mechanics of emulsifying machines, being presented with the minimum of demands on the previous knowledge of the reader, and the information appears to be thoroughly up-to-date. There is a short introductory account of surface tension and surface films, and of the functions of soluble and solid emulsifying agents, chapters on the chemistry of emulsifying agents, on the methods of making emulsions, the properties, especially the stability of emulsions, and an extremely interesting final chapter on the principal uses of emulsions. There is a glossary of technical terms for the convenience of the less academically-minded readers, and a very useful appendix with the trade names and formulæ of a great many emulsifying agents on the market.

The subject is very carefully and logically developed, and the author has taken untold pains to simplify the exposition without loss of scientific accuracy; the only questionable statement noticed by the reviewer was the statement that surface tension *suddenly* drops to zero at the critical point. The book may be heartily recommended to scientific as well as to "practical" people.

N. K. A.

Surface Active Agents. By C. B. F. Young and K. W. Coons. [Pp. x + 381, with 41 figures.] (Brooklyn: Chemical Publishing Co., Inc., 1945. $6.00.)

The most useful part of this book is probably the large number of recipes for wetting agents, adhesives, soldering fluxes, special cutting and soluble oils, etc. An attempt is made to present the elementary theory of surface tension, which occupies some 85 pages, but its value is much diminished by the number of errors and approximations introduced, and by the amount of repetition. The scientific accuracy may be gauged from the following points noted, among others: the formula for the ring method for determining surface tension is given without any correcting factors; a stalagmometer is figured with rounded instead of sharp edges to the tip; the bubble pressure method is shown in a diagram with a quite wide tube, but the formula given is for infinitely narrow tubes; Adam is surprisingly quoted as saying that the molecules in condensed films are "never vertical"; on p. 27 a column containing five almost non-polar and one rather polar group is headed "hydrophilic," presumably a misprint for hydrophobic which would be nearly correct for the five non-polar groups. Contact angles and empirical wetting times are lumped together as "surface tension or modification of this interesting effect"; there are even a number of spelling mistakes.

Much of the book consists of fairly long, and rather uncritical, abstracts of papers, scarcely knit together at all. Nevertheless, though as a coherent treatise this book is disappointing, readers are likely to find a good deal of useful, if rather miscellaneous, information in it.

N. K. A.

Metallic Corrosion, Passivity and Protection. By Ulick R. Evans, M.A., Sc.D. With an Appendix by A. B. Winterbottom, M.Sc.Tech. Second edition. [Pp. xxxiv + 863, with 144 figures.] (London: Edward Arnold & Co., 1946. 50*s.* net.)

It is nine years since the first edition of Dr. Evans's book appeared, and at that time it quickly established itself as the fullest presentation of the subject. It has now been out of print for several years, and the growth of an already voluminous literature has been so great that a very thorough revision has had to be undertaken. The magnitude of the task may be gathered from the fact that the author index contains nearly 2000 names. It must be said at once that the work of revision has been exceedingly well done. With so many references, little seems to have been missed. In spite of the mass of detail which might have made the text forbidding it is very readable. The old controversies over corrosion have died down, and rival views are fairly discussed. Work carried out in the author's laboratory naturally bulks largely in the presentation, with emphasis on the statistical method of experiment. The order of arrangement, although improved in detail, remains essentially the same as in the first edition. This involves a certain degree of artificial separation of cognate matters. Thus on consulting the section on fretting corrosion the treatment of an important subject appeared to be scanty, but a search under other headings revealed a much more complete review in an earlier section on the part played by friction and oxidation in corrosion.

On making trial searches on subjects chosen at random the information was usually found, conveniently recorded, but an exception was the attack of loaded petrol on steel exhaust valves, although the action of such fuels on the light alloys is noticed. The reviewer would suggest that more use might be made of microscopical investigations. Corrosion is most often a topochemical process, related to the distribution of grain boundaries or of distinct phases within a metal otherwise homgeneous, and a study of the process under the microscope, requiring a good deal of patience, is of value in elucidating the mechanism of corrosion. Such studies would usefully supplement the excellent investigations of the author and his school using electrochemical methods. The application of those studies to industrial problems is well discussed. Methods of protection, including such relatively new processes as phosphate coating and improved methods of painting, electrodeposition, and coating with plastic varnishes, receive full attention. An interesting feature is the mathematical analysis at the close of many of the sections. Mr. Winterbottom's appendix, dealing with the optical study of surface films on metals, is based mainly on the work of Tronstad and his school.

The work will appeal strongly both to the practical user of metals and to the laboratory metallurgist as the clearest and most authoritative account of the whole subject. A word must be said about the publishers' share in the production. Although considerably enlarged, the use of a thinner paper makes the book less bulky and more attractive than the earlier edition and also allows it to remain open flat at any page; a feature which will be appreciated by all who have to handle large volumes.

C. H. Desch.

A Dictionary of Metallography. By R. T. Rolfe, F.R.I.C. [Pp. viii + 243.] (London: Chapman & Hall, Ltd., 1945. 15*s*. net.)

In view of its title this is a curious book. In the preface the author appears to accept the definition of Metallography as "that branch of science which relates to the constitution and structure, and their relation to the properties of metals and alloys," and distinguishes it from "Chemical Metallurgy, comprising reduction and refining." Yet he deals with constitution in seven words, and takes nine lines for refining and heat refining. The longest articles deal with Electronic Structure (about 60 lines), the Periodic Table (36 lines in addition to the Table, which occupies a whole page), and X-ray analysis (67 lines). While some reference to all these is certainly desirable in a book on Metallography, the amount of space given to them seems disproportionate to the space occupied by the four longest articles on purely metallographic subjects, which are Ac and Ar points (36 lines), Pearlite (24 lines), S-curves (23 lines) and δ iron (22 lines). Thermal analysis is dismissed in 11 lines, microstructure in 7, and the equilibrium diagram in the same number. Yet the Bessemer process is given 34 lines.

It may seem ungenerous to complain of being given much more than would be expected from the title in a dictionary, where one does not have to read any of the articles in which one is not interested, in order to find the desired information, but it must be admitted that the impression conveyed to the reviewer is one of a want of balance. Much more serious is the inaccuracy in a few of the definitions. For example, a eutectic is defined as the alloy with the lowest melting point in a series. This is not necessarily true, nor is the alloy with the lowest melting point necessarily an eutectic one (*cf.* Au-Cu alloys); the melting point of magnesium is given as 651°, whereas 649° has been the accepted value for several years; and the statement that this metal is "workable only by a special technique" is likely to give a completely false impression.

These mistakes, however, are relatively few, and in general it may be said that the dictionary should be a useful addition to the metallographist's bookshelf, and were a later edition subjected to severe pruning and correction in some parts, and to expansion in others, it would be an invaluable one.

John L. Haughton.

An Introduction to Textile Bleaching. By J. T. Marsh, M.Sc., F.R.I.C., F.T.I. [Pp. xiii + 512, with 154 figures, including 32 plates.] (London: Chapman & Hall, Ltd., 1946. 32*s*. net.)

During the last twenty-five years advance in our knowledge of the composition and structure of textile fibres and of the chemistry and physics of old-established textile processes has been spectacular, and has resulted not only in the adoption by progressive industrial concerns of comprehensive systems of quality control, but also in the development of many completely new processes. Very few English text-books on textile subjects, however, include this new material, which is scattered about in a large number of original papers and patent specifications. The volume under review makes an important contribution towards remedying this state of affairs, for in it is collected together a great deal of information on one aspect of textile chemistry and technology. The scope of the book is less limited than its title suggests, for it deals with many topics including the composition and properties of the more important textile fibres, wetting and detergency, the scouring and bleaching of cotton, linen, and rayon, the scouring of raw wool, wool yarn, and wool pieces, carbonising, crabbing, silk degumming, wool and silk bleaching, drying of textiles, and the assessment of damage in textile materials. The subject matter is well presented and is illustrated by an adequate number of photographs and line drawings.

A knowledge of chemistry is desirable in order to appreciate the volume fully, as it is written from the point of view of the research chemist rather than from that of the practical man, but it will be welcomed by students, technologists, and research workers alike, for it provides a thorough and authoritative account of research work and of the principles of present-day practice, and is extremely well supplied with references to original papers and patent specifications—a feature which is lacking in most books on textile subjects.

C. S. Whewell.

GEOLOGY

Bibliography and Index of Geology exclusive of North America.

Vol. 9, 1941–42. By J. M. Nickles, Marie Siegrist and Eleanor Tatge. [Pp. xix + 450.] (Geological Society of America, 1943.)

This volume, the ninth that has been issued since the inception of a co-operative arrangement between the Geological Society of London and the Geological Society of America, is published through the munificence of the Penrose Bequest administered by the Geological Society of America. The corresponding bibliographies and indexes of North American geology are issued periodically as Bulletins of the United States Geological Survey, of which the authors or editors above-noted are librarians. These lists combined must be pretty well exhaustive of the geology of the world. The serials that have been examined (and listed) are world-wide in their provenance, and in all languages. The present series of bibliographies are especially valuable for the inclusion of great masses of Russian and other literature in unfamiliar tongues which would otherwise be overlooked. Each entry consists of full bibliographical details, and also a most valuable short summary of its contents. The index, of 109 pages in double columns, is admirably done. In short, these bibliographies form one of the most valuable aids to research that geologists have at their disposal. G. W. T.

BOTANY

An Introduction to Industrial Mycology.

By George Smith, M.Sc., F.R.I.C. Third edition. [Pp. xiv + 271, with 143 figures.] (London: Edward Arnold & Co., 1946. 20*s.* net.)

Moulds are used extensively in industry at the present day and the trend would indicate that this tendency will increase. Whether their appearance is interpreted as "controlled fermentation" or "spoilage," control will be necessary, and this happy state of affairs is more likely to ensue if the biologist (or chemist) in charge has a practical knowledge of moulds and their uses in industry. However, they must be of the right kind, as I well remember the author lecturing on this subject, and many members of the audience discovered to their chagrin that the moulds concerned could not be used for coating aeroplane parts.

This book is outstanding in being a very practical guide to anyone engaged in controlled fermentations, or on spoilage of foods or fabrics by moulds. It is not always possible for the person on whom this responsibility is placed to have had a special training in Mycology. This is quite understandable as the spoilage may not warrant the employment of a full-time microbiologist and chemists are only too ready to investigate new problems. The numerous and excellent photomicrographs which this book contains renders species identification a reasonable problem and should appeal particularly to such people.

Most books on moulds are too academic for the industrial biologist, and rarely devote more than a few paragraphs to the "Fungi Imperfecti"; yet this is one of the most important groups of the fungi from an economic viewpoint, and its members appear with alarming regularity on foods and textiles. They spoil fermentations whether they be carried out in flasks holding 200 mls. or in tanks holding several thousand gallons. This subject is very adequately dealt with in four chapters, and the treatment of the Aspergilli and the Penicillia deserves special mention.

Laboratory technique and the physiological behaviour of moulds receive adequate attention and this part of the book will be most helpful to the chemist who has turned part biologist.

The production of Penicillin and other fine chemicals by fermentation will undoubtedly expand enormously and this third edition has appeared in time to fill a gap in the shelves of published industrial microbiology.

R. D. ANDREWS.

Pure Cultures of Algæ: Their Preparation and Maintenance. By E. G. PRINGSHEIM. [Pp. xii + 119, with 1 plate and 8 figures.] (Cambridge: at the University Press, 1946. 7*s.* 6*d.* net.)

THE fresh-water algologist has always been handicapped by the difficulty of obtaining a sufficiency of material. A few algæ are always with us, in quantity sufficient for our needs, but the appearance of the majority is fleeting and, seemingly, erratic and all too often investigations have been curtailed owing to difficulty in obtaining suitable supplies over a period. Attempts have been made in the past to remedy this deficiency, often at the cost of immense labour, which, it must be admitted, has scarcely been commensurate with the results obtained.

Prof. Pringsheim's work in this field seems to have met with considerable success, for he has developed methods by which numerous algæ and flagellates can be cultured. This success would appear to be due largely to two reasons—he has worked on the assumption that, save for special purposes, bacteria-free cultures are not necessary, and further, he has exploited what might be termed "rough-and-ready" methods, culturing in soil or peat extracts, beef extracts and so on. Such work should open up the field to more exact methods.

The book describes, often in minute detail, the technique for establishing and maintaining algal cultures. A method is described of washing algal cells with the aid of a micropipette, enabling flagellates of no more than 12μ long to be handled. Despite the author's assurance that it is not difficult to acquire the necessary skill in preparing and maintaining the cultures, one may well wonder if such techniques do not call for long and patient practice.

F. W. J.

ZOOLOGY

D.D.T. The Synthetic Insecticide. By T. F. WEST, M.Sc., Ph.D. (Lond.), F.R.I.C., and G. A. CAMPBELL, M.Sc. (Leeds), F.R.I.C. [Pp. xii + 301, with 13 plates.] (London: Chapman & Hall, Ltd., 1946. 21*s.* net.)

THE value of D.D.T. in medical entomology was established under war conditions when the standards of personal hygiene, previously the main defence against insect-borne diseases, were shattered. What use can be made, under peace conditions, of this odourless and colourless compound with its remarkable properties of specific toxicity to insects and of stability when exposed in thin films to sunlight or to sweat? This question can be answered only from practical trials in the many fields in which the insecticide has potential uses; not only in the conquest of diseases spread among man, domestic animals and crop plants by insects, but in the control of noxious insects in the pantry, the granary, the milking shed, the glasshouse and outdoors on horticultural and agricultural crops and in the forest. The authors have facilitated this exploratory work by assembling, from a widely-scattered literature, the results reported up to the autumn of 1945. They have also included a detailed account of work, not before known to the reviewer, on the incorporation of D.D.T. in distempers and paints by which means a simple control of domestic insect pests may be possible. Because the successful practical use of D.D.T. requires a thorough knowledge of the chemical and physical properties of the compound, a full account is given together with methods by which the insecticide can be formulated to suitable sprays, dusts or mists.

The book will therefore meet the needs of a wide range of readers from the amateur, anxious to know whether this material has met the high expectations roused by the exuberant publicity which its discovery received in the popular press, to the applied entomologist faced with the problem of controlling a particular insect pest. As the latter worker will require to know how D.D.T. compares with other insecticides for a particular purpose, the authors have wisely included the results of comparative trials, and have so freed the book from the bias which might be suggested by the use of the definite article in its title.

H. M.

Inventaire des Oiseaux de France. By Noël Mayaud. [Pp. viii + 211.] (Paris: André Blot.)

The artistic world owes much to the creative imagination of the French people, but this national attribute can hardly be described as one of the qualifications of a statistical scientist! Indeed, during the past century French ornithology has suffered much from the lively imagination of the provincial naturalist, with the result that most of the local avifaunas—and the country is rich in such documents—have, to a large extent, been vitiated by the inclusion of dubious or fanciful records. For instance, Maisonneuve has admitted that essentially British bird, the Scotch Grouse, in his catalogue of species, occurring in the department of Maine-et-Loire! We do not question its presence in that part of France, but suggest that an enterprising gourmet probably took a keen interest in its journey there.

From this mass of unreliable literature M. Noël Mayaud must have often found it difficult to discriminate between the "grain and the chaff" when compiling his *Inventaire des Oiseaux de France*. Very wisely he has relegated the more incredible records to an appendix entitled "Captures Contestables ou Contestées." This list contains a formidable total of over a hundred birds. While we are in entire agreement with this arrangement we cannot help feeling that the number of suspects might have been made even larger by the transference, and consequent addition, of many of the records he has accepted without demur. For example the appearance of that purely oceanic and tropical species, the Frigate Bird, on the Loire is rather difficult to believe. Surely the capture of the Baikal Teal in France is most likely due to the accidental escape of individuals from captivity? Nor does the admission of the century-old reports of the Semipalmated Snipe seem altogether reasonable. On the other hand, a few of the "contested" records are, in the reviewer's opinion, sufficiently reliable to have been included in the main body of the work. Eversmann's Warbler is a case in point. This bird was recorded by the writer in 1913 and the specimen, shot at Manonville, Meurthe-et-Moselle, in May of that year, is still in his collection, and available for inspection by anyone wishing to see it. There can be no doubt as to its identity or of its provenance.

With these and other amendments, M. Mayaud's little book should prove a useful aid to all students of French ornithology. Lacking any reference to external distribution, or to the habits of the birds, it can only be regarded as a basis for further research, as, indeed, is clearly indicated by its unpretentious title. COLLINGWOOD INGRAM.

La Vie des Sauterelles. By L. CHOPARD. [Pp. 204, with 18 plates and 15 figures.) (Paris: Librairie Gallimard, 1945. Frs. 150.-.)

As a subject for the natural historian the Orthoptera are well-nigh unrivalled. The extraordinary diversity in bodily form, their structural adaptations and varied habits, all lend themselves to a sympathetic but non-technical treatment. In the present volume, the sixth in this library of natural history, these and many other aspects of their biology are admirably conveyed.

After a brief introduction to the families of the Orthoptera a purely systematic approach is dispensed with. There follows a consideration of shape *in relation to environment*: the elongated forms of the Phasmids and certain Acridiidæ living amongst dry vegetation; the curious depressed bodies of certain desert grasshoppers; the modifications of cave-dwelling species; the cryptic resemblances to the background, to bark, to leaves and even to flowers. There is an attractive chapter on the song of grasshoppers which is developed particularly in relation to the French fauna. The mechanism of stridulation and the significance of different types of song are discussed in the light of modern work, but the text is sufficiently leisured to recall the appreciation of the song by the ancient Chinese and to describe the curious industry which grew up in that country around the tending and training of fighting crickets. A more detailed consideration of insect hearing would have been a profitable addition to this chapter.

Development is treated in outline only, but under the heading of self-preservation is gathered much general biological information on the haunts of the Orthoptera and on their means of defence, including entomological curiosities such as reflex bleeding, reflex immobilisation, and the complementary acts of autotomy and regeneration. The final chapters are devoted to aspects of reproduction and to the relations of the Orthoptera to man. While domestic pests and locust invaders can only be accounted an unmitigated nuisance, it is pleasing to note that on the credit side of the balance sheet the author sets the advancement of our ideas through the use of many of these insects as laboratory animals.

Written in a lively prose style, this book follows the best traditions of French natural history. The author has tempered enthusiasm with restraint and has succeeded in the difficult task of writing a book of interest to the specialist and general reader alike. The 15 text figures and 18 plates illustrating objects such as Mantid oothecæ, as well as a variety of insects, are of excellent quality.

A. D. LEES.

Petit Atlas des Mammifères. I. Monotrèmes, Marsupiaux, Édentés, Rongeurs. II. Ongulés. III. Cétacés, Siréniens, Pinnipèdes, Carnivores, Chiroptères. IV. Insectivores, Primates. By PAUL RODE, Dr. ès Sc. [I: pp. 47, with 12 plates; II: pp. 61, with 18 figures and 12 plates; III: pp. 65, with 6 figures and 12 plates; IV: pp. 81, with 12 figures and 12 plates.] (Frs. 90.- each.)

Petit Atlas des Oiseaux. I. Passereaux. II. Passereaux (fin), Rolliers, Pics, Perroquets, Rapaces nocturnes. III. Rapaces diurnes, Pigeons, Gallinacés, Rales, Grues. IV. Bécasseaux, Goélands, Canards, Hérons, Impennes, Ratites. By L. Delapchier. [I: pp. 39, with 16 plates; II: pp. 35, with 12 plates; III: pp. 39, with 12 plates; IV: pp. 63, with 12 plates.] (Frs. 90.– each.)

Petit Atlas des Poissons. I and II. Poissons marins. By Léon Bertin, Dr. ès Sc. **III. Poissons des Eaux douces: Espèces françaises.** By Dr. Jacques Pellegrin. [I: pp 51, with 12 plates; II: pp. 31, with 12 plates; III: pp. 57, with 13 figures and 12 plates.] (Frs. 150.– each.)

Petit Atlas des Amphibiens et Reptiles. I. Apodes, Urodèles, Anoures, Rhynchocéphales, Chéloniens, Crocodiliens. II. Lacertiliens, Ophidiens. By F. Angel. [I: pp. 47, with 12 plates; II: pp. 51, with 12 plates.] (Frs. 90.– each.)

Petit Atlas des Fossiles. I. Fossiles primaires et triasiques. II. Fossiles jurassiques et crétaciques. III. Fossiles tertiaires et quaternaires. By Georges Denizot, Dr. ès Sc. Second edition. [I: pp. 33, with 18 plates; II: pp. 34, with 22 plates; III: pp. 32, with 20 plates.] (Frs. 75.– each.)

Published by Éditions N. Boubée & Cie., 3 Place Saint-André-des-Arts, Paris 6.

The amateur naturalist is chiefly interested by those animals that he can observe alive in their normal environment; the student of zoology is obliged to take the whole animal kingdom as his material, and he must hear and know about many creatures that he has seen neither alive nor in a museum. This little series of booklets would be a valuable personal possession for both naturalist and zoologist, and would fascinate their children. It covers the whole vertebrate kingdom with almost a thousand coloured illustrations, well executed and very well reproduced, representing individual animals of the various types included within each vertebrate Class. To the naturalist they show the variety of forms related to those that are known to him personally; to the zoologist they give an attractive form and substance to beasts that he may know only as skeletons or names.

The illustrations are systematically arranged, and some information is given about each species illustrated. Each volume contains in addition a substantial introductory text giving the characters and zoological classification of the groups and much assorted information on their structure, habits, and other features of interest. The volumes are compact so that the student can carry a little museum in his pocket.

The three volumes dealing with fossils are disappointing. The attempt to treat the whole of the vast and difficult subject of palæontology in the present form could only be a failure.

W. H.

PHYSIOLOGY AND MEDICINE

L'Évolution physiologique. Étude des Pertes de Fonctions chez les Microorganismes. By A. Lwoff. [Pp. 308, with 12 figures and 33 tables.] (Paris: Hermann & Cie, 1944.)

From liberated France comes this excellent survey in the field of microbiological nutrition in support of the author's theory that evolution proceeds by a loss of synthetic function. This idea was first put forward by the author in 1932 in connection with his study on the nutritional requirements of the protozoa and independently by Knight and Fildes in the field of bacterial nutrition. The theory postulates the essential unity in the metabolic requirements of all living creatures. Differences exist in that certain organisms can synthesise "essential metabolites" for themselves, whilst others, even of the same species, have to be given them preformed. This difference in the nutritional requirements of organisms is explained by supposing that originally organisms were able to synthesise all their nutritional requirements from inorganic sources, as indeed many do to this day. This general synthesis of organic material led to localized changes in the environment of some organisms, which then finding themselves surrounded by nutritive substances preformed proceeded to use them in preference to synthesising them. In this process the synthetic activity of the enzymes formerly used for nutritional processes was diminished or lost, resulting in organisms which were stimulated in growth by provision of preformed metabolites or which would not grow without them. The degree of demand for preformed metabolites is, therefore, some index of the extent of evolution from primitive types and is particularly marked among the obligate parasites which have maintained themselves exclusively on the store of nutrients in living creatures. There are two ways in which this evolutionary process may be traced: (1) in the material required for synthesis of protoplasm, and (2) in the energy mechanisms employed by the organism. Lwoff thus divides organisms into nine physiological types ranging from *chemo-autotrophic* which not only use inorganic sources of carbon and nitrogen for synthesis, but also obtain their energy by the oxidation of inorganic compounds, to *allo-metatrophic* which require to be given one or more growth factors and obtain their energy by the oxidation of organic compounds. Any one species of bacteria or protozoa may contain more than one of these physiological types and it is possible experimentally to change one type into another either more or less exacting in its requirements. The Lwoff-Knight hypothesis postulates that a change to a less-exacting type, as occurs in the training of organisms to dispense with a growth factor, is due to the manifestation of a latent enzyme capable of synthesising it originally belonging to a more primitive type, *i.e.* a reversion in the evolutionary process. In some cases no amount of training will enable the organism to develop the enzyme. Here the latent enzyme precursor is lost; a genetic change has taken place. In order to give weight to his hypothesis, the author covers a wide field of investigations in the nutrition of protozoa, bacteria, fungi and higher plants and animals. He marshals his facts well and makes a convincing argument, although his work could with advantage be more condensed. The bibliography contains over 300 references, not all of which, however, are quoted in the text. G. P. GLADSTONE.

A Textbook of Biochemistry. By PHILIP H. MITCHELL. [Pp. xvi + 640, with 84 figures and 79 tables.] (New York and London: McGraw-Hill Book Company, Inc., 1946. 25*s.* net.)

THIS well-produced sturdy volume is clearly intended by the author to teach the young idea to shoot biochemically, for his preface frankly indicates that the book is meant for students, as is indeed conveyed by its title. Consequently it is natural—and right—to find it essentially factual and sternly resisting any temptation to wander up speculative by-paths, however fascinating.

The author has also written a *Textbook of General Physiology*, and one might therefore be forgiven for expecting a certain accent on the "bio" of biochemistry, all the more as Prof. Mitchell himself says, "This book will be found to place emphasis upon nutrition." The somewhat anthropocentric attitude one is led to expect from this remark is more clearly expressed in the next, that "correct nutrition is part of the basis of human welfare."

The chapters, in order of appearance, are on Carbohydrates, Photosynthesis, Fats and Related Substances, Proteins and Amino Acids, Nucleoproteins and Nucleic Acids, The Vitamins, Enzymes, Digestion, Absorption and Detoxication, Blood and Lymph, Chemistry of Respiration and Acid-base Regulation, Biological Oxidation, Calorimetry and Energy Metabolism, Carbohydrate Metabolism, Metabolism of the Lipids, Protein Metabolism, Urine, Composition of Food and Dietetics, The Chemistry of Animal Tissues, Chemistry of the Hormones, Chemotherapy. They are preceded by "An Introduction to Biochemical Literature" with a selected bibliography, containing the names of books and review publications that must be regarded as only suitable for those advanced students who have mastered the contents of this book, and followed by Author and Subject Indexes.

The fact that the book includes references to such recent developments as Astbury's and Huggins's views on protein structure, Frazer's concept of fat absorption and the development of work with tracer isotopes shows that the author is up-to-date in his knowledge of biochemical research not only in his own country but also in this. Sometimes he seems almost too modern, though, in his spelling, for "biotine" is not a normal form (until, anyhow, the substance becomes recognised by the British Pharmacopœia) and "distrophy" is inaccurate in meaning as well as in etymology!

It has not been possible in a fairly extensive random sampling of this volume to find errors either of omission or commission: the standard of accuracy in setting seems as high as is that of the book's general production. To have made available, if at a somewhat less penetrative didactic level, a book that can confidently compete with Bodansky's *Introduction to Physiological Chemistry* in its own country is an achievement on which author and publisher are both to be congratulated.

A. L. BACHARACH.

An Introduction to Biochemistry. By WILLIAM ROBERT FEARON, M.A., Sc.D., M.B. Third edition. [Pp. x + 569.] (London: William Heinemann Medical Books, Ltd., 1946. 21*s*. net.)

BIOCHEMISTRY is a rapidly developing science, and even during the five years of world war, which have intervened between the times of publication of the second and third editions of this text-book, numerous important advances have taken place. Prof. Fearon has managed to incorporate many of these in the new edition. Apart from bringing the book up to date in this way, he has endeavoured to make it more useful, particularly to students of clinical medicine, by amplifying his treatment of such topics as acid-base balance, animal calorimetry, carbon dioxide transport, energy transformation, blood chemistry, bone formation, food absorption and detoxication. A chapter has been added at the end of the book on "Tissue Chemistry"; this gives an account of the chemical constitution of various body tissues including muscle, epithelial and connective tissue, brain, bone and teeth, and is both interesting and useful.

This book is comprehensive within the limits the author has set himself. The medical student will find some information on any biochemical topic he is likely to be interested in, and usually most of what it is important he should know. Indeed, it could be unreservedly recommended were it not for unmistakable evidence that the final revision and proof-reading has not been all that it should. There are unfortunately many errors and misprints; opening the book almost at random we found on successive pages pyridoxine described as 1-methyl-2-hydroxy-3 : 4-dihydroxypyridine, and pantothenic acid as a derivative of *α-γ*-dihydroxy-*ββ*-dimethylpropionic acid; both of which are obviously impossible as systematic names of any organic compound. A careful reading of several chapters brought to light a dozen or so such misleading statements and misprints, and a number of these are not new, but have survived from the second edition. This cannot but leave one uneasy about the general reliability of the wealth of information provided within the pages of this book. It is therefore difficult to recommend it without qualification as a reference or even a text-book. Perhaps, in preparing the next edition, the opportunity will be taken to check and correct all these questionable statements, and to make certain that any new matter introduced is also accurate and free from misprints. The book would then be a most useful as well as a most readable volume. W. O. K.

HISTORY OF SCIENCE

Pasteur. By HENRI MONDOR. [Pp. 189, with 9 plates.] (Paris: Éditions Corrêa & Cie, 1945. Frs. 150.–.)

THIS short biography of Pasteur is not intended as a new study of the great French scientist, but as a tribute from a surgeon on the occasion of the fiftieth anniversary of Pasteur's death. The author reviews the main facts of his subject's life and of his classical researches; he properly stresses the great services that Pasteur rendered to humanity, especially in his influence on operative surgery by the reduction in the appalling mortality figures; he describes the crude and ignorant opposition against which Pasteur stubbornly and bravely contended; and he reveals a deep appreciation and understanding of the personality and outlook of Pasteur. "Le XIX^{e} siècle," writes M. Mondor, "discrédité naguère par snobisme ou par ruse, n'eût-il apporté au monde que la microbiologie, devrait être tenu pour grand."

M. Mondor writes in a clear, easy and concise style and his book is well illustrated with nine beautiful plates. As a short and sympathetic biography of Pasteur, it can be recommended to all students of science and medicine.

D. MCKIE.

Journal of the History of Medicine and Allied Sciences. Vol. I, No. 1, January 1946. [Pp. 182, with numerous illustrations.] (Published quarterly by Henry Schuman, 20 East 70th Street, New York 21, N.Y. Subscription rate $7.50 in the U.S., Canada and Latin America; $8.50 elsewhere. Single copies, $2.50.)

THE appearance of a new journal, and such an admirable one as this, dealing with the history of medicine and the allied sciences is, indeed, welcome, and one hopes that it indicates an increasing interest in the study of the subject. This interest is more widespread in the United States than it is here if one may judge from the limited number of publications devoted to it. "At present there is in the United States only one other publication in this field, the *Bulletin of the History of Medicine*, edited by Dr. Henry E. Sigerist," says Dr. George Rosen, the editor-in-chief, in his introduction to this volume. He adds, "It will not compete with, but will supplement the *Bulletin*", and in this connection it is interesting to note that Dr. Sigerist is one of the consulting editors.

"During the past seventy-five years," adds Dr. Rosen, "a large part of the medical profession has turned away from the history of medicine", and everyone will agree with him that this trend has resulted in a serious loss for the profession. The same may be said, and even more definitely, of the lack of attention which is given to the study of history by statesmen and others. The object of the Journal is "not to cultivate medical history as a mere search for antiquities, as a kind of hunt for curios, but rather as a vital, integral part of medicine." It is a worthy object, and time will show if the optimism of the editor when he states that "We believe this can be done" is justified. Judging from this first number it certainly seems that it will be, and that "while maintaining the highest literary and scholarly standards" it will succeed in presenting "studies that can be of interest to as large a section of the medical profession as possible." A mere list of the contributions to this issue would suffice to show how well this has been attempted as a commencement, and all one's space might be given to a particular article such as Dr. Charles Singer's introductory portion of his "Some Galenic and Animal Sources of Vesalius," a masterly study which is given pride of place. In the course of it Dr. Singer points out that, though Vesalius repeatedly and violently denounced Galen, yet many of his own anatomical descriptions were taken from the text of Galen rather than from the book of Nature. Vesalius he designates as "this noisy, bustling exhibitionist genius," and speaks of "the gratuitous obscurity and studied verbosity of his descriptive writing".

Dr. Josiah C. Trent, in "The London Years of Benjamin Waterhouse," throws interesting sidelights on that remarkable period, the later part of the eighteenth century, and on some of the outstanding medical personalities of the time, especially Dr. John Fothergill, who was his mother's cousin and with whom he lived for a long while, Dr. John Haygarth, Joseph Priestley, Dr Lettsom, William Curtis, author of *Flora Londinensis*, John Hunter, and others.

Of other articles mention may be made of Dr. George Urdang's "Pharmacopœias as Witnesses of World History," the value of which is enhanced by the reproduction of a number of title-pages; of Dr. Saunders' and Dr. O'Malley's study of "Bernardino Montaña de Monserrate", author of the first text on anatomy to be published in the Spanish Vernacular; of Dr. Loren C Mackinney's valuable survey of an aspect of human credulity in his "Animal Substances in Materia Medica: a Study in the Persistence of the Primitive"; of a learned and well-documented article on "The Two Earliest Dentistry Woodcuts," by Curt Proskauer, D.D.S.; of Dr. P. I. Nixon's pleasant and entertaining account of an elusive person "Dr. Benjamin Harrison, Temporary Texan," who is described as "clever, irresponsible, drunken", and of whose adventures one would willingly read more; of Jane M. Oppenheimer's "A Note on William Blake and John Hunter," in which she identifies "Jack Tearguts" in Blake's *An Island in the Moon* as John Hunter and speaks of Blake's darkly brilliant and erratic mind—in other words mentally unstable? Two other articles I was unable to read, not because of lack of interest but on account of the binder's unkindness in leaving out part of them, namely Phyllis Allen's "Medical Education in Seventeenth-Century England," and Dr. Erwin H. Ackerknecht's "Incubator and Taboo." What I have read of these makes me the more regret this unintentional deprivation.

Altogether a fine portion of mixed feeding in medical history and one to which a cordial welcome can heartily be given. May it have the success it deserves.

Hubert G. Norman.

BOOKS RECEIVED

(Publishers are requested to notify prices.)

Analytical Method in Dynamics. By H. O. Newboult, Fellow of Merton College, Oxford. Oxford: at the Clarendon Press, 1946. (Pp. viii + 81, with 16 figures.) 7*s*. 6*d*. net.

Theory of Lie Groups: I. By Claude Chevalley. Princeton: Princeton University Press; London: Oxford University Press, 1946. (Pp. xii + 217.) 20*s*. net.

An Introduction to the Study of Eclipsing Variables. By Zdeněk Kopal, Research Associate, Harvard College Observatory. Harvard Observatory Monograph No. 6. Cambridge, Mass.: Harvard University Press; London: Oxford University Press, 1946. (Pp. x + 220.) 22*s*. 6*d*. net.

Les Radiations. Par Charles Fabry, Membre de l'Institut, Professeur honoraire a la Sorbonne et a l'Ecole polytechnique. Paris: Librairie Armand Colin, 1946. (Pp. iv + 220, with 26 figures.) Frs. 60.–.

Piezoelectricity. An Introduction to the Theory and Applications of Electromechanical Phenomena in Crystals. By Walter Guyton Cady, Ph.D., Sc.D., Professor of Physics, Wesleyan University. New York and London: McGraw Hill Publishing Co., Ltd., 1946. (Pp. xxiv + 806, with frontispiece, 168 figures and 36 tables.) 45*s*. net.

Electrons in Action. By James Stokley. New York and London: McGraw-Hill Publishing Co., Ltd., 1946. (Pp. x + 320, with 55 figures and 36 plates.) 18*s*. net.

The Cathode Ray Oscillograph in Industry. By W. Wilson, D.Sc., B.E., M.I.E.E., M.Amer.I.E.E., M.Brit.I.R.E., Manager, G.E.C. Development Laboratory, Witton. Second edition. London: Chapman & Hall, Ltd., 1946. (Pp. xii + 244, with frontispiece and 194 figures.) 18*s*. net.

Atomic Energy for Military Purposes. The Official Report on the Development of the Atomic Bomb under the Auspices of the United States Government, 1940–1945. By Henry De Wolf Smyth, Chairman, Department of Physics, Princeton University; Consultant, Manhattan District, U.S. Engineers. Princeton: Princeton University Press; London: Oxford University Press, 1946. (Pp. viii + 308, with 8 plates.) 12*s*. 6*d*. net.

A Primer of the Internal Combustion Engine. By H. E. Wimperis, C.B., C.B.E., M.A., Past President of the Royal Aeronautical Society, formerly Director of Scientific Research, Air Ministry. Third edition. London: Constable & Co., Ltd., 1944. (Pp. xii + 141, with 60 figures.) 5*s*. net.

Engineering Research in the University. The Inaugural Lecture to the Chair of Civil and Mechanical Engineering given in the University of London, Queen Mary College. By Professor Edmund Giffen. London: Oxford University Press, 1946. (Pp. 12.) 1*s*. 6*d*. net.

Aqueous Solution and the Phase Diagram. By Frederick Field Purdon and Victor Wallace Slater, B.Sc., F.R.I.C., M.I.Chem.E. London: Edward Arnold & Co., 1946. (Pp. iv + 167, with 47 figures and 35 tables.) 24*s*. net.

Introduction to Emulsions. By George M. Sutheim. Brooklyn: Chemical Publishing Co., Inc., 1946. (Pp. vi + 260, with frontispiece, 22 figures and 25 tables.) $4.75.

Surface Active Agents. Theoretical Aspects and Applications. By C. B. F. Young, Ph.D., Consultant; Director of Research, Clark Babbitt Industries, Inc.; Adjunct Professor, Brooklyn Polytechnic Institute, and K. W. Coons, Ph.D., Head of Department of Chemical Engineering, University of Alabama; Technical Director, National Southern Products Corp. Brooklyn: Chemical Publishing Co., Inc., 1945. (Pp. x + 381, with 41 figures.) $6.00.

The Diffraction of X-rays and Electrons by Free Molecules. By M. H. Pirenne, Dr. Sc. (Liège). The Cambridge Series of Physical Chemistry. Cambridge: at the University Press, 1946. (Pp. xii + 160, with 2 plates and 82 figures.) 12*s*. 6*d*. net.

Metallic Corrosion, Passivity and Protection. By Ulick R. Evans, M.A., Sc.D., Reader in the Science of Metallic Corrosion, Cambridge University. Second edition. London: Edward Arnold & Co., 1946. (Pp. xxxiv + 863, with 144 figures.) 50*s*. net.

An Introduction to Textile Bleaching. By J. T. Marsh, M.Sc., F.R.I.C., F.T.I. London: Chapman & Hall, Ltd., 1946. (Pp. xiv + 512, with 154 figures, including 32 plates.) 32*s*. net.

Bibliography and Index of Geology exclusive of North America. Vol. IX, 1941–42. John M. Nickles, Marie Siegrist, and Eleanor Tatge. Published by the Geological Society of America, 1943. (Pp. xx + 450.)

An Introduction to Botany. By Arthur W. Haupt, Associate Professor of Botany, University of California at Los Angeles. Second edition. New York and London: McGraw-Hill Publishing Co., Ltd., 1946. (Pp. xii + 425, with frontispiece and 289 figures.) 17*s*. 6*d*. net.

The Identification of Conifers. By A. Bruce Jackson, A.L.S., Veitch Memorial Medallist, 1923. London: Edward Arnold & Co., 1946. (Pp. viii + 152, with 48 figures.) 9*s*. net.

Trace Elements in Plants and Animals. By Walter Stiles, M.A., Sc.D., F.L.S., F.R.S., Mason Professor of Botany in the University of Birmingham. Cambridge: at the University Press, 1946. (Pp. xii + 189, with 12 figures and 19 tables.) 12*s*. 6*d*. net.

Notes on Microscopical Technique for Zoologists. By C. F. A. Pantin, Sc.D., F.R.S., Reader in Invertebrate Zoology in the University of Cambridge. Cambridge: at the University Press, 1946. (Pp. viii + 73, with 8 figures.) 6*s*. net.

Some Properties and Applications of D.D.T. Chemical Defence Research Department, Ministry of Supply. London: H.M. Stationery Office, 1946. (Pp. 34.) 6*d*. net.

D.D.T. The Synthetic Insecticide. By T. F. West, M.Sc., Ph.D., F.R.I.C., and G. A. Campbell, M.Sc., F.R.I.C. London: Chapman & Hall, Ltd., 1946. (Pp. xii + 301, with 13 plates.) 21*s*. net.

Biology of Tissue Cells. Essays by Albert Fischer, Head of the Biological Institute, Carlsberg Foundation, Copenhagen. Copenhagen: Gyldendalske Boghandel, Nordisk Forlag; Cambridge: at the University Press; New York: G. E. Stechert & Co., 1946. (Pp. x + 348, with 55 figures, including 21 plates.) 31*s.* 6*d.* net.

Annual Review of Physiology. Vol. VIII. James Murray Luck, Editor, and Victor E. Hall, Associate Editor, Stanford University. Stanford University P.O., California: American Physiological Society and Annual Reviews, Inc., 1946. (Pp. viii + 658.) $5.00.

An Introduction to Biochemistry. By William Robert Fearon, M.A., Sc.D., M.B., Fellow of Trinity College, and Professor of Biochemistry, University of Dublin. Third edition. London: William Heinemann Medical Books, Ltd., 1946. (Pp. x + 569.) 21*s.* net.

A Textbook of Biochemistry. By Philip H. Mitchell, Ph.D., Robert P. Brown, Professor of Biology, Brown University. New York and London: McGraw-Hill Publishing Co., Ltd., 1946. (Pp. xvi + 640, with 84 figures and 79 tables.) 25*s.* net.

Inadequate Diets, Deaths and Diseases and a Food Plan for Madras. By K. G. Sivaswamy. Nutritional Diseases. By Dr. M. P. Chacko. Food Hints. By Dr. S. Gurubatham. Royapettah, Madras: Servants of India Society, 1946. (Pp. 72 + 12.) Rs. 2.–.

Food-Famine and Nutritional Diseases in Travancore (1943–44). Surveys by K. G. Sivaswamy, K. K. Chandy, Lt.-Col. T. S. Shastry, Dr. M. E. Naidu, Dr. T. V. S. Shastry, and 7 other doctors. Servindia Kerala Relief Centre, 1945. (Pp. 264, with 33 illustrations.) Rs. 5.–.

Food Control and Nutrition Surveys, Malabar and S. Kanara. By K. G. Sivaswamy, the late V. R. Nayanar, Dr. R. G. Kakade, and L. N. Rao. Diet and Disease Surveys. By Eleven Doctors. Royapettah, Madras: Servindia Kerala Relief Centre, 1946. (Pp. 225, with 7 illustrations.) Rs. 4.–.

Famine, Rationing and Food Policy in Cochin. By K. G. Sivaswamy. Medical Surveys. By Lt.-Col. T. S. Shastry and Dr. J. A. Bhat. Royapettah, Madras: Servindia Kerala Relief Centre, 1946. (Pp. x + 78 + 35, with 8 plates.) Rs. 3.–.

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Some Chapters in Cambridge Medical History. By Sir Walter Langdon-Brown, Emeritus Professor of Physic in the University of Cambridge. Cambridge: at the University Press, 1946. (Pp. viii + 119.) 6*s.* net.

The Agricultural Development of the Middle East. By B. A. Keen, D.Sc., F.R.S. A Report to the Director General, Middle East Supply Centre, May, 1945. London: H.M. Stationery Office, 1946. (Pp. xii + 126, with 15 plates and 2 maps.) 5*s.* net.

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Rural Education and Welfare in the Middle East. By H. B. Allen, Director of Education, Near East Foundation, New York. A Report to the Director General, Middle East Supply Centre, September 1944. London: H.M. Stationery Office, 1946. (Pp. vi + 24, with 1 map.) 1*s.* 6*d.* net.

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